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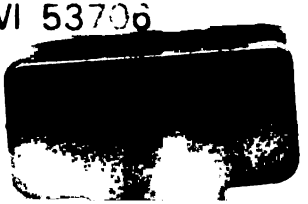

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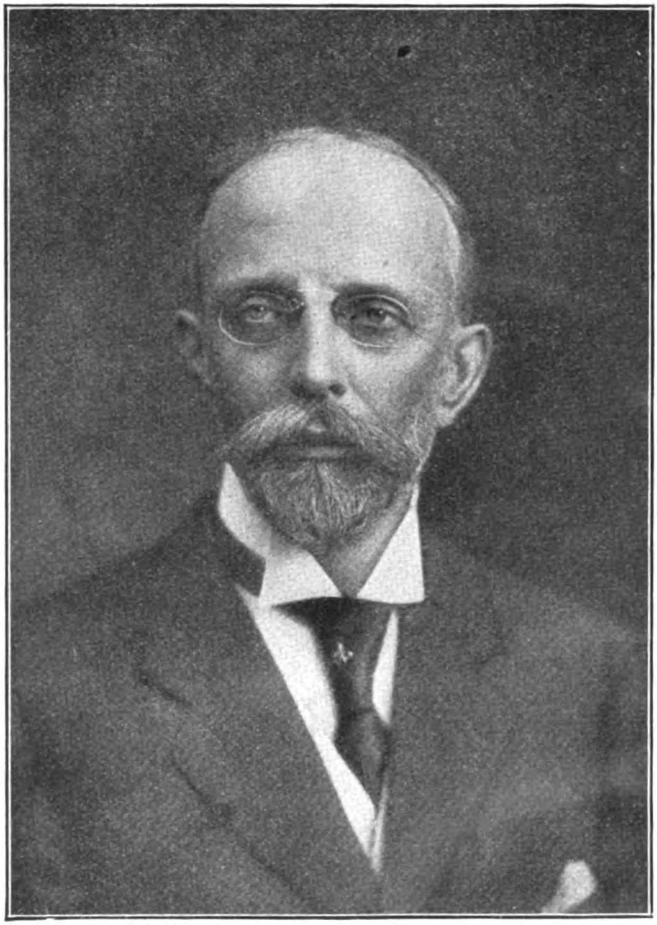

(*Frontispiece.*)

CHARLES KIRCHHOFF.

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TRANSACTIONS

OF THE

AMERICAN INSTITUTE OF MINING ENGINEERS

VOL. LVI

CONTAINING PAPERS AND DISCUSSIONS OF THE ARIZONA MEETING, SEPTEMBER, 1916, AND OF THE NEW YORK MEETING, FEBRUARY, 1917

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PREFACE

This volume contains the papers and discussions of the Arizona meeting that were not included in Vol. LV, and those presented at the New York meeting of February, 1917, with the exception of ten papers. The New York papers omitted from this volume will be published in Vol. LVII.

The papers presented at the two meetings—Arizona, September, 1916, and New York, February, 1917—were printed in the monthly Bulletin during the year beginning with March, 1916, and the papers omitted from this volume appeared in the last four Bulletins of the year. Vols. LV and LVI, therefore, supersede Bulletins 111 to 118, inclusive.

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PROCEEDINGS OF THE ONE HUNDRED AND FOURTEENTH MEETING, NEW YORK CITY, FEBRUARY, 1917

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Luncheon Committee

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ENTERTAINMENT OF LADIES

One of the most gratifying and pleasantest features of the meeting was the presence of approximately one hundred ladies on several occasions, such as luncheon and the other entertainments which had been provided for them, and which included luncheon at the Engineering Society's Building on Monday, Tuesday, and Wednesday, the visiting ladies being met as usual by members of the Ladies Committee in the Ladies Reception Room each day shortly before luncheon. On Monday afternoon, Henry C. Frick, Esq., very courteously opened his art galleries at No. 2 East 71st Street, New York City, where was seen one of the finest private art collections in America, which was enjoyed not only by the ladies, but also by many of the members of the Institute. The visit to these art galleries was followed by tea and exhibition ice skating on the roof of the Waldorf Astoria Hotel.

On Tuesday morning at 10.30 a.m., under the Chairmanship of Mrs. Sidney J. Jennings, an open forum on current events of the day was held. Mrs. Honnold and Mrs. Kellogg, of the Commission for Relief in Belgium, made addresses upon the work of this Commission, and the ladies present thereupon organized a Woman's Auxiliary of the American Institute of Mining Engineers. The aims and purpose of this Auxiliary are outlined in the address of Mrs. Jennings which is given on page xix. In the afternoon a visit was made by the ladies, guests, and members of the Institute to the art galleries of Senator William A. Clark, who not only opened his house for the purpose but also had an organ recital upon his magnificent pipe-organ and served refreshments to the visitors. Senator and Mrs. Clark received the guests.

Wednesday afternoon, following the luncheon, the visiting ladies attended a matinee of "The Yellow Jacket" at the Harris Theater and thereafter visited a motion picture studio of the Famous Players Film Company and saw some pictures taken.

At the Annual Dinner on Tuesday evening, at the exhibition of motion pictures in colors on Wednesday evening, and at the all-day excursion to West Point on Thursday, the presence of the ladies was especially welcome and lent added attractiveness to these occasions.

SOCIAL FEATURES OF THE MEETING

Reunion Evening: On Monday evening, February 19, the entire fifth floor of the Engineering Society's Building was thrown open to the men members and guests of the Institute. Light refreshments were served, as well as souvenir boxes containing cigars and cigarettes. A pamphlet of songs of a popular nature containing the college songs of practically all

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of the universities represented at the meeting was kindly published for the occasion by *Metallurgical and Chemical Engineering*. The meeting opened with the singing of "America." Moving pictures which had been loaned by the French National Committee, showing the interior of munition factories in France, and films showing French artillery in action, were shown. David H. Browne presided and read one of his own humorous and charming stories of Irish child life. Addresses were also made by E. P. Mathewson, Philip N. Moore and Sidney J. Jennings. Mr. Browne then called upon Dr. R. W. Raymond, Secretary Emeritus, who spoke as follows:

"I have been honored by appointment to the pleasant duty of introducing to this company of his professional colleagues Mr. Herbert C. Hoover. Of course, Mr. Hoover needs no introduction. It isn't necessary to look into "Who's Who" in order to find out who's Hoover! If you should consult that admirable volume, you would find an outline of what we well-informed old fellows knew about him a couple of years ago. We remembered him as a promising graduate of the Stanford University, a pupil of the distinguished Prof. J. C. Branner; then as an active and intelligent field-worker on State and Federal geological surveys; then as a mine manager on the Pacific slope; then as the directing or advising engineer of great corporations, exploring and mining on all the continents, and conducting that "big business" which both requires and breeds big men. And we remembered him also as the author of masterly books on the principles and economics of mining and of a wonderful translation of Agricola's *De Re Metallica*, with footnotes which constitute a cyclopedia in themselves. But all this, that a few of us knew about him, is now swallowed up in the things that everybody knows!

"By special agreement with Mr. Hoover, neither he nor I will utilize this festive occasion by recommending the enterprise of international philanthropy in which he now stands at the head. It does not seem appropriate to attempt, here and now, to touch either your hearts or your pockets in the name of such a cause, however holy. You have responded elsewhere already to that appeal. Besides all other contributions previously made through other channels, the whimsical corporation of "The Belgian Kiddies," very recently organized, has made a good beginning by receiving from the engineers of this country and forwarding to Mr. Hoover's Commission the sum of $75,000. But, I repeat, we are not going to consider that subject to-night. We are here to greet a representative American mining engineer, and to acknowledge the glory which in that capacity he has shed upon us all.

"Let me, however, return for a moment to Hoover's translation of Agricola. That book was written in the sixteenth century in Latin—probably the worst Latin of history; for the author coined words of his own for the tools, machines, processes and officials for which classic literature contained no names. The German version, published a few years later, was made by a physician who knew nothing about mining, and is a mass of absurd misunderstandings. So old Agricola, one of the greatest champions of science in the battle against superstition, was deprived of his full fame by being buried in his own bad language! He could be exhumed and exhibited in his true proportions only by the skillful hands of some one who knew all kinds of Latin, medieval German and ancient mining and metallurgy besides. Let me say frankly that the more I study this admirable translation, the more I am inclined to give the chief credit

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to Mrs. Hoover, whose name, as well as that of her husband, stands upon the title-page!

"But Hoover had studied Agricola to some purpose. On the 93d page of that book, the old expert, writing four centuries before the passage of the eight-hour law, describes the division of mining labor, for purposes of continuous efficiency, into three daily shifts. And when in August, 1914, a hundred thousand American refugees, caught in the sudden catastrophe of war, invaded London as a frantic multitude, thirty American mining engineers, who happened to be residing in that city, organized themselves into a committee with Mr. Hoover as chairman, and, as the first step in systematic operation, constituted three shifts of eight hours each, thus becoming at once a body which could not be tired out, because two-thirds of it was always resting while the other third worked! So while government officials threw up their hands in despair, and well-meaning enthusiasts wore themselves to a frazzle in protracted over-exertion—"pouring themselves out" as a useless libation, instead of containing themselves for steady and tireless effort, this committee in three shifts went steadily on, giving to the world an object-lesson of knowing how and doing well —of the scientific conservation of human energy; of efficiency compared with—well, let us say, *shiftlessness!*

"For these American mining engineers were trained in big business. They knew how to handle both men and materials on the great scale; how to work together; how to keep cool; how to meet emergencies; how to climb over, or tunnel under, or go around obstacles.

"And we of the Institute know what kind of wives American mining engineers are apt to have—companions in enterprise and danger, equally fearless in camp or cabin—oh! you know the kind: you have one yourself! —and they were there too, making the splendid exhibition complete! We always believed we—we pairs, I mean—were the finest double blossoms developed by modern civilization; and now we are sure of it.

"I have promised not to speak of the later and wider work which naturally sought the same hands. But I cannot pass it by in utter slience; for above and through all our shouts and plaudits and self-congratulations over this great achievement of patriotic ardor expressed in scientific method and skill—above it all, and "smiting a silence" through it all, I hear a Voice, saying, "Ye have done it unto the least of these!"

"So I end as I began. You cannot introduce Hoover. You can only precede him—and you must not do that too long. Knowing that Hoover was coming after me, I have felt like the Irishman who was running wildly through a railway-cutting, pursued by a fast train, the engineer of which succeeded in stopping it just as he was about to be run over. When they said, 'Pat, why didn't you get off the track and up on the embankment?' he said 'Faith, I could only just kape ahead on a level; d'ye think I'd try it *up hill?*'

"Well, gentlemen, there is only one way to introduce Hoover. All of you get up, and lift high your glasses, and say with a will:

"Here's—Hoover."

(Which they did.)

Mr. Hoover, rising to reply, received an ovation from his fellow-members which, for a considerable period, prevented him from speaking. When he could be heard, he modestly disclaimed the praise bestowed upon him personally, declaring that it was due to the splendid team-work of the thirty American engineers who formed the backbone of the Relief

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Administration and whose possession of the high professional ideals and the coöperative spirit fostered by the American Institute of Mining Engineers under the guidance, for many years, of Dr. Raymond, had made the work possible.

Mr. Hoover described graphically the nature of the Committee's labors, and narrated several incidents, humorous and pathetic, connected therewith.

Finally, in allusion to the later and present work of the Belgian Relief Commission, he said it was planting the American flag, not over conquered territory or public buildings or fortifications, but in ten million grateful hearts.

After Mr. Hoover had finished, the Chairman announced that any speech that might follow would be in the nature of an anti-climax and the meeting then adjourned after again singing a verse of "America."

Annual Dinner.—The Annual Dinner was held at the Hotel Astor and was preceded by a reception at 7:00 p.m., which was given to President and Mrs. L. D. Ricketts and Mr. and Mrs. Herbert C. Hoover. After dinner short addresses were made by President L. D. Ricketts and President-elect Philip N. Moore. Following these addresses, President Ricketts announced that the previous Board of Directors of the Institute, as their last official act; had unanimously elected Herbert C. Hoover an Honorary Member of the Institute. A tremendous ovation was given to Mr. Hoover, who spoke briefly of the work of his associates on the Commission for Relief in Belgium. The dinner was followed by dancing which lasted until well after midnight.

Luncheons.—On Monday, Tuesday, and Wednesday, luncheon was served in buffet fashion on the fifth floor of the Engineering Society's Building. The luncheons were largely attended by both members and ladies. On Wednesday several members of the Naval Consulting Board and the Army and Navy Departments of the United States were guests of the Institute at luncheon.

Moving Pictures in Colors and Lecture Thereon.—On Wednesday evening, Dr. Herbert T. Kalmus gave a lecture illustrated by experiments and moving pictures in colors, showing the fundamental principles of the process. This was given in the auditorium of the Engineering Society's Building.

Excursion to West Point.—Notwithstanding that snow fell on Wednesday night and was falling on Thursday morning, 259 persons, of whom 81 were ladies, assembled at the ferry of the West Shore Railroad before 9:00 a.m. to take the special train. A Pullman car had been provided for the convenience of the ladies and the sun appeared before the special train emerged from the tunnel onto the western bank of the Hudson, and thereafter the weather was all that could be desired. Upon arrival at West Point the whole party went to Cullum Memorial Hall, the ladies being conveyed in busses. From there the party journeyed to the Riding Academy, where a most interesting exhibition of mounted drill was given by the cadets. This was followed by luncheon at Cullum Memorial Hall. Twenty members of the West Point Military Academy staff, with members of their family, were the guests of the Institute at luncheon and afterward dancing was enjoyed by many of the members and guests. At 2 o'clock all assembled in the auditorium, where an informal session was held at which Major Arthur S. Dwight, Chairman of the Institute's committee in charge of this excursion and Chairman of the Committee

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on Reserve Corps of Civilian Engineers, presided. After a few appropriate complimentary and congratulatory remarks, Major Dwight introduced Col. John Biddle, Superintendent of the Military Academy, who welcomed the visitors to West Point, expressing his pleasure in the fortunate change of the weather which permitted them to see the place in the height of its winter beauty. As the most suitable entertainment which he could offer in his address, Col. Biddle proceeded to give a brief but exceedingly interesting summary of the history of the Military Academy, emphasizing the connection of Washington with its establishment and location, and concluding with an instructive statement of its purpose as a school of general preliminary training for officers, who receive further special instruction in other Government institutions, culminating in the War College at Washington.

Mr. Sidney Jennings, Vice President of the Institute, being called upon to reply, introduced, as the spokesman on this occasion, Dr. Rossiter W. Raymond, Secretary Emeritus, whose family had been for many years connected with the Military Academy, especially through his brother, the late Brig. Gen. Charles W. Raymond, of the Corps of Engineers, who spent many years at West Point as instructor; who, as the engineer of the post, erected several of the important buildings, such as the hospital, the cadet barracks, etc.; and who, besides his work for 40 years as a military engineer, was famous as explorer of the Yukon, constructor of the harbor of Philadelphia, designer and builder of the Delaware breakwater, and finally Chairman of the Board of Engineers which planned and directed the great enterprise of the Pennsylvania R. R. Co., with its tunnels, terminals, yards and shops, in New Jersey, Manhattan and Long Island.

Dr. Raymond spoke as follows:

"*Mr. Chairman, Fellow-Engineers, Ladies and Gentlemen:* I accept with frank pleasure the words just spoken concerning my brother. He was indeed a great civil as well as an accomplished military engineer; and he illustrated and exemplified, in his long career of forty years of successful achievements without a single failure, that fraternal coöperation between the American engineers in civil life and the Engineer corps of the Army, the recent development of which has inspired us all with patriotic pride. My own distinct recollections of West Point date from the days of the War for the Union, when, as staff officer, I visited the Academy, and took great delight in being respectfully saluted by my cadet brother. In later years, after my brief military career was over, and during his unusually long residence here, I used to visit him, and to maintain a most agreeable friendship with many of his fellow-officers on the Corps of Engineers. I have spent a solemn half-hour to-day, before this session began, in wandering about this splendid memorial hall, and realizing, as I gazed upon these portraits and tablets, that my Army friends and contemporaries have departed, and that 'glory guards their graves.' But I have been comforted by meeting here, not only those who recall my generation, but also the comrades of the Raymonds of a later date. Out yonder, in the West Point cemetery—the fairest spot on earth—beside my brother's grave, is the grave of his son, Captain Jack Raymond of the 10th cavalry—oh! they all loved Jack!—and there are two of Jack's brothers in active service still. So I may fairly feel that the relations of more than half a century between my family and this historic Academy are still subsisting unimpaired. You will pardon, I am sure, this outbreak of personal feeling, which could not be suppressed in such a presence and at such an hour.

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"We mining engineers have been accustomed to claim that mining engineering comprises the operations of all other branches of engineering, together with peculiar processes and difficulties of its own. But perhaps we must now yield that proud comprehensive pre-eminence to the military engineers. It is they who have to know and to employ in modern warfare all arts and sciences. That even the highly abstruse and abstract science of astronomy may be a weapon of conquest, let me prove to you by a little story.

"In 1869, a military expedition was sent by the United States from the mouth of the Yukon river a thousand miles or more, to the British post of Fort Yukon. The expedition was commanded by Captain Charles W. Raymond, of the Corps of Engineers, U. S. A., whose military force consisted of Michael Foley, private, of the 9th U. S. Infantry. The garrison of Fort Yukon, on the other hand, consisted of two men in the service of the Hudson's Bay Company.

"The American force established an encampment (one tent) on the esplanade—that is to say, in the front yard of the fort, where it mounted its heavy artillery—a zenith telescope. Bombarding the sky with this battery, it obtained on the 7th day of August, 1869, during an eclipse of the sun, the data from which the longitude of the post could be determined; and on the next day Capt. Raymond informed the British garrison that Fort Yukon was many miles west of the 141st meridian, which constitutes the eastern boundary of Alaska—in other words, that it was upon American soil—and called upon them to haul down the British flag and substitute the stars and stripes. This they declined to do; but, the two armies being drawn up on opposing lines, the garrison maintained an attitude of innocuous desuetude and watchful waiting, while Private Foley, 9th Infantry, U. S. A., by a bold and skillful sortie, effected the desired change of national colors; after which the British forces shook hands, and amicably retired, to build a new fort on the other side of the international boundary. This was the first instance in history of the capture of an important fortified position by one man with a telescope! It was a prophetic illustration of the decisive importance of artillery! And what a significant indication did it present of the direction in which the millenium is to be reached in warfare by the progressive reduction of armaments! But we must bear in mind that as quantity is diminished, quality must be improved. The one gun which reduced Fort Yukon, nearly half a century ago, had an effective range of some 90,000,000 miles! (Laughter and applause.)

"Well, science having thus entered the sphere of war in these latter days, it is but natural that war should make its appeal to the men of science. Fifty years ago, there was some controversy between the civilian and the military engineers of the United States. *We* thought that *they* ought not to have the charge of great public improvements, like those of rivers and harbors and canals. And it was perhaps an aggravation of our dissatisfaction that they performed their work so well. But that controversy has settled itself, like all controversies, religious, political, or other, since the world began—not by the victory of either party, but through the advance of both parties to higher ground, where old differences are forgotten or disregarded or dismissed as trivial in the presence of a new danger, duty and devotion, common to all. Yes, the military engineers have swallowed us, and we do not disagree with them. Indeed, we are proud and happy in our new fellowship. Look at Arthur

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Dwight, who presides at this moment upon this platform. Do you think he cares for those academic distinctions which, a few months ago, he wore with such dignity and satisfaction? He's a *Major* now. And look at me! When I receive annually from the Recorder of the Military Order of the Loyal Legion of the United States a letter asking me to pay my dues, it thrills me with joy. For it is addressed to me as Captain, without any mention of titles in philosophy or jurisprudence; and it renews my youth and strength to have my alphabetical appendix thus removed!

"Yes, all American engineers are simply patriots now. These are tense and critical hours. Since we listened, a few minutes ago, to the guns which saluted the memory of Washington upon this, his natal day, our ears have been strained to catch the muttered message from a dark horizon, and we know not how soon the storm may break, even upon the beauty and the white-robed peace that surround us here.

"One of the earliest and greatest poets of our country, Drake, chose this region as the scene of an exquisite poetic narrative from which he carefully omitted all human passions or purposes. From its serene opening lines:

> 'The moon shines down on old Cronest;
> She mellows the shades on his shaggy breast,
> And seems his huge gray form to throw
> In a silver cone on the wave below.'

to the last words—'the fays are gone,' the scenes and images of the poem are those of fairy-land. Yet the same pen wrote in the same environment that immortal lyric,

> 'When Freedom from her mountain height
> Unfurled her standard to the air,
> She tore the azure robe of night
> And set the stars of glory there!——'

I will not recite it further. You know it. We used to speak it in school —and it is a good piece to speak to-day!

"As you recall it, let it be associated in your minds henceforth with the lovely and the lofty memories of this place and this day. Imagine that upon those heights of Storm King and Cronest was that 'mansion in the sun' whence Freedom 'called her eagle bearer down, and gave into his mighty hand the symbol of her chosen land!'

"And, as we shortly leave this charming place and these its gallant, friendly guardians, our hearts will breathe a friendly benediction upon the scenic beauty, the historic glory and the heroic sons of West Point!"

Major Dwight then introduced Capt. Stuart C. Godfrey, Corps of Engineers, U. S. Army, who, after a graceful acknowledgement of Dr. Raymond's tribute to West Point, and a reference to the mutual courtesies of the occasion, which had made his colleagues and himself both hosts and guests, gave an interesting talk upon the methods and weapons of modern warfare, illustrated with lantern views from the present European war.

Mr. Philip N. Moore, President-elect of the Institute, briefly expressed, for himself and other visiting members from the West, a high

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appreciation of the entertainment which they had received throughout this annual meeting, and the hope that many of the Eastern members would attend the St. Louis meeting, next summer, thus giving opportunity for some return of this courtesy. In closing, he introduced Mrs. Moore, who felicitously extended a similar acknowledgment and invitation to the New York Ladies Committee, and to the Eastern ladies generally.

The excursionists then spent the remaining time most agreeably in inspecting buildings, classrooms and points of interest, including the beautiful cemetery, the new chapel (where an organ concert delighted the music-lovers) the gymnasium (where there was a display of calisthenics and fencing) and Cullum Memorial Hall, with its impressive great auditorium, the walls of which are hung with portraits of the West Point graduates who have won distinction in the wars of the Union—many of them having fallen in battle. At 4:30 p.m. the special train bore the visitors back to New York where it arrived at about 6:15. Thus ended the largest meeting of the Institute which had ever been held, except where some sister Institution had joined forces with it. In size it was matched by interest and social pleasure, ranking in these two particulars as among the best ever held by the Institute. The number of members and guests who registered was 630, but it is known that many were present without registering.

Technical Sessions

Monday Morning, Feb. 19, 1917.—The opening session, like all the technical sessions, was held at the headquarters of the Institute in the Engineering Societies' Building, New York City. It was on Geology and was presided over by William Kelly.

The following papers were presented by their authors or authors' representatives:

Geology of the Iron-Ore Deposits of the Firmeza District, Oriente Province, Cuba. By Max Roesler. (Discussed by William Kelly, J. T. Singewald, Jr., Benj. L. Miller, John D. Irving, L. C. Graton, C. P. Berkey. Written discussion by W. L. Cumings.)

The Manganese Ores of the Lafayette District, Minas Geraes, Brazil. By Joseph T. Singewald, Jr., and Benjamin Leroy Miller. (Discussed by L. C. Graton. Written discussion by F. L. Garrison.)

The following papers were presented by title:

Recent Geologic Developments on the Mesabi Iron Range, Minnesota. By J. G. Wolff. (Written discussions by Carl Zapffe, E. J. Collins, J. F. Wolff.)

The Geology of the Bawdwin Mines, Burma, Asia. By M. H. Loveman.

Geology and Ore Deposits of Mohave County, Arizona. By Frank C. Schrader. (Written discussions by J. C. Anderson, J. D. Sperr, J. B. Platts.)

Manganese Ores of Russia, India, Brazil and Chile. By E. C. Harder. (Discussed by J. T. Singewald, Jr. Written discussion by H. K. Scott.)

Monday Morning, Feb. 19, 1917.—Session on Metallography, H. M. Boylston presiding.

The following papers were presented by their authors or authors' representatives:

Recrystallization after Plastic Deformation. By Henry M. Howe.

Grain Growth Phenomena in Metals. By Zay Jeffries.

On Grain Growth. By Henry M. Howe. (The above papers were discussed by W. E. Ruder, F. E. Carter, J. W. Richards, H. M. Boylston. Written discussion by J. A. Mathews.)

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Tungsten and Molybdenum Equilibrium Diagram, and System of Crystallization. By Zay Jeffries. (Written discussion by A. G. Worthing.)

The System Tungsten-Molybdenum. By Frank Alfred Fahrenwald. (The above papers were discussed by J. W. Richards, H. M. Boylston, Zay Jeffries.)

Monday Afternoon, Feb. 19, 1917.—This session was under the auspices of the Committee on Petroleum and Gas, Mark L. Requa presiding.

The following papers were presented by their authors or authors' representatives:

Reservoir Gas and Oil in the Vicinity of Cleveland, Ohio. By Frank R. Van Horn. (Discussed by David T. Day.)

Evidence of the Oklahoma Oil Fields on the Anticlinal Theory. By Dorsey Hager. (Discussed by L. L. Hutchison, M. L. Requa, I. N. Knapp, C. Naramore, Dorsey Hager, F. T. Hirschberg, R. H. Johnson, M. M. Thompson. Written discussion by H. A. Wheeler.)

The Possibility of Deep Sand Oil and Gas in the Appalachian Geo-Syncline of West Virginia. By David B. Reger. (Discussed by M. M. Thompson, L. L. Hutchison.)

The Need and Advantages of a National Bureau of Well Log Statistics. By W. G. Matteson. (Discussed by I. N. Knapp, B. L. Miller, Dorsey Hager, David B. Reger, R. H. Johnson, S. A. Taylor, M. L. Requa, C. Naramore, L. L. Hutchison.)

The following papers were presented by title:

Problems Connected with the Recovery of Petroleum from Unconsolidated Sands. By William H. Kobbé. (Discussed by I. N. Knapp, M. L. Requa, C. Naramore, Dorsey Hager. Written discussions by Arthur Knapp, I. N. Knapp.)

The Influence of the Movement in Shales on the Area of Oil Production. By Richard A. Conkling. (Discussed by Dorsey Hager. Written discussion by D. W. Ohern.)

Monday Afternoon, Feb. 19, 1917.—Session on Milling and Smelting, Sidney J. Jennings presiding.

The following papers were presented by their authors or authors' representatives:

Magnetic Concentration of Low-Grade Magnetic Iron Ore. By S. Norton and S. LeFevre. (Discussed by J. L. W. Birkinbine. Written discussion by G. C. Foote, F. L. Nason.)

*An Investigation on Rock Crushing Made at McGill University. By John W. Bell. (Discussed by R. B. T. Kiliani. Written discussion by C. W. Merrill.)

*Countercurrent Decantation. By Luther B. Eames. (Discussed by J. V. N. Dorr.)

The Viscosity of Blast-Furnace Slag. By Alexander L. Feild. (Discussed by R. H. Richards, J. W. Richards, H. A. Guess, A. L. Feild. Written discussion by W. McA. Johnson, Geo. K. Burgess, A. W. Fahrenwald, R. B. Sosman, G. A. Rankin, J. E. Johnson, Jr.)

Matte Granulation at Herculaneum, Mo. By S. Paul Lindau and Henry B. Smith.

The following paper was presented by title:

The Function of Alumina in Slags. By Carl Henrich. (Discussed by A. S. Dwight, E. P. Mathewson, W. B. Boggs, J. W. Richards, W. C. Smith.)

Tuesday Morning, Feb. 20, 1917.—Annual Meeting, Professor J. W. Richards presiding.

Sixty-two members were present in person and 2276 voted by letter ballot.

President Ricketts was present and occupied the Chair, but on account of weakness in his voice, at his request, Vice-President Joseph W. Richards presided.

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The minutes of the Annual Meeting held on Feb. 15, 1916, were read, and on motion, duly made and seconded, were approved.

Reports of the President, Secretary, and Treasurer were presented in writing.

Report of the Tellers for Election of Officers and Directors was presented in writing and the following Officers and Directors were declared elected:

Director and President,	Philip N. Moore,
Director and Vice-President,	Charles W. Goodale,
Director and Vice-President,	Mark L. Requa,
Director,	Robert M. Raymond,
Director,	Willet G. Miller,
Director,	Allen H. Rogers,
Director,	Howard N. Eavenson,
Director,	J. E. Johnson, Jr.

The report of the Tellers on Simplified Spelling was presented in writing and showed the following:

For	Against
Paragraph 1–919	Paragraph 1–290
Paragraph 2–765	Paragraph 2–429
Paragraph 3–659	Paragraph 3–540

The number of ballots returned unmarked leaving the matter of spelling for discussion by the Board of Directors amounted to 346.

The reports of the following committees were presented:

Library Committee;

Committee on Papers and Publications;

Committee on Membership.

The report of the J. A. Holmes Safety Association was presented in writing.

Tuesday Morning, Feb. 20, 1917.—Session on Non-Metallic Minerals, Heinrich Ries presiding.

The following papers were presented by their authors or authors' representatives:

* A Study of the Silica Refractories. By J. Spotts McDowell. (Discussed by J. W. Richards, W. F. Rochow, H. D. Hibbard, J. B. Umpleby.)

* The Conservation of Phosphate Rock in the United States. By W. C. Phalen. (Discussed by E. G. Spilsbury.)

The following paper was presented by title:

* The Genesis of Asbestos and Asbestiform Minerals. By Stephen Taber. (Written discussion by J. C. Branner, J. A. Dresser, R. P. D. Graham, G. P. Merrill.)

Tuesday Afternoon, Feb. 20, 1917.—Session on Iron Blast Furnace Practice, Joseph W. Richards presiding.

The following papers were presented by their authors or authors' representatives:

Dry-Hot versus Cold-Wet Blast-Furnace Gas. By Linn Bradley, H. D. Egbert and W. W. Strong. (Discussed by J. W. Richards, C. P. Perin. Written discussion by R. J. Wysor, K. Huessener, F. H. Willcox.)

Some Suggestions Regarding Construction of Hot Blast Stoves. By Linn Bradley, H. D. Egbert and W. W. Strong. (Discussed by F. G. Breyer, L. E. Riddle, C. P. Perin.)

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Potash as a Byproduct from the Blast Furnace. By R. J. Wysor. (Discussed by J. S. Unger, L. E. Riddle, F. G. Breyer, H. A. Huston, W. H. Ross, G. A. Roush. Written discussion by C. H. Rich, J. S. Unger, W. A. Schmidt.)

The following paper was presented by title:

Calculations with Reference to the Use of Carbon in Modern American Blast Furnaces. By Henry Phelps Howland. (Discussed by J. W. Richards. Written discussion by A. H. Lee, N. M. Langdon, W. H. Blauvelt, J. W. Richards.)

Tuesday Afternoon, Feb. 20, 1917.—Session on Flotation, Edward P. Mathewson presiding.

The following paper was presented by the author's representative:

Notes on Flotation—1916. By J. M. Callow. (Discussed by E. E. Free, J. W. Bell, H. W. DuBois, G. D. Van Arsdale, J. V. N. Dorr, E. P. Mathewson, H. A. Megraw. Written discussion by Rudolf Gahl, E. R. Ramsey.)

Wednesday Morning, Feb. 21, 1917.—Session on the Manufacture of Iron and Steel, Henry D. Hibbard presiding.

The following papers were presented by their authors or authors' representatives:

The Seasoning of Castings. By Richard Moldenke. (Discussed by H. D. Hibbard, A. E. Outerbridge, Jr., Leonard Waldo, J. S. Unger, M. H. Medwedeff. Written discussion by A. E. Outerbridge, Jr.)

Roll Scale as a Factor in the Bessemer Process. By A. Patton and F. N. Speller. (Discussed by Henry D. Hibbard, C. S. Robinson, J. W. Richards, A. Patton. Written discussion by E. T. McCleary, H. H. Campbell, M. R. Stevenson.)

Temperature Measurements in Bessemer and Open-Hearth Practice. By George K. Burgess. (Discussed by J. W. Richards. Written discussion by R. C. Drinker.)

The Manufacture of Weldless Steel Tires for Locomotive and Car Wheels. By Guilliaem Aertsen. (Discussed by Henry D. Hibbard, Bradley Stoughton.)

The following paper was presented by title:

Significance of Manganese in American Steel Metallurgy. By F. H. Willcox. (Discussed by J. W. Richards, Albert Sauveur, Richard Moldenke, D. F. Hewett, A. E. Outerbridge, Jr., Leonard Waldo, H. D. Hibbard. Written discussion by J. S. Unger.)

Wednesday Afternoon, Feb. 21, 1917.—Session on Metallography and Heat-Treatment of Steel, Albert Sauveur presiding.

The following papers were presented by their authors or authors' representatives:

Erosion of Guns—The Hardening of the Surface. By Henry Fay. (Discussed by Hudson Maxim, Bradley Stoughton, Leonard Waldo, H. C. Wilson, J. W. Richards, A. L. Walker. Written discussion by Albert Sauveur, Ralph Earle, Lawrence Addicks.)

Notes on the Heat Treatment of High-Speed Steel Tools. By A. E. Bellis and T. W. Hardy. (Discussed by C. G. Fink, J. A. Mathews, H. M. Boylston, A. Sauveur. Written discussion by M. H. Medwedeff, Robert J. Anderson, F. C. Langenburg.)

Effect of Time in Reheating Hardened Steel Below the Critical Range. By C. R. Hayward and S. S. Raymond. (Discussed by A. Sauveur, J. A. Mathews, Bradley Stoughton, H. M. Boylston, Wm. Campbell, M. H. Medwedeff.)

The Effect of Sulphur on Low-Carbon Steel. By Carle R. Hayward. (Discussed by A. Sauveur, M. H. Medwedeff, J. S. Unger, G. Aertsen, Leonard Waldo. Written discussion by G. F. Comstock.)

A Method for Distinguishing Sulphides from Oxides in the Metallography of Steel. By George F. Comstock. (Discussed by H. M. Boylston, G. F. Comstock.)

Wednesday Afternoon, Feb. 21, 1917.—Session on Mining Methods, etc., R. M. Catlin presiding.

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*Anthracite Stripping. By J. B. Warriner. (Discussed by S. A. Taylor, R. V, Norris, T. M. Dodson, Geo. S. Rice, Harrison Souder, H. M. Chance, Robert Peele. R. M. Catlin, H. M. Crankshaw, A. O. Ihlseng, W. S. Ayres, D. B. Reger, B. F. Tillson, E. M. Chance.)

*Portable Miners' Lamps. By Edwin M. Chance. (Discussed by G. S. Rice. Written discussion by Herbert M. Wilson, M. D. Cooper, Robert P. Burrows, H. H. Clark.)

The following papers were presented by title:

*The Pennsylvania Mine Fire, Butte, Mont. By C. E. Nighman and R. S. Foster.

*Shot-Firing in Bituminous Mines. By M. D. Cooper. (Discussed by B. L. Tillson, Lucien Eaton, T. M. Chance, H. M. Crankshaw, R. M. Catlin, E. T. Lednum. Written discussion by J. J. Rutledge, Geo. S. Rice, E. M. Chance, D. Harrington, A. La Motte.)

*Report of the Secretary of the Committee on Safety and Sanitation. (Discussed by B. F. Tillson, E. M. Shipp, H. N. Eavenson, Wallace McKeehan.)

WOMEN'S AUXILIARY OF THE AMERICAN INSTITUTE OF MINING ENGINEERS

As already announced in the notice of the 114th (New York) Meeting of the Institute, the ladies present met and organized the Women's Auxiliary of the Institute. All ladies of the families of members of the Institute are eligible to this Auxiliary. The purpose and objects of the Auxiliary are well stated in the address of the President, Mrs. S. J. Jennings, in opening the organization meeting on the morning of Feb. 20, 1917. The address follows:

"Ladies: Friends from far and near, on behalf of the Women's Entertainment Committee of the A. I. M. E., I extend to you a hearty welcome.

"Most especially are we glad to see our friends from the Pacific Coast and the middle West, whose royal hospitality to the members of the A. I. M. E. and their wives and daughters on their trips to California and to the Copper Country of the Southwest can never be forgotten. We are glad to be able in some small measure to return it.

"This interchange of visits has given to us the idea that it would be pleasant and profitable for the women of the families of the engineers to have some closer bond of union, such as is now enjoyed by the men, and that we might form a Women's Auxiliary to the A. I. M. E., whose members might jointly and individually interest themselves in work for their country or community or for humanity at large.

"That the engineer is the pioneer of civilization is a platitude we are accustomed to hear frequently aired, and we women are rather apt to consider complacently what fine men we have for husbands and what splendid work they have done, and we are not at all blind to the fact that we have been contributors to their success, if not by helping them with mathematical calculations, like one charming wife I know, or like another forceful, delightful woman who put in a pump in her husband's absence, in another way by doing our ordinary woman's work of making home pleasant, providing digestible food, and keeping as far as possible the worries of life from the man who has problems enough in his work to fill two ordinary lives.

"Year by year we grow more keenly alive to the fact that as the art of the engineer draws the world closer together and brings far distant regions within our ken; that as the great ships, the outcome of the mechanical

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engineer's brain, carry the minerals the mining engineer has wrested from the earth, from continent to continent; that as the skill of the engineers of all kinds is brought into play in the terrible war for freedom and justice, in the throes of which Europe struggles bleeding, so do our responsibilities to humanity increase. We can no longer indulge in the comfortable selfishness of confining our energies to our homes. We have time not fully employed there and humanity calls on woman for help and sympathy in its troubles and woes, for fellow feeling in its joys, and for mutual aid in getting the very best from life. Humanity's cry is insistent and it calls on us to work and to help, to give of our time, our capacity, our thought, our talent, our wealth. Are we to turn a deaf ear and say we have enough to occupy us in our homes? It is a strange anomaly that it is always the busiest people who have the most time. I believe that we are most of us, if not all, giving of ourselves for the world at large, but you remember the old fable that carries an eternal truth: An old man on his death bed called his sons around him and gave them a bundle of sticks tied with a band and told them to break the bundle. The sticks were stout and strong and not one of the sons had strength enough to break the fagot. "Now my sons," said the father, "loosen the band and break the sticks one by one." It was easily done. "See," said the old man, "United you stand strong, singly you break and fall." It is this truth we have in mind when we put before you a scheme to form a Women's Auxiliary to the A. I. M. E., a society in which we can all work together for our country, for our community, or for any section of humanity.

"If it is your pleasure to sanction any such organization, it would seem a good thing that at these yearly meetings we should settle on some particular work that we can do, preferably in collaboration with the A. I. M. E., then that each member returning to her own home should form a center and enlist other women in her district to help. It would probably be wise to have on the governing council six or seven directors from various parts of the country to represent the views of different sections.

"Our work, our help is needed. Can we not work more efficiently together as one band, than as separate units? It may be that our country will need us soon—humanity needs us now. May we make it our proud boast that the women of the A. I. M. E., are ready at the first call?"

Notice will shortly be sent to the members of the Institute asking for information regarding the ladies of their families. The membership dues of the Auxiliary are $1 per year, which should be sent to the Treasurer. The officers are as follows:

President, Mrs. S. J. Jennings,
First Vice-President, Mrs. Arthur S. Dwight,
Second Vice-President, Mrs. Henry S. Monroe,
Third Vice-President, Mrs. H. W. Hardinge,
Treasurer, Mrs. George D. Barron,
Corresponding Secretary, Mrs. Axel O. Ihlseng,
Recording Secretary, Mrs. Bradley Stoughton.

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REPORT OF THE SECRETARY FOR THE YEAR 1916

I have the honor to submit herewith the report of the Secretary for the year 1916, showing the principal activities of the Institute.

Meetings.—The 112th Meeting, including the Annual Business Meeting, was held in New York City, February 14 to 17, inclusive, 1916. The full report of this meeting was given in Volume LIV of the *Transactions*. The number of papers presented was 53. The number of members and guests registered was 357.

The 113th Meeting was held in Arizona, September 17 to 25, inclusive, 1916. A special train conveyed members and guests from New York and other points *en route* to the meeting and returned them to their points of embarkment. This train left New York on Thursday, September 14, and returned on Friday, September 29. The full report of this meeting was given in Volume LV. The number of papers presented was 65. The number of members and guests in attendance was 284.

Publications.—Three volumes of *Transactions* were issued in 1916; namely, Volumes LI, LII and LIII. Volume LIV was printed for delivery in 1917.

Library.—The full report of the Library is given in the report of the Library Committee. Especial attention is called to the greatly increased activities of the Library Service Bureau, which acts especially for the benefit of the members distant from headquarters, keeping them posted on current literature on any engineering subject; making translations; photographic reproductions; reference lists; searches; compilations of statistical data on engineering subjects, etc.

Members' Rooms.—Two rooms at headquarters are devoted to the exclusive use of members and especially members from out of the city. The Institute now has a complete series of photographs of Past Presidents, which are hung in the larger Members' Room. An almost complete series of photographs of Honorary Members is possessed by the Institute, and many of these are hung in the smaller Members' Room. It is hoped that, with the coöperation of the members, the photographs of Honorary Members will shortly be completed.

Affiliated Student Societies.—With the assistance of the Committee on Junior Members and Affiliated Student Societies, there has been much better coöperation between the Affiliated Student Societies and the headquarters of the Institute, and the publication of the activities of the different Societies is now a regular feature of the monthly *Bulletin*.

Applications have been received and acted upon favorably in connection with the following societies: The Mining and Metallurgical Society of the Carnegie Institute of Technology, the Scientific Club of the Texas School of Mines and Metallurgy, El Paso, and the Michigan College of Mines, Houghton, Michigan. An application has been received from the Geological and Mining Society of American Universities, Black Hills Chapter, Rapid City, S. D., which is at present pending, awaiting further information from the Society.

Membership.—The membership of the Institute on January 1, 1916, was 5,224 and on December 31, 1916, was 5,774, a gain of 550 in the year.

Local Sections.—Two new Local Sections of the Institute were estab-

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lished in 1916; namely, the Nevada Section and the Mexico Section. The latter is a reorganization of the Instituto Mexicano de Minas y Metalurgia, and is the first Section of the Institute to be formed outside the boundaries of the United States.

Section Secretaries Attending Meetings.—The Board of Directors made a special effort during the year to encourage the attendance at meetings of the Institute of the Secretaries of Local Sections, in order that the influence of the Institute might in this way be more widely extended. The result was very encouraging in every way and nine out of the thirteen secretaries were in attendance at the Arizona Meeting.

Charles Kirchhoff.—On July 23, 1916, the Institute lost one of its most highly esteemed and honorable members, its Past President, Charles Kirchhoff. Joining the Institute within four years from its foundation he became at once, and for more than forty years continued to be, its earnest and active supporter, serving it as Manager, Councilor or Director for terms aggregating nine years; as Vice-President for two years and twice as President for a year. I am sure I express the feelings of all in placing on record our gratitude for the life and labors, and our sorrow in the loss, of our distinguished colleague.

American Society of Civil Engineers.—After preliminary negotiations, and after a vote of the membership of the American Society of Civil Engineers favoring action in the proportion of six to one, a contract was entered into between that Society and the present Founder Societies and the United Engineering Society, whereby the American Society of Civil Engineers became an additional Founder Society and part owner of the Engineering Society's Building and the land upon which it stands. Work is now progressing rapidly upon an addition of three stories to be placed upon the top of the present building. The library of the American Society of Civil Engineers has already been incorporated with the libraries of the three other Founder Societies, forming thus what is believed to be the largest and most nearly complete engineering library in the world.

Library Endowment.—Dr. James Douglas has added another to his many generous gifts to the engineering profession by presenting to the combined library of the four Founder Societies an endowment fund of $95,000. This, added to the $5,000 previously contributed by Dr. Douglas, makes the present handsome endowment of $100,000, which will be used to extend the usefulness of the library to those who are at a distance and cannot call in person, as well as to those who are able to visit it.

Engineer Officers' Reserve Corps.—When Major-General Leonard Wood, at a luncheon tended to him by the Secretaries of the National Engineering Societies, suggested coöperation between the Army Department and the Engineering Societies, toward the formation of a Reserve Corps of Engineers, an Institute Committee was quickly formed, which readily and effectively took up the work, in coöperation with similar committees from the other Engineering Societies. To Dr. Henry S. Drinker, Chairman of the Committee, to Arthur S. Dwight, his most active associate, and to Messrs. D. M. Riordan, and Warren A. Wilbur, the appreciation of the Institute is due for their able work which, with the coöperation of the others, resulted after about six months in a provision incorporated in the Army Reorganization Act, which became effective July 1, 1916, authorizing the organization of an Engineers Officers' Reserve Corps. Full details of this were published in the *Bulletin* for August, 1916, and have also been sent through the mails to every mem-

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ber of the Institute. I am proud to say that many members of the Institute have availed themselves of this opportunity to place their services at the disposal of the United States in the event of an emergeney.

Industrial Preparedness, Naval Consulting Board.—A letter from President Woodrow Wilson, dated January 13, 1916, invited the Institute to join with the American Society of Civil Engineers, the American Society of Mechanical Engineers, the American Institute of Electrical Engineers, and the American Chemical Society in appointing representatives in each state and territory of the union for the purpose of making an industrial inventory of the United States, in order that the country might be prepared to meet any emergency. It is a matter of history how well the members of the Institute responded, to the end that, in a few months the inventory was completed and the statistics classified and transmitted to the Army and Navy Departments. The Institute cordially thanks its members, many of whom served as associates on the Naval Consulting Board in the different states, while others generously assisted them in the making of the inventory.

Military Engineering Lectures.—The engineers of the country were quick to realize the importance of education in the matter of preparedness. Committees were formed in New York, Chicago, San Francisco, and other places, to awaken interest in the subject of preparedness, to carry on lectures on Military Engineering by officers of the United States Army Department, and to assist in organizing preparedness parades, which should bring the importance of the subject to the attention of the largest possible number of persons. The Institute was not officially represented in the formation of these committees, but the Board of Directors gave official sanction and individual assistance to the work.

National Research Council.—During our Civil War the National Academy of Science was chartered in response to a need that was felt of scientific advice to our Government. This Academy has been frequently consulted by Congress, by the President, and by other officers of the Government, and it was, therefore, requested by President Wilson to appoint a National Research Council, the object of which is to coördinate the scientific research work of the country in order to secure efficiency in the solution of the problems of war and peace. The Council was without funds until the Engineering Foundation, which exists under the auspices of the four National Engineering Societies, offered to place its resources at the Council's disposal, including the services of the Secretary, to act as Secretary of the Council. The offer was accepted and the Council is now enjoying office space on the 9th floor of the Engineering Society's Building. The following members of the Institute are members of this Council: Van H. Manning, Charles F. Rand, Charles R. Van Hise, Charles D. Walcott, W. R. Whitney.

John Fritz Medal Awarded to Dr. James Douglas.—All members of the Institute will rejoice in hearing of the award, in the year 1916, of the John Fritz Medal to our beloved Past President and Honorary Member and benefactor, Dr. James Douglas. The John Fritz Medal has been awarded to eleven persons other than John Fritz himself and the following members of the Institute have been recipients of it: James Douglas, Thomas Alva Edison, John Fritz, Robert Woolston Hunt, John Edson Sweet.

Respectfully submitted,

BRADLEY STOUGHTON, *Secretary.*

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REPORT OF TREASURER, 1916

Receipts

General Funds:		
Initiation fees	$6,008.50	
Arrears of dues	1,772.69	
Current dues	46,576.56	
Advance dues	1,820.97	
Sale of binding	10,554.43	
Sale of advertising	6,003.42	
Sale of Transactions	3,237.07	
Sale of special editions	1,722.50	
Sale of Bulletins and pamphlets	2,263.95	
Miscellaneous Receipts:		
Interest on investments and deposits	480.06	
Sale of pins and fobs	312.80	
For the collection of checks	161.84	
From Finance Committee, Feb., 1916, Meeting	101.02	
Subscriptions to the land fund	86.14	
Arizona Meeting refund from N. Y. Central Railroad	52.48	
Local Section refund from N. Y. Central Railroad	31.85	
Miscellaneous	425.68	
Total Receipts General Funds	81,611.96	
Cash balance Dec. 31, 1915	1,063.39	
		$82,675.35
Special Funds:		
Life memberships to be invested	$750.00	
Barron Fund to furnish the Members' room	637.02	
Interest on Hadfield Prize	33.53	
Interest on Thayer Prize	2.99	
Total Receipts Special Funds	1,423.54	
Cash balance, Dec. 31, 1915	2,065.84	
		3,489.38
		$86,164.73

Payments

General Funds:		
Bulletin	$17,789.60	
Year book	1,218.72	
Transactions	8,777.17	
Binding	7,175.25	
Special editions	292.73	
Editorial and office	25,748.06	
Treasurer	1,009.89	
Library	4,580.00	
Advertising	1,989.05	
Meetings	2,385.01	
Local sections	2,105.98	
Technical Committee	84.30	
Committee on Increase of Membership	1,042.62	
Back volumes	998.18	
Circulars	505.20	
Miscellaneous	3,136.42	
Total Payments, General Funds	78,838.18	
Balance, Dec. 31, 1916	3,837.17	$82,675.35
Special Funds:		
Life memberships invested	939.53	
Barron Fund to furnish the Members' Room	637.02	
Total Payments, Special Funds	1,576.55	
Balance, Dec. 31, 1916	1,912.83	3,489.38
		$86,164.73

GEORGE C. STONE, *Treasurer.*

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REPORT OF THE COMMITTEE ON MEMBERSHIP FOR 1916

The total number of applications brought before the Committee during the year 1916 was 962; the total number of persons who were elected and became members of the Institute during the same period was 750.

The total membership of the Institute on Dec. 31, 1916, was 5,781, consisting of 19 Honorary Members, 5,246 Members, 215 Associate Members, and 301 Junior Members. The changes in membership during the year are shown on the accompanying schedule:

Total membership, Dec. 31, 1915		5,221	
Loss by resignation	53		
Loss by suspending	125		
Loss by death	57		
Total loss during 1916		235	4,986
Gain by election	750		
Gain by reinstatement	45		
Total gain during 1916			795
Membership, Dec. 31, 1916			5,781
Change of status:			
Associates to Members	4		
Junior Members to Members	11		
Total	15		

KARL EILERS, *Chairman.*

REPORT OF THE COMMITTEE ON INCREASE OF MEMBERSHIP FOR 1916

During the year Jan. 1 to Dec. 31, 1916, 962 applications for membership in the Institute were received, as compared to 558 during the corresponding period in 1915.

The lines along which the Committee has worked during the year group themselves under seven principal heads.

Large Companies.—Early in the year a letter was addressed to each member of the Institute who is the executive head of an organization, pointing out to him the advantages that would accrue to that organization through having as many of its subordinate members as were qualified participating in the activities of the Institute. This was followed by a second letter to those who made no reply to the first. The response to these letters was very gratifying, 12 new members having been secured at a single plant, and smaller numbers at others. This was effectively supplemented by our President, who wrote the executive heads of some of the largest mining and smelting companies, pointing out to them the advantage to be derived from sending some of the junior members of their staff to attend the Arizona Meeting. One organization sent 24 of its younger men, many of whom were not members of the Institute, but who subsequently sent in applications for membership. It is to be hoped that the precedent thus set will be acted on in fuller measure in succeeding years.

Mexican Section.—Early in the year arrangements were consummated whereby the Mexican Institute of Mining and Metallurgy became the Mexican Section of our Institute. It is gratifying to have the relations between the two countries thus strengthened, and when more peaceful

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conditions are there attained it is reasonable to expect that a large number of new members can be secured in Mexico.

Prospective Members.—Up to the present time this Committee has chiefly busied itself with presenting to the attention of those known to be engaged in mining and metallurgical work, but not members of the Institute, the advantages of membership therein. There are, throughout the country, a very large number of men actually engaged in mining and metallurgical work who are not known by any member of the Committee, and possibly not by any member of the Institute. Reaching these men is a difficult task, for even if we were able to obtain their names and addresses we have no personal knowledge that they are qualified for membership, and a general invitation to apply for membership might result in embarrassment, since the Membership Committee rigidly upholds our standards of membership. Much thought has been given to this problem, and its solution has been aided by the Board of Directors by placing at the disposal of this Committee such funds as are required for the effective conduct of its work. Arrangements have been made to provide the Committee with a paid assistant secretary, with headquarters at the Institute building, and a systematic campaign is being made to secure the names and addresses of men actually engaged in mining engineering and metallurgical work who are not members of the Institute, with good results. The securing of personal contact with these men has not been so well worked out. Efforts have been made to secure the aid of local members of the Committee, and of the Secretaries of local sections, and this may perhaps be more effectively done in the future. Meanwhile a more comprehensive plan is under discussion and will be submitted to the Board of Directors for its approval at an early date.

Oil and Gas Men.—Through the courtesy of Messrs. Tinsley and Woodworth, the names and addresses of a large number of men engaged in the oil and gas industry, and who would make desirable members for the Institute, were secured. These gentlemen were invited to join, but the response has so far been somewhat disappointing. The papers and discussions dealing with oil and gas before the Institute are of much importance, and means of bringing that fact home to those engaged in the oil and gas industry should engage our attention during the coming year.

Iron and Steel; Coal.—Many members of the Institute are engaged in the iron and steel industry, but in proportion to the relative numbers of men involved our representation in this field is smaller than it should be. The same may be said of the coal-mining industry. With the coöperation of the Technical Committees for these branches, we hope to secure a steady increase of membership in these fields.

Foreign Members.—The growing importance of the Institute is reflected by the increasing number of members who are residents of other countries. The greatest proportionate increase has been in China and Japan, but Canada, Australia, and the South American countries have shown large gains. South America offers an especially attractive field for the work of this Committee.

Total Membership.—Two years ago the membership roll of the Institute passed the 5,000 mark. Since that time over 1,500 applications for membership have been received, but we have not yet reached the 6,000 mark. This is due to the losses by death, resignation, and those members who, in accordance with the Constitution, are suspended from the rolls for non-payment of dues. The total membership of the Institute on Dec. 31,

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1916, was 5,904, but as 130 members were dropped on Jan. 1, 1917, for non-payment of dues, we shall not pass the 6,000 mark until the middle of this year. As the losses through non-payment of dues are heavy it may eventually prove good policy for this Committee to exert itself in order to [illegible] possible, in preventing the loss of members through this cause. [illegible] source of loss arises from the fact that only 85 per cent. of those [illegible]ly for membership finally become members by accepting election [illegible]ying their dues. It seems probable that closer coöperation be[illegible]s Committee and the Local Sections will be of service in meeting these problems.

Respectfully submitted,

THOMAS T. READ, *Chairman.*
W. H. SHEARMAN, *Secretary.*

REPORT OF THE LIBRARY COMMITTEE FOR THE YEAR 1916

In accordance with the requirements of By-Law LX, I beg leave to submit herewith the report of the Library Committee for the year 1916.

The activities shown by the sale of the Institute's publications during the year are somewhat larger than last year, due principally to the increased sale of special editions on account of the larger sale of the Posepny and Emmons Volumes, owing to increased advertising and to the reduction in price of the Emmons Volume. The sales were distributed as follows:

Sale of Transactions	$3,237.07
Sale of Special Editions	1,722.50
Sale of Bulletins and Pamphlets	2,263.95
Total	$7,223.52

The Institute library has now been merged with the libraries of the other three Founder Societies and is known and administered as the Library of the United Engineering Society. When this merger was effected, a proviso was made that the books contained in the Institute library up to the date of the merger were still to be considered as the individual property of the Institute, and this arrangement applied also to the libraries of the other two Founder Societies.

In view of the changed situation since the first merger, your Committee suggests that it might be well to consider a relinquishment of ownership to the books which formerly comprised the library of the American Institute of Mining Engineers.

The addition to the United Engineering Society's library during the year amounted to a total of 2,902 volumes, pamphlets and maps, distributed as follows:

	Volumes	Pamphlets	Total
Gifts	795	311	1,106
Exchanges	658	13	671
Purchases	980	27	1,007
Old material	22	35	57
	2,455	386	2,841
Maps	....	....	61
Total	....	....	2,902

The activities of the library during the year have been very much increased, the attendance having amounted to 13,848.

At the time the Library Board came into control of the united libraries

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they found a very large number of duplicate books stored away and unavailable. These duplicates have now all been carefully gone over, and hidden away amongst them were found quite a number of books which were not duplicates; these have now been added to the shelves of our library. All of the real duplicates were properly listed and sold, the proceeds being added to the endowment fund of the library.

Your Committee takes great pleasure in reporting to you the receipt of a further gift of $95,000 from our Member, Dr. James Douglas, which gift has been added also to the library endowment fund.

During the year, the Library Board has installed a photostat machine for the reproduction of articles from periodicals, maps and drawings. The necessity for this installation has been shown by the constant work it has been called upon to perform. Since its installation in April last, 4,772 reproductions have been made.

Preparations are now in progress to receive and house the library of the American Society of Civil Engineers, and the actual moving of the books will take place early in 1917.

The Library Service Bureau, instituted less than a year and a half ago, has proved that the service was greatly needed, as it at once became extremely popular. Previous to its organization, the demands for search work in the library amounted to less than $200 per annum. The past year the receipts have reached approximately $5,800, received from 589 applicants and classified as follows:

	Since Jan. 1, 1916
Civil Engineering	47
Electrical Engineering	49
Mechanical Engineering	130
Mining Engineering	54
Metallurgical	46
Chemical	95
Miscellaneous	74
Total (Searches)	495
Translations (orders)	82
Abstracts	7
Copying	5
Total (Miscellaneous)	94
Grand Total	589

The expenditures for salary and work done amounted to $5,366. The initiation of this work was made possible by a loan of $250 from each of three Founder Societies, to be repaid out of any balances existing in the year's operation. I am glad to be able to say that this amount has now been practically earned and is ready to be returned to each of the Societies.

Owing to the amount of extra work which will be entailed by the accession of the 67,000 volumes from the American Society of Civil Engineers, the expenses of the library will be considerably increased, but this will not require any increased appropriation from the Founder Societies as with the additional $4,000 received from the American Society of Civil Engineers and the $5,000 interest from the endowment fund these extraordinary expenses will be met.

As required by the By-Law, I attach herewith the stock of publications on hand.

Respectfully submitted,

E. GYBBON SPILSBURY, *Chairman.*

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PAPERS

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Biographical Notice of Charles Kirchhoff

BY R. W. RAYMOND, NEW YORK, N. Y.

(New York Meeting, February, 1917)

CHARLES WILLIAM HENRY KIRCHHOFF was born March 28, 1853, at San Francisco, Cal., where his father, Charles Kirchhoff, was at that time consul for his native country, Germany. A few years later, the family moved to Hoboken, N. J., in which city the son received his preliminary education at the Hoboken Academy, proceeding later to Germany, where he was graduated in 1874 as mining engineer and metallurgist at the Prussian Royal Mining Academy of Clausthal, in the Harz. Upon his return to the United States, he became chemist of the Delaware Lead Refinery at Philadelphia, and retained that position for three years. But in 1877 he began what was to be an almost uninterrupted life-long association with David Williams, the publisher of *The Iron Age*, of New York, who established in that year, and continued for a brief period, a journal entitled the *Metallurgical Review*, on the editorial staff of which Mr. Kirchhoff received a place. The enterprise was doubtless an attempt to cover a wide metallurgical field outside of that which properly belonged to *The Iron Age*. But it was soon abandoned, and Mr. Williams wisely concentrated his energies upon the older journal, which, under his vigorous and skillful management, and the labors of the able editors and correspondents whom he selected, became one of the greatest institutions of its class in the world. From 1878 to 1881, Mr. Kirchhoff was assistant editor of *The Iron Age*. From 1881 to 1884, he was managing editor of the *Engineering and Mining Journal;* in 1884 he returned to *The Iron Age*, to become for five years its associate editor, and in 1889, on the retirement of James C. Bayles, editor-in-chief.

This list of dates and employments, without further comment, sufficiently indicates, at least to the eye of an expert, that Mr. Kirchhoff had found his congenial career in trade journalism. His qualifications for this profession were somewhat exceptional. He possessed the scientific training, the knowledge of foreign languages and literatures, and the power of making and keeping friends, which enabled him to get early notice of technical novelties; he had the taste for statistics which made him both industrious and intelligent in their collection and use; and to these traits he added a mastery of the meaning of such accumulated data, and a sane, critical judgment of the situations which they represented, as well as of the sources and the figures themselves, which made his opinion weighty

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concerning them. His estimates and prognostications, expressed in quiet, clear, forcible but unsensational style, were quoted by the non-technical press, when editorial utterances, more brilliant in rhetoric or pleasantry, passed unrepeated. In short, his mind was equipped for the long-distance as well as the near-by view of things; he could see both science and business, and his lens was achromatic at either focal distance. Such men surely achieve reputation and influence, and never lose what they have thus patiently won.

It was in 1883, while he was managing editor of the *Engineering and Mining Journal*, that Mr. Kirchhoff was engaged by the Director of the U. S. Geological Survey to collect and arrange annually the statistics of the production of lead, copper and zinc in this country. Having had, at an earlier period, some personal experience in work of that sort, I feel myself qualified to judge of its difficulties and to recognize success in the overcoming of them. It is not a simple matter of commanding the reports of all producers and compiling the results. It means the personal winning of the confidence of individual producing concerns, one after another; the intelligent reduction of their data to a common standard; the ingenious filling of gaps in the returns; the checking of statistics of production by those of transportation, sales and stocks on hand—in short, the editing, in a critical as well as a mechanical sense, of a mass of material more or less incomplete and heterogeneous. Such a labor, performed by a competent hand through a series of years, doubtless grows easier and easier because it educates the contributors while it makes them increasingly willing to do their part toward a result so creditable and useful. Mr. Kirchhoff's reports, continued for 23 years, constitute a lasting memorial of his own ability and of his influence upon the managers of great national industries.

But a man might possess all the admirable qualities above enumerated, and yet lack the indefinable gift of administration. This gift Mr. Kirchhoff exhibited when in 1904, upon the death of John S. King, business manager of *The Iron Age*, he became Vice-President and General Manager of the great David Williams Co. This responsibility, as well as that of his editorial position, he carried successfully until his retirement from active business at the close of 1909, when *The Iron Age* was sold to other parties. This was the occasion of a luncheon in his honor, given by his friends and associates at the Engineers' Club, Jan. 16, 1910, and made notable by the attendance of many distinguished engineers and captains of industry, including the venerable John Fritz, then 87 years old, who made the journey from Bethlehem for this purpose. Numerous letters and speeches (including the presentation, by Mr. Kirchhoff's associates in the David Williams Co., of a bronze statue) bore witness to the admiration and esteem with which he was universally regarded. There was general regret over his retirement from a position which he had so

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signally adorned—a regret from which, as I remember, I ventured to dissent, preferring to praise and congratulate him upon a rest well earned, and taken at a time when, with powers unimpaired, he could both enjoy and make fruitful of good to his fellows a life more wide and free. Alas! it was otherwise decreed.

Mr. Kirchhoff's activity and reputation were not confined to the United States. As editor of *The Iron Age*, he maintained relations with technical and commercial leaders abroad, and frequently visited Great Britain, Germany and France for the purpose of studying the developments of the iron and steel industries of those countries. A series of his articles in *The Iron Age*, republished in 1900 as a book, entitled "Notes on Some European Iron-Making Districts," was widely read. After his retirement, he reviewed in the columns of *The Iron Age* the proceedings of the International Metallurgical Congress, which he attended at Düsseldorf in the spring of 1910; and in the following October he read before the American Iron and Steel Institute in New York a notable paper on the recent progress of iron and steel metallurgy, as reflected in that Congress.

Mr. Kirchhoff was specially active and prominent in connection with the American Museum of Safety Devices and Industrial Hygiene, on the managing board of which he served for several years. His labors in this field were recognized by the government of France, which conferred upon him in 1908 a decoration as *Officier d'Instruction Publique.*

He was a member of the American Iron and Steel Institute, the Iron and Steel Institute of Great Britain, the Verein deutscher Eisenhüttenleute, the American Society of Mechanical Engineers, and honorary member of the Franklin Institute of Philadelphia.

He joined the American Institute of Mining Engineers in 1875, immediately after beginning his career as a metallurgical chemist. In 1884, he was strongly supported as a candidate for the office of Secretary, left vacant by the resignation of Dr. Drown. He and I were not only friends but associates at that time on the editorial staff of the *Engineering and Mining Journal;* and these relations were not at all disturbed by our amicable contest for the Institute secretaryship. No one who was present at the banquet held in connection with the Cincinnati meeting of February, 1884, has forgotten the graceful and humorous speech made by Mr. Kirchhoff from the standpoint of the defeated candidate. Thenceforward to the end of his life, he was my strong supporter, wise adviser, and loyal and beloved friend.

In 1887 and 1888, and again in 1892 and 1893, he served as a member of the Council; and in 1896 and 1897, as Vice-President; in 1898 and again in 1911 as President; from 1907 to 1912 as Director, and in 1913 as Past-President. His presidential address, at the New York meeting of February, 1899, on "A Decade of Progress in Reducing Costs," is a classic, showing precisely what such a paper should be, and exhibiting those char-

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acteristics of careful and tireless inquiry, comprehensive grasp of details, wisdom in generalization and clearness of statement which I have already noted as the elements of his power and success.

But not less important, and far more laborious, was his work in the memorable year 1890, when the Institute, with the aid of American engineers and ironmasters, entertained some 500 guests, belonging to the Iron and Steel Institute of Great Britain and the Association of German Ironmasters. Mr. Kirchhoff was Secretary of the American Reception Committee, of which Mr. Andrew Carnegie was Chairman. But his most arduous labors were performed in connection with Mr. W. P. Shinn of the Transportation Committee, for whom he personally conducted one of the two Pullman trains which were kept running through the month of October with the foreign guests as passengers.

For some years before his death, Mr. Kirchhoff fought a brave battle against a disease usually regarded as fatal. With patient and cheerful courage, he held his own for a while, and even seemed to be gaining ground. But it was trench-fighting, and gains were small and slow. An attack of the grip, reinforcing the older enemy, brought the end at last; and he died, July 22, 1916, at his summer home near Asbury Park, N. J. He leaves a widow, two sisters and a brother.

In closing this imperfect sketch of my dear friend, I can present no better summary of his character than that which is embodied in the Minute adopted by the Directors of the American Institute of Mining Engineers upon the tidings of his death, and I therefore repeat as my own this paragraph:

"Mr. Kirchhoff was one who secured, because he practiced, loyalty in friendship, justice and kindness in personal intercourse, openness and equity in business. Modest, but not timid; prudent, but progressive; gentle, but firm; full of chivalry and of common sense; indefatigably industrious and inexhaustibly patient, he won victories without making enemies, and crowned with a heroic death the record of a life unstained."

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The Manganese Ores of the Lafayette District, Minas Geraes, Brazil

BY JOSEPH T. SINGEWALD, JR.,* PH. D., BALTIMORE, MD., AND BENJAMIN LEROY MILLER,† PH. D., SOUTH BETHLEHEM, PA.

(New York Meeting, February, 1917)

INTRODUCTION

FOR a number of years Russia, India and Brazil have outranked all other countries as producers of manganese ores. During the 5 years immediately preceding the European war, the average annual production of Russia was 844,000, of India 694,000 and of Brazil 200,000 long tons. Since the outbreak of the war there has been a considerable falling off in the Russian and Indian production, particularly in the former, whereas the production of Brazilian ores has been greatly increased, amounting in 1914 to over 250,000 tons and in 1915 to nearly 350,000, with conditions favorable for a still larger production in 1916. Manganese is one of the few industrially important metals that are not produced in the United States in quantity commensurate with our needs, so that we have been compelled to import annually about 300,000 tons, having a value of over $2,000,000, and these ores have been obtained from the three countries mentioned above. Their rank as contributors to our imports of manganese ores was India, Russia, Brazil; and in 1913, Brazil contributed only one-fifth. During the past 2 years Brazil has furnished rapidly increasing quantities to this country, and, with the falling off of imports from India and Russia, has become our principal foreign source. In 1914, Brazil furnished two-fifths of the imports and more than India or Russia, and in 1915 over nine-tenths. The manganese ores of Brazil are consequently of more than usual interest to us at this time.

THE MANGANESE-ORE DISTRICTS OF BRAZIL

The manganese mining industry of Brazil dates from the year 1894, and since that year the total production has been over 3,000,000 tons. With the exception of a small quantity produced in the State of Bahia from deposits west of the City of Bahia, this output has come from the

* Associate in Economic Geology, Johns Hopkins University.
† Professor of Geology, Lehigh University.

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State of Minas Geraes and has been exported through the port of Rio de Janeiro. In Minas Geraes there are two distinct, though not widely separated, manganese districts, known generally as the Miguel-Burnier and the Queluz or Lafayette districts. The Miguel-Burnier district was the first to be developed and for a number of years was the more important producer; but, since the phenomenal development of the Morro da Mina mine, the Lafayette district has assumed far greater importance.

In order to contrast the modes of occurrence of the deposits in the two districts, a brief description of the Miguel-Burnier district will be given, in the nature of a summary of a previously published fuller account.[1] The deposits of this district extend as a narrow belt about 10 miles long, parallel to the Ouro Preto branch of the Estrada da Ferra Central do Bresil, and lie at the southern edge of the great iron-ore region of central Minas Geraes. The orebodies occur as lenses or beds intercalated in a series of sedimentary strata showing a rapid succession of itabirite, schist, calcareous schist, and limestone. Stratigraphically they are found in the upper part of the Itabira iron-formation and in the lower part of the overlying Piraçicaba schist,[2] formations of probable Algonkian age. The ores are very high-grade manganese oxides, chiefly a mixture of psilomelane and pyrolusite, averaging 50 per cent. manganese, 1 per cent. silica, and 0.03 to 0.05 per cent. phosphorus. The great drawback of the district is the fact that the beds are steeply dipping and narrow, rarely over 6 ft. in width, so that expensive underground mining must be resorted to, and the individual deposits are relatively small. Two views have been advanced to explain the origin of the ores. H. K. Scott, who has written the most complete account of them, says:[3] "Whatever may have been the original state of the manganese ore bed, there can be no doubt that in its present condition, and down to the level to which it has been worked, it is a residual deposit from which the other elements have been leached out." O. A. Derby, in a discussion of Scott's paper, endorsed this view and referred to the original state of the manganese ore beds as limestone with varying proportions of metallic carbonates and siliceous impurities.[4] On the other hand, Harder and Chamberlin say:[5] "From their occurrence it must be assumed that they are similar in origin to the associated rocks, that is, that they are original sedimentary

[1] Joseph T. Singewald, Jr., and Benjamin LeRoy Miller: High-Grade Manganese Ores of Brazil. *The Iron Age*, vol. 97, pp. 417–420 (1916).

[2] E. C. Harder and R. T. Chamberlin: Geology of Central Minas Geraes, Brazil. *Journal of Geology*, vol. 23, pp. 358–363 (1915).

E. C. Harder: Manganese Ores of Russia, India, Brazil and Chile. *Bulletin* No. 113, p. 788 (May, 1916).

[3] H. K. Scott: Manganese Ores of Brazil. *Journal of the Iron and Steel Institute*. vol. 57, pp. 188–189 (1900).

[4] *Idem*, p. 212.

[5] *Op. cit.*, p. 406.

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deposits of manganese oxide." The same statement is made by Harder alone.[6] Our own feeling is that while one cannot positively state that the ores were not laid down in the form of manganese oxides as integral parts of a sedimentary series, their relations to the associated limestones are such as to make their interpretation as residual products of decomposition and replacement of manganiferous limestone the more probable explanation of their origin.

The Lafayette District

Manganese deposits were discovered in the Lafayette district immediately after the inauguration of mining operations in the Miguel-Burnier district stimulated a search for manganese ores in Minas Geraes, but it was not until the year 1900 that the district became a regular producer, an output of 31,000 tons coming from the Piquery and São Gonçalo Mines in that year. This was increased to nearly 75,000 tons the following year, a production considerably in excess of that of the entire Miguel-Burnier district. In 1902, the Morro da Mina came in as a producer and firmly established the district as the chief manganese-producing district of Brazil.

Lafayette is a station, located at the edge of the town of Queluz, on the Estrada da Ferra Central do Bresil, about 32 km. south of Burnier and 462 km. from Rio de Janeiro. From the fact that the most important producing mines have been located in its vicinity the district has been generally referred to as the Lafayette district. The most important mine at present is the Morro da Mina, owned and operated by a Brazilian company known as the Companhia Morro da Mina, which has increased its production to 700 tons per day. It is located 7 km. northeast of Lafayette. On the same hill, a German company under the name Mineração de Agua Preta has been working the rubble ores to the east of the Morro da Mina ground. This company is producing at the rate of 2,000 tons per month and in the 6 or 7 years it has been operating has produced a total of 200,000 tons. On a small hill to the southeast of the Morro da Mina, the extension of that zone is being developed by a company known as the Companhia Queluz da Mina. The only other producing mine is the Cocuruto which lies about 40 km. southwest of Lafayette and is connected with the railroad at Christiano, a station 23 km. further south, by a 60-cm. gage line 40 km. in length. This mine has been operated about 7 years and is producing 3,000 tons monthly. It is owned by a Belgian company, the Société Anonyme de Manganese de Ouro Preto, which formerly worked the São Gonçalo Mine. After each had produced about 250,000 tons of ore, the São Gonçalo and Piquery mines were abandoned 10 years ago as worked out. They

[6] *Op. cit.*, p. 791.

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were located about 15 km. northwest of Lafayette. The Piquery Mine is of particular interest in that it is the only one of which there is a geological description and it was there that Derby obtained the first evidence of the original character of the manganese rock from which the ores were derived.

In addition to those mentioned, a number of other deposits have been discovered and prospected to some extent, but apparently the results were not favorable enough to warrant further development as none of them became important producers. It is certain, however, that the district has not been thoroughly prospected and there is every reason to expect that systematic exploration would discover deposits equal to those that have been found.

Geology of the Lafayette District

The geology of the Lafayette district differs markedly from that of the Miguel-Burnier. It lies to the south of the area underlain by the

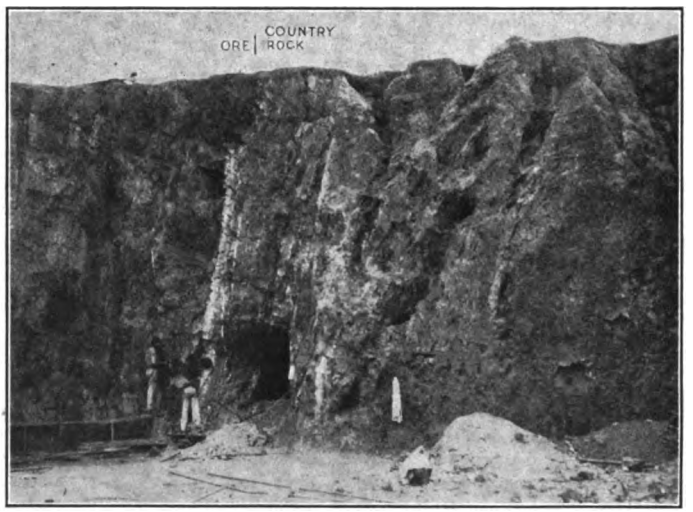

FIG. 1.—SHARP CONTACT OF MANGANESE ORE AND COUNTRY ROCK IN MORRO DA MINA MINE.

great iron-bearing series, and its ores are found in the basement complex of supposed Archean age which underlies a large part of the State of Minas Geraes. The rocks making up this basement complex are chiefly granite and gneiss with which are associated amphibolite, and micaceous and quartzose schists. There are also small intrusions of diorite and gabbro, mostly in the form of dikes. The granite seems to be intrusive into the gneiss and schist, but the relations between the schist and gneiss are not clear.

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The manganese deposits occur as elongated masses of more or less lenticular shape within the rocks of the basement complex. As will be explained below, they represent residual products of decomposition of an original manganiferous rock made up of manganese carbonate and silicates. The immediate wall-rock of the deposits has likewise undergone decomposition, in many instances being nothing more than a clay in which the original rock texture is poorly preserved, so that it is usually difficult to determine its original character. In most cases, however, it seems to have been either gneiss or schist. The contact of wall-rock and ore generally appears quite sharp, as is shown in Fig. 1, but closer examination often reveals small nests and stringers of manganese oxide in the decomposed rock. There are also horses of equally decomposed rock and of the same character within the orebodies themselves.

The Piquery Mine

The geological relations of the Piquery orebody have been described by O. A. Derby in two papers published in 1901 and 1908,[7] and the following account is abstracted from them.

"The Piquery orebody presents the appearance of a mass of secondary material, or gossan, resulting from the alteration of a vertical dike or vein, some 10 or 12 m. wide. . . . The ore is a hard spongy black oxide, apparently consisting for the most part of psilomelane but with an admixture of other oxides that frequently occur in beautiful crystallizations in the spongy cavities. . . . In the midst of the merchantable ore occur inconstant bands and patches of hard siliceous material with the appearance of a quartzite, but which on examination proves to be composed almost exclusively of a finely granular mass of ashy white manganese garnet. A complete series of alteration phases between perfectly typical garnet rock and merchantable ore can be readily selected, and there can be no doubt that the latter results from the decay and leaching of the former."

Derby describes three phases of the garnet rock that he observed in 1901:

"1. A very fine-grained, compact and finely jointed rock of bluish-gray color with partings lined with asbestos. Under the microscope the rock is seen to be composed almost exclusively of closely appressed idiomorphic grains of white garnet showing a clear border but with the center highly charged with a fine black opaque powder that appears to be graphite. . . .

"2. A dark brown rock heavily charged with manganese oxide and too friable to permit the preparation of microscopic sections is evidently of the same type but more completely decomposed. . . .

[7] O. A. Derby: On the Manganese-Ore Deposits of the Queluz (Lafayette) District Minas Geraes, Brasil. *American Journal of Science*, Ser. 4, vol. 12, pp. 18–32 (July, 1901).

O. A. Derby: On the Original Type of the Manganese-Ore Deposits of the Queluz District, Minas Geraes, Brasil. *American Journal of Science*, Ser. 4, vol. 25, pp. 213–216 (March, 1908).

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"3. A milky white rock which under the microscope is seen to be composed of about equal parts of garnet and quartz. . . . The quartz in a fine mosaic about the garnet grains and in minute refilled joints is almost certainly secondary."

Residues of manganese garnet were likewise found by Derby at the São Gonçalo, Morro da Mina, Agua Limpa and Barroso Mines. The last two mines were located about 10 km. southeast and 9 km. south of Lafayette respectively. As the result of his observations at these mines, Derby drew the following conclusions concerning the origin of the deposits:

"The orebodies of the Queluz district are residual deposits derived through decomposition and leaching from an original type or types of rock in which manganese garnet was the most constant and characteristic silicate element. . . . This type, which may appropriately be denominated *queluzite*, is more or less intimately associated at São Gonçalo, Morro da Mina and Barroso with decomposed schistose rocks that evidently contained an original manganese-bearing silicate and which from the absence of recognizable clastic elements and from other characteristics, so far as they can be made out, is presumed to have been an amphibolic schist representing a sheared basic eruptive. . . . In the Agua Limpa schist, moreover, the manganese-bearing element is spessartine, as in the orebodies, thus giving greater plausibility to the hypothesis that the relation between these last and the above-mentioned rocks may be a genetic one. If thus related, the orebodies present strong analogies with those of magnetic titaniferous and chromic iron ores that are now generally considered as magmatic segregations in various types of eruptives, and, all things considered, this hypothesis seems the most plausible one for the manganese ores here discussed."

After the Piquery orebody was worked out and the original manganiferous rock was exposed in the bottom, Derby made a further study of it, the results of which are given in the 1908 paper. He found that the rock consists mainly of "a black, fine-grained, highly jointed and somewhat flaggy rock with the aspect of a limestone, with broad bands and patches of a more massive, yellowish-gray rock with the aspect of a quartzite." The latter is the garnet rock described in the previous paper, but which now turns out to be of secondary importance. The limestone-like rock on treatment with cold weak acid effervesces freely with an abundant separation of gelatinous silica and an insoluble residue containing spessartite and graphite. In places there is also a considerable admixture of rose-colored rhodonite in streaks and patches as the predominant component. Microscopic examination by Dr. Hussak showed the rocks to consist of manganese carbonate, tephroite, and spessartite with a small amount of rhodonite. The paper gives three analyses of these rocks, of which No. I was selected with reference to a supposed high carbonate content, No. II as having a lower carbonate content, and No. III was taken from the earlier paper and represents the garnet rock first found.

For the purpose of comparing the rocks represented by these analyses with the Morro da Mina rocks described below, their mineralogic composition has been calculated approximately on the basis of their described

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Analyses of Original Manganese Rock at Piquery

	I	II	III
CO_2	22.62	4.59	.....
SiO_2	11.80	27.69	38.47
MnO	47.52	57.48	27.90
Al_2O_3	7.50	1.41	21.07
Fe_2O_3	.....	2.48	7.38
CaO	3.76	1.82	4.70
MgO	6.27	4.60	.....
	99.47	100.07	99.52

mineralogic characteristics. The commercial ores average twice as high in alumina as in ferric iron and on this basis there is just enough ferric iron and alumina in analysis I to take care of the silica in forming spessartite. If all the CO_2 is calculated as rhodochrosite, there is a deficiency of 4 per cent. MnO. But there is more than enough CaO and MgO to take care of this deficiency, so that roughly this rock consisted of 32.6 parts of spessartite and 59 parts of manganese carbonate by weight, or 32 and 68 parts respectively by volume. If in analysis II, the ferric oxide and alumina are calculated as manganese garnet, the CO_2 as rhodochrosite and the remaining silica as tephroite, there is a deficiency of nearly 9 per cent. MnO which is slightly more than covered by the CaO and MgO. On this basis the rock consisted of 12 parts of rhodochrosite, 14.5 parts of spessartite and 75.2 parts of tephroite by weight, or 17, 17.5 and 65.5 parts respectively by volume. Analysis III represents a garnet rock with a little more alumina and about 4 per cent. more silica than is needed to take care of the MnO, CaO, MgO and iron considered as ferrous iron. On this basis it consisted of 65 parts of spessartite, 12.6 of grossularite, and 15.3 of almandite by weight. Actually, of course, the rock consisted of a garnet intermediate in composition between the three but more nearly approaching the composition of spessartite. These calculations show the same great variation in the mineralogic composition of the Piquery rock that is found at the Morro da Mina Mine, except that rhodonite does not appear to have the prominence it attains at the latter.

These new observations necessitated different conclusions and Derby in this second paper says of the deposits that they "seem to be due to the alteration of an original rock with predominant carbonate of manganese and tephroite rather than of spessartite and rhodonite as hitherto supposed." In regard to the genesis of the rock he is silent in this paper, nor does he apply the term *queluzite* to it.

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The Morro da Mina Mine

The Morro da Mina Mine is located on a hill 2 or 3 miles north of the town of Queluz, which has an elevation of 1,110 m. above sea level and rises to a height of 200 m. above the surrounding country. A branch

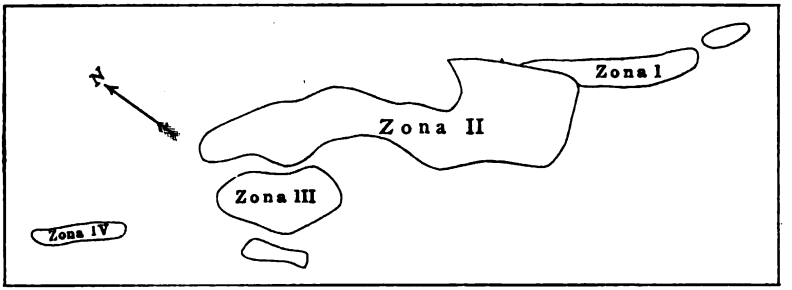

FIG. 2.—HORIZONTAL SECTION OF MORRO DA MINA OREBODIES AT LEVEL 55 M BELOW SUMMIT OF HILL.

line of the railroad connects the mine with the Estrada da Ferra Central do Bresil at Lafayette, so that the ore can be loaded into the cars at the

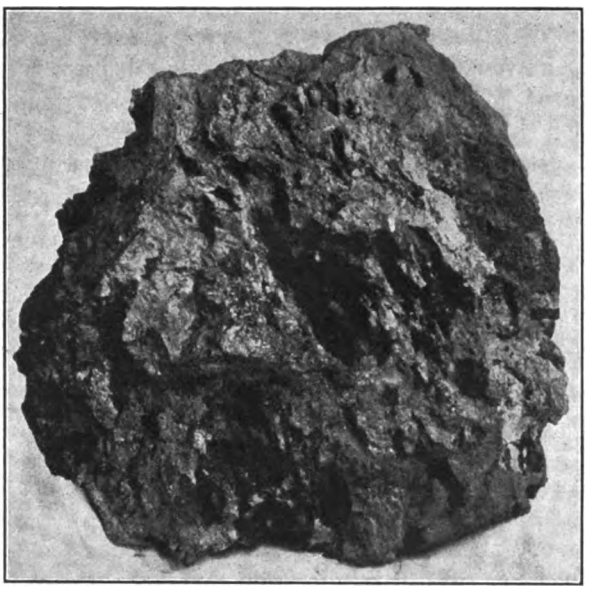

FIG. 3.—SOMEWHAT DRUSY MASSIVE PSILOMELANE, THE MOST COMMON TYPE OF ORE AT THE MORRO DA MINA MINE.

mine and requires no further handling until transferred to ships for exportation at Rio de Janeiro. The mine presents one of the most remarkable manganese deposits in the world both in respect to size and quality

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of the ore; in fact, the manager of the mine, J. de A. Lustosa, says it is the largest known deposit of high-grade manganese ore. Since it became an important producer in 1902, it has yielded a total of over 1,000,000 tons, and in 1915 its production was about 200,000 tons. Development work has proved an ore reserve of 10,000,000 tons.

The orebodies occur at the top and on the flanks of the hill as a series of more or less overlapping lenses extending in a direction N 35° W., with a vertical dip and a pitch of 45° to the southeast. The relative positions, shapes and sizes of the orebodies that have been developed as they occur on the level 55 m. below the top of the hill are shown on the mine map in Fig. 2. The four largest are known as Zona I, II, III and IV respectively and have maximum dimensions of 200 by 30 m., 420

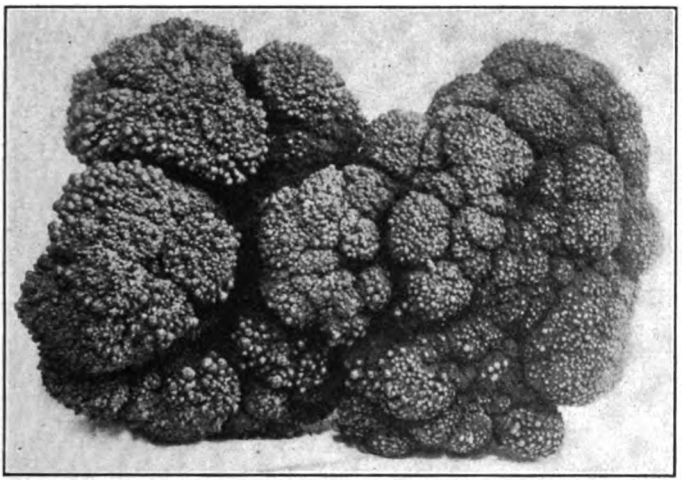

FIG. 4.—CAULIFLOWER-LIKE CLUSTER OF MANGANESE OXIDES FOUND QUITE FREQUENTLY IN THE MORRO DA MINA ORES.

by 120 m., 140 by 70 m., and 100 by 20 m. In depth Zona I and Zona II have been cut by a development tunnel 130 m. below the summit of the hill, indicating that the ores extend at least that far down. In addition to the ores *in situ*, a large part of the hillside below the ore outcrops is covered with rubble ore derived from them.

The ore consists for the most part of psilomelane, which occurs in a variety of forms. Most commonly it is simply more or less drusy massive psilomelane as in Fig. 3, but mammillary, botryoidal and concretionary forms are abundant and frequently quite elaborate. The surface of some of these, except for the black color, reminds one of cauliflower. A typical specimen is illustrated by Fig. 4. Associated with the psilomelane is considerable manganite and pyrolusite which occur for the most part as cavity linings and fillings in the former. The manganite occurs lining the cavities both in radiating groups of acicular crys-

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tals and in distinct prismatic crystals. The pyrolusite is often pseudomorphic after manganite showing that it has in part been derived from it. The average composition of the ore as shipped is:

Average Analysis of Morro da Mina Ore

Water at 100° C	2.50 per cent.
Volatile	12.40 chiefly oxygen.
Insol. Residue	3.46
Fe_2O_3–Al_2O_3	8.75 alumina about twice ferric content.
Silica	1.76
P	0.069
S	Absent
Manganese	50.47

It is of interest to compare this average with the following mean of analyses of cargoes of manganese ores landed at Middlesborough, England, during the years 1897 to 1906 as compiled by L. L. Fermor[8] for samples dried at 100° C.:

	India	Russia
Mn	50.86	49.58
Iron	6.31	0.83
Silica	5.71	10.17
P	0.127	0.161
Alumina, etc	6.80	12.77

These figures indicate that the three countries produce very high-grade ore with the advantage in favor of the Brazilian ores, particularly over the Russian, as regards silica and phosphorus.

The ore is mined by hand for the most part in open cuts. A number of tunnels and adits have been driven, but these were intended either for development purposes or to connect the various open cuts to give access to the loading platforms and bins at the railroad. The method in vogue is to strip the orebody of such overburden as it may carry, and then as the ore is mined screen it over iron screens with 0.8-in. square openings. The oversize is the merchantable ore. The screenings constituting 15 per cent. of the crude ore, carry 34 to 35 per cent. Mn and are being stored apart from the waste to be beneficiated at some future time. The ore *in situ* furnishes two-thirds of the present output, and one-third is derived from workings in the rubble ores.

The Original Manganese Rock

Though decomposition of the original manganese rock has extended to considerable depth in places in the Morro da Mina Mine, to at least 130 m. as demonstrated in the exploratory tunnel previously mentioned,

[8] L. L. Fermor: Manganese-Ore Deposits of India. *Memoirs of the Geological Survey of India*, vol. 37, Pt. III, p. 518 (1909).

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portions of the rock have escaped alteration and are well exposed in the mine workings. A large mass of the rock is actually exposed at the surface at the southwest corner of the outcrop of Zona II. It is characterized by a predominance of the silicates, and especially garnet, and for that reason has not succumbed to the processes of weathering. The abrupt transition from this rock to ore is shown in Fig. 5, and it is only the presence of numerous stringers, tongues and patches of manganese oxide penetrating the rock close to the ore that makes clear their genetic relations. More interesting and instructive exposures are found in the 130-m. tunnel. This tunnel crosscuts in a westerly direction the decomposed country rock for 120 m. to Zona I, then cuts diagonally across it in good ore for 150 m., and on emerging penetrates 50 m. of the manganese

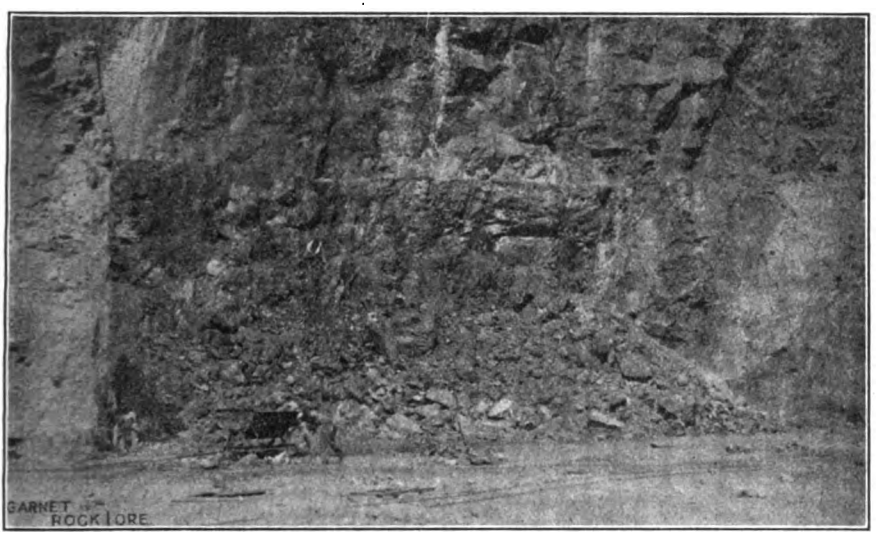

FIG. 5.—SHARP CONTACT BETWEEN THE MANGANESE ORE AND THE ORIGINAL MANGANESE ROCK CHARACTERIZED BY AN ABUNDANCE OF MANGANESE GARNET.

rock to the ore of Zona II. After again cutting 50 m. of ore, a small horse of the manganese rock is encountered with ore on the opposite side. The four contacts of ore and rock exposed in this tunnel showed the same rapid change from one to the other with only a narrow transition zone marked by stringers and tongues of the black manganese oxide penetrating the manganese rock.

Most of this rock has the appearance of a fine-grained dark gray crystalline limestone and is easily scratched with the point of the pick. Its specific gravity, however, is considerably above that of limestone. Here and there are brown patches and streaks with a violet tinge that consist of massive garnet, and there are frequently spots and stringers of pink rhodonite that at once attract attention. A closer examination

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reveals the presence of sufficient light pink silicate in much of the material to give a pinkish tone to its dominant gray color.

Examination of thin sections shows that the essential constituents are manganese carbonate, spessartite, rhodonite and tephroite. Taking the average of all the rock, the manganese carbonate is the most abundant mineral and the tephroite the least abundant. The spessartite is more widespread in its distribution but probably not much in excess of the rhodonite in actual quantity. The relative quantity of the different minerals varies most widely, so that some of the rock consists almost entirely of one of the minerals while some has them present in almost equal quantity, with the exception of the tephroite, which, in the sections examined, was never present in more than subordinate amount. In this respect the rock differs from the Piquery rock described by Derby in which tephroite was prominent and rhodonite a subordinate constituent. Which of the two minerals was formed in a given case depended undoubtedly on the relative amount of silica available, a low silica content giving rise to the formation of the orthosilicate tephroite and a higher silica content to the metasilicate rhodonite.

A thin section of a specimen most closely resembling crystalline limestone consists almost entirely of anhedral grains of manganese carbonate averaging between 0.16 and 0.25 mm. in diameter. That the carbonate is essentially manganese carbonate rather than calcium or magnesium carbonate is shown by the specific gravity of the rock, which is 3.55. The manganese carbonate is swarming with flakes of graphite ranging in size from 0.03 to 0.08 mm. and somewhat less abundantly with small garnets ranging from 0.01 to 0.03 mm. in diameter. In the entire slide there are only a few small areas containing rhodonite or tephroite. The minute garnets in this slide differ from the larger garnets in the more siliceous phases of the rock in being perfectly clear, whereas the latter are characterized by the presence of numerous minute black inclusions such as described by Derby as graphite in the rocks studied by him. Another specimen of more siliceous looking rock with a pinkish tone and streaks of rhodonite shows in thin section manganese carbonate, garnet, rhodonite and tephroite in abundance. The constituents average in size from 0.08 to 0.16 mm. with a few larger crystals of rhodonite. As is generally the case, the garnets are pronouncedly euhedral while the other minerals are anhedral. A specimen of particularly siliceous looking rock having a specific gravity of 3.8 consists for the most part of rhodonite with which is associated considerable euhedral garnet. The rock itself is light brown in color with a distinct pinkish tone. In addition to the silicates there is also an appreciable amount of the carbonate present. The garnets range in size from 0.10 to 0.15 mm. and the rhodonite crystals average about two or three times as large. Other thin sections show essentially similar features.

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The manner in which this rock undergoes alteration to manganese oxides is very interesting. A specimen undergoing oxidation is illustrated

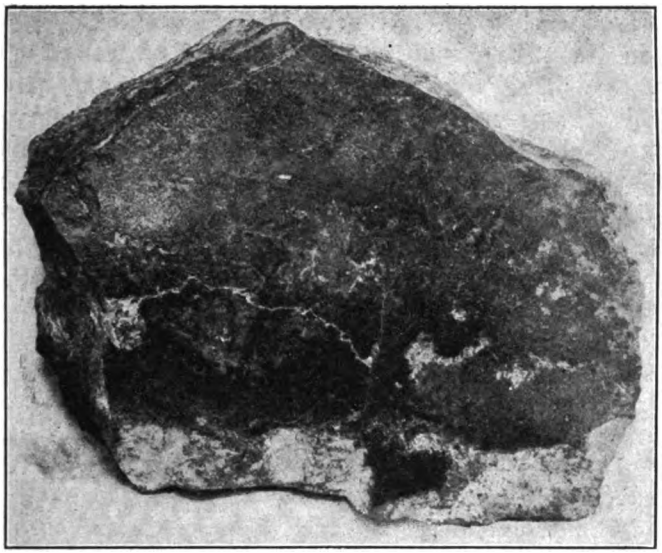

FIG. 6.—POLISHED SURFACE OF THE ORIGINAL MANGANESE ROCK IN PART ALTERED TO OXIDES OF MANGANESE. (Black areas and patches represent the manganese oxides.)

in Fig. 6. As the oxidizing solutions penetrate the rock they first break down the manganese carbonate, tephroite and rhodonite and leave

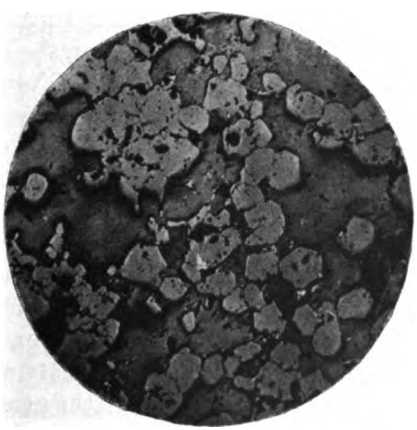

FIG. 7.—RESIDUAL GARNET GRAINS IN A GROUNDMASS OF MANGANESE ORE RESULTING FROM THE OXIDATION OF THE MANGANESE ROCK.

behind embedded in the manganese oxides the grains of garnet. That is, oxidation does not run ahead decomposing the manganese carbonate first

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and then as a more advanced stage tephroite and rhodonite successively, as one might expect; but the three minerals are replaced simultaneously at the very front of the advance of oxidation. A magnified polished surface of rock which has undergone this first stage of alteration is shown in Fig. 7, in which the garnet crystals are seen disseminated in a matrix of manganese oxides. Such material from this mine was described by Derby in his first paper and both minerals considered primary elements of the rock. In his second paper he doubted this first interpretation and was more inclined to regard the manganese oxide a residue of the alteration of original carbonate and silicate minerals with retention of the spessartite. The evidence of the thin sections makes clear that the latter is the true explanation. As this material is further worked on by the oxidizing solutions, the garnet begins to succumb around the periphery of the grains and along fractures until finally it too has been replaced by the manganese oxides.

Genesis of the Lafayette Type of Deposit

The manganese deposits of the Lafayette district inevitably call to one's mind the Indian deposits which likewise consist of manganese oxides associated with manganese silicate rocks, and as we shall have occasion to refer to them in the discussion of their genesis, it will be helpful to summarize at the outset certain of the salient features of the Indian deposits as described by L. L. Fermor.[9] There are two main types of manganese deposits in India, associated respectively with the *kodurite* and *gondite* rocks. The former are found in the Vizagapatam district on the east coast, the latter in the Central Provinces and other parts of India. Typical kodurite is composed of potash feldspar, manganese garnet and apatite, with or without pyroxene. From the mineralogical and chemical composition of the rock and its geologic relations, Fermor concludes that kodurite is an igneous rock. It was subjected to alteration under oxidizing conditions and gave rise to the manganese ores. He finds that the manganese garnet is the most stable mineral and is often left behind in a matrix of psilomelane. To the gondite rocks he gives an entirely different interpretation. He thinks they were originally deposited as sediments and the manganese which they contain as chemical sediments, most probably in the form of oxide, though perhaps to a limited extent as carbonate. These sediments were later subjected to intense dynamo-metamorphism, and where they consisted of relatively pure chemical sediments of manganese were converted into compact psilomelane and braunite. Where alumina and silica had also been deposited, spessartite and rhodonite were formed, and if there was an excess of silica it was crystallized as quartz. The resultant rock is called

[9] *Op. cit.*

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gondite. Typical gondite is a very fine-grained rock consisting of tiny round grains of spessartite set in a mosaic of quartz. Where alumina is deficient rhodonite is found. The ores associated with the gondite rocks, Fermor believes are the result of combined decomposition and replacement of them by waters containing CO_2 and O, but that this action took place at considerable depth and that the carbon dioxide was a portion of that liberated in the metamorphism of the rocks of the region and the oxygen a portion of that liberated in the conversion of the original manganiferous sediments into the manganese silicate rocks. A small proportion of softish and more or less porous ore he attributes to the later surficial alteration of the gondite.

An explanation of the genesis of the Lafayette manganese deposits involves two problems: First, the genesis of the original manganese rock; second, the alteration of it to manganese oxides, that is, to the manganese ores.

As the evidence is beyond question in regard to the second stage in the formation of these ores, that problem will be discussed first. The manner of occurrence of the orebodies, as for instance the change in depth of the Piquery ores into the manganese rock, the abundant evidence of the alteration near the surface of the manganese rock into ore and the microscopic evidence of the same phenomenon, point unmistakably to the derivation of the ores from the manganese rock under conditions of weathering in the zone of oxidation. That is, the ores were formed under conditions similar to those that gave rise to the ores of the kodurite rocks of India and not as Fermor thinks most of the ores in the gondite series were formed. The chemistry of the alteration of the Brazilian manganese rock is simple compared with that of the kodurite. The principal constituents to be removed are the CO_2 of the manganese carbonate and the SiO_2 of the silicates. Examples of the efficacy of meteoric waters for that purpose are so abundant and generally recognized that this part of the process hardly requires elaboration. The universal alteration of iron carbonates to oxides and hydroxides in the zone of oxidation and the enormous amount of silica removed from the iron silicates in the formation of the Lake Superior iron ores are illustrations of the same chemical actions on similar compounds of a chemically closely related element. In the kodurite rocks the removal of considerable alumina was also necessary and the chemistry of this was the only step that Fermor found at all difficult to explain. But even the very slight solubility of alumina in ordinary dilute meteoric waters he thought would suffice for its ultimate removal. This difficulty, however, hardly enters in our problem. Except in the case of the local phases of garnet rock, which has anyhow to a large extent resisted alteration, the alumina content of the original rock is no higher, and the few available analyses would indicate actually a little lower, than that of the ores; and, further-

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more, the total amount involved in either is not over 5 per cent. Consequently the problem of the removal of alumina does not confront us.

The ores have been described as somewhat porous and drusy, but by no means to such an extent as would be called for if they merely represented the residual product of leaching of manganese carbonate and silicates. Nor is there evidence to indicate that their present more compact form is due to shrinkage in volume or compression of the residual manganese oxides; but the evidence both megascopic and microscopic shows that the decomposing solutions deposited manganese oxide, volume for volume for the silica and carbon dioxide removed. Meteoric waters encountering such large quantities of manganese carbonate cer-

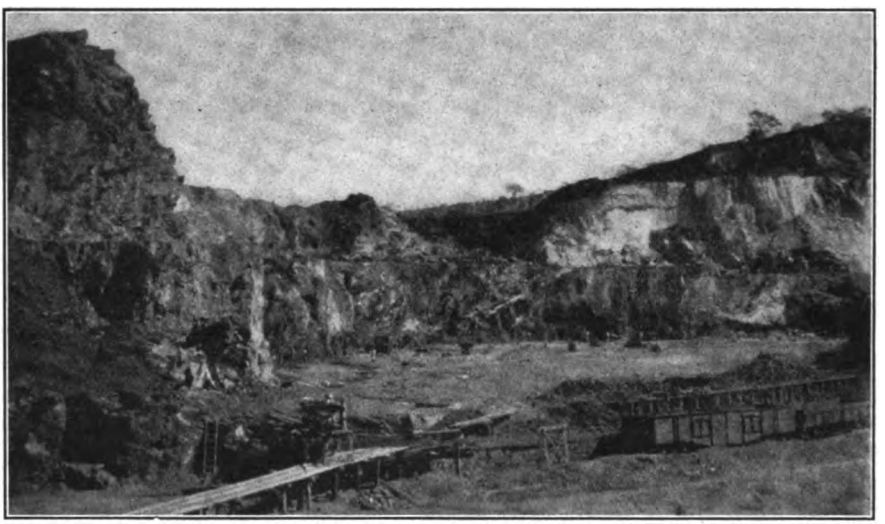

FIG. 8.—VIEW OF SOUTH END OF OPEN CUT OF THE MORRO DA MINA MINE, SAID TO BE THE LARGEST KNOWN DEPOSIT OF HIGH-GRADE MANGANESE ORE.

tainly took a great deal of it into solution as manganese bicarbonate which would be deposited at other points as the oxide; and in this way the deposition of manganese oxide accompanied the leaching of the other constituents of the rock. The alteration of the manganese rock to ore was, therefore, one of simultaneous leaching and addition, resulting in a relatively compact mass of the oxides. The present drusy character of some of the ore is clearly the result of subsequent action of solution and redeposition by meteoric waters upon the manganese oxides formed in this way.

The problem of the origin of the manganese rock is not so easy to solve. One is at once confronted with the fact that our knowledge is rather meager concerning the exact nature of the rocks with which it is associated

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and its geologic relations to them, a full knowledge of which is essential to a final solution. The chemical, mineralogic and petrographic characters of the rock, however, do rule out some explanations, and point strongly to another. Further, the geologic relations that are reasonably well established are not at variance with this explanation but in a measure support it. That is, the sum of all evidence points to an analogy of this rock with the gondite of India. We believe that the rock is the product of dynamo-metamorphism of manganese sediments deposited in the form of manganese carbonate with varying but considerable quantities of silica and varying but smaller quantities of alumina. These sediments differed, therefore, initially from the gondite sediments in averaging much lower in silica, and by the deposition of the manganese in the form of carbonate instead of oxide; and this initial difference in composition accounts for the present difference in mineralogy of the two rocks.

The greater resistance to decomposition of the garnet in the manganese rock at the Piquery mine led Derby in 1901 to the erroneous conclusion that it consisted essentially of manganese garnet and to establish the rock type *queluzite* to which he attributed an igneous origin. Later, in 1908, he found that the garnet rock was but a subordinate phase of an entirely different type of rock which is essentially the same as that which we found abundantly at the Morro da Mina Mine. Attention has been called to the fact that in this later paper he does not apply the term queluzite to this rock and ventures no opinion as to its genesis. Though he did not specifically retract his earlier statement, the presumption is that, in the light of the discovery that the original rock was entirely different from what he had supposed, he abandoned his interpretation of its origin. In the meantime, L. L. Fermor,[10] on the basis of Derby's first paper, drew an analogy between this rock and his kodurite as regards their genesis, though calling attention to their dissimilarity in composition. A persistence of this same view is manifested in the statement by Harder and Chamberlin[11] in their discussion of these ores that "manganese ores associated with igneous rocks, such as those described above, occur abundantly in India—" and the statement by Harder[12] that "The relation of the manganese rock to the enclosing crystalline rocks has not been definitely determined; it may be interlayered with the gneiss or crystalline schist, or perhaps intrusive into them." The chemical and mineralogic composition of this rock as now established at both Piquery and Morro da Mina effectually precludes an igneous origin for it and demonstrates the presence of an original carbonate rock.

Even before Derby's second paper appeared, a view at variance with it

[10] *Op. cit.*, pp. 273–274.
[11] *Loc. cit.*, p. 405.
[12] *Loc. cit.*, p. 790.

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was expressed by his associate, E. Hussak.[13] He regarded the spessartite rock described by Derby together with a banded spessartite-rhodonite rock, which he mentions as occurring on the periphery of the Piquery orebody, the products of contact metamorphism by the eruptive gneiss of impure manganese carbonate sediments. The postulation of the derivation of the silicates from manganese carbonate in advance of the discovery of the presence of large amounts of carbonate is interesting. Beyschlag, Krusch and Vogt[14] are inclined to carry Hussak's ideas a step further and suggest that the manganese may have been contributed as part of the process of contact metamorphism. That this was not the case is proved by the fact that the carbonate still present, constituting a large percentage of the rock, is the manganese carbonate; and that consequently the original carbonate was a manganese carbonate. There remains to decide between the view of Hussak that the rock is the result of contact metamorphism of sedimentary beds of impure manganese carbonate and our own that it is a product of dynamo-metamorphism of such beds.

In the first place, the country rock of the ore deposits is not everywhere gneiss, but is in some cases schist, and these schists are admitted to be metamorphosed sediments. Nor are all of the gneisses necessarily igneous, though it has been generally held that they are such for the most part. Consequently the presumption is against assuming an igneous contact for all of the deposits. In the second place, the texture and appearance of the silicates is not that of typical skarn minerals, or products of contact metamorphism; but it is that of silicates crystallized in a carbonate rock that has been subjected to regional metamorphism. The reply might be made that the whole region has been subjected to dynamo-metamorphism subsequent to the contact metamorphism, but it is equally true that the texture of the rock is not what one would expect to result from a skarn rock subjected to regional metamorphism. On the other hand, we cannot escape the conclusion, demanded by both hypotheses, that there first existed impure manganese carbonate, and it is obvious that the rocks have undergone regional metamorphism. The inevitable result would be a rock such as we have. Our explanation is, therefore, in harmony with the available evidence and makes the manganese rock of the Lafayette district genetically identical with the gondite of India, the mineralogic difference of the two rocks being due to their initial difference in chemical composition.

[13] Eugen Hussak: Ueber Atopit aus den Manganerzgruben von Miguel Burnier, Minas Geraes, Brasilien. *Centralblatt für Mineralogie, Geologie und Paläontologie*, 1905, pp. 240–245.

Eugen Hussak: Über die Manganerzlager Brasiliens. *Zeitschrift für praktische Geologie*, vol. 14, pp. 237–239 (1906).

[14] Beyschlag, Krusch, Vogt: *Die Lagerstätten der nutzbaren Mineralien und Gesteine*, vol. 2, p. 596 (1913).

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Discussion

L. C. Graton, Cambridge, Mass.—I should like to ask the authors if they attempted to draw any analogies with the manganese deposits at Franklin Furnace, N. J., and whether there are any traces of zinc in these Brazilian deposits.

J. T. Singewald, Jr.—We did not think of them at all as analogous to the Franklin Furnace deposits and there is no appreciable percentage of zinc present. If these ores do contain zinc it could be there only as a fraction of a per cent.

F. Lynwood Garrison, Philadelphia, Pa. (communication to the Secretary*).—There is no disputing the fact that the manganese ore supply situation in this country demands serious consideration and is a matter of fundamental public concern. We are consuming this material at a steadily accelerating rate and there is no hope for a proportionate increase in the domestic supply.

In the year 1916, this country imported 560,000 tons and produced only 27,000 tons from our own territory In 1915, the local production was 9,709 tons, which on its face shows the American output to have tripled during the past year. Those of us who are familiar with the character and scope of the manganese deposits in this country, however, know perfectly well that these figures are deceptive and that no such ratio of increase can be expected and maintained. It therefore behooves us to give careful and immediate consideration to the sources of foreign supply now available to us, and at the present moment this means predominantly Brazil as long as the war endures.

As is well known, steel cannot be made in large quantities without the use of ferromanganese alloys, and in view of current political and economic events, it is plain that were our ports blockaded or our ships denied the freedom of the seas, the steel production of this country would necessarily be seriously affected if not practically paralyzed, for, unlike Germany before the war, it does not seem that we have accumulated any considerable reserve of manganese ores and, as I have indicated, our domestic yield last year was about 4½ per cent. of the consumption.

Prior to the war, we imported large quantities of ferromanganese from Europe. Inasmuch as both England and Germany have practically no domestic supply of manganese ores, like ourselves they had to depend upon importations chiefly from India and Russia. It is evident, therefore, that the situation in England and France is serious and in Germany must be desperate, for it is known that her reserves of manganese ore have long since been consumed and, failing the discovery of some suitable substitute as a deoxidizer in the process of steel manufacture, that

* Received Feb. 26 1917.

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country today must be face to face with a problem of the utmost gravity in the consequently necessitated restriction of her steel production. Naturally we hear very little about the matter, for it is a technical subject beyond the ken of the general public in all countries. Most people assume that given an unlimited supply of iron ore and coal, a corresponding production of steel is simply a question of apparatus to make it.

The United States and Canada have been combed over pretty thoroughly in the hope of finding manganese deposits of sufficient magnitude to meet our needs, but so far with uniformly disappointing results. Plenty of manganiferous iron ore has been found in the Southern States and some superficial, but never rich, manganese ore deposits in residual clays. We have nothing in this country similar to the great deposits in Minas Geraes of the Lafayette type, although there is some resemblance between our Appalachian deposits in the Potsdam sandstone or quartzite and those of the Miguel-Burnier district which are also associated with quartzites. The latter, however, are infinitely larger, although they also are evidently superficial, but nevertheless by virtue of their size and extent must contain large bodies of good, merchantable ore.

Seemingly we have no cause to hope that there will ever be found in this country any manganese deposits comparable to those of India, Russia and Brazil. Since the war, the demand for Brazilian ore has increased in leaps and bounds, and were the handling and shipping facilities at Rio de Janeiro less archaic and primitive, the output might be greatly enlarged.

Rio de Janeiro is the only Brazilian port from which manganese ores are being shipped. About 9 or 10 years ago, attempts were made to mine and ship manganese ores from Bahia, and some small tonnage was exported, but the enterprise culminated in failure.

The existing productive districts of Brazil are today those and only those mentioned in the paper of Singewald and Miller and although quite distinct geologically they are very near together geographically. There is abundant reason to believe that the productive manganese area of this enormous country may be considerably enlarged as the result of well-directed and sustained exploration. But as yet little intelligent and systematic work of this kind has been attempted, although manganese ores have been discovered in numerous localities and the geologic data we already possess indicate the existence of many more yet to be found.

At present the Lafayette type of deposits appears to hold promise of larger ore reserves than those of the Miguel-Burnier class. From what is known of the genesis of the ores in both of these districts, it might seem that in neither case can much persistence in depth be expected of the orebodies. This would appear to be especially so with the deposits associated with and intercalated in sedimentary strata.

The genesis of the ores in the Lafayette district, as described by

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Singewald and Miller, follows rather closely the hypothesis laid down by Derby some 20 years ago in which he called attention to the important role limestone and calcium compounds seemingly play in the origin of the Lafayette (Queluz) ores, as well as those in the sedimentary rocks.

The intimate relations between iron, manganese and limestone beds has long been recognized. The affinity of these two metals for certain quartzose rocks such as the itabirites of Brazil, the Potsdam sandstone of the Appalachian region and the quartzites of the Dharwar series in India, is equally noteworthy. But the suggestion made by Derby as quoted by the authors on page 12, to the effect that the orebodies of the Lafayette type present strong analogies to certain ore types in magmatic segregations, imports a new idea into the problem which it seems to me ought to receive careful consideration.

Before discussing this subject in detail, there are a few general facts connected with it which it might be well to review. Years ago Penrose pointed out that pre-Paleozoic rocks, especially the Archean, have played a far more important role as a source of manganese than the later igneous rocks.[1]

The sedimentary and residual ores of iron are fully paralleled by those of manganese, the gossan iron ores being the only ones having no true manganese equivalent.

The simultaneous leaching of iron and manganese from the rocks by the same solvents in much the same way is evident.

It is also plain that although these metals are redeposited under similar conditions, the precipitation takes place more or less separately. All manganese ores contain some iron, and most iron ores carry manganese, and it is important, as a practical matter, for us to understand, as far as it is possible, the chemical reactions involved in the question.

The carbonates of iron and manganese are isomorphous and are precipitated as such together. Resulting oxidation leaves the two metals associated in their relative proportions. The oxides of these metals, on the contrary, are not isomorphus and may be precipitated together only in mechanical association, as in bog ores. When iron and manganese ores occur in more or less separate deposits, different influences must have been at work from those that have thrown them down together.

According to Fresenius, the iron is precipitated first as ferric hydroxide, the manganese remaining in solution as a bicarbonate to be finally thrown down in a calcareous sinter, or, in other words, solutions of manganese carbonate are more stable than similar solutions of ferrous carbonate, and in consequence the manganese salt is carried or migrates farther, thus effecting at least a partial separation of the two metals from the same solution.

The thermo-chemical observations of Dieulafait are in accord with

[1] *Annual Report of the Arkansas Geological Survey* (1890), 544.

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the above deductions and rest upon the principle that when several reactions may conceivably take place in the same solution, *the one attended by the greatest evolution of heat will occur.* The thermo-chemical equations of Dieulafait are important and in brief are as follows:

$$2FeO + O = Fe_2O_3 + 26.6 \text{ Cal.}$$
$$2MnO + 2O = 2MnO_2 + 21.4 \text{ Cal.}$$

If oxygen acts on a mixture of FeO and MnO, or upon substances equivalent to them, ferric oxide will form *first* and be the more stable.

$$FeO + CO_2 = FeCO_3 + 5.0 \text{ Cal.}$$
$$MnO + CO_2 = MnCO_3 + 6.8 \text{ Cal.}$$

When carbon dioxide unites with these oxides, then *the manganese compound will form first and be the more stable.* If oxygen and carbon dioxide act together in considerable excess, Fe_2O_3 and MnO_2 will both be formed; but if they act slowly in small quantities, the oxygen will go to produce Fe_2O_3, and $MnCO_3$ can be generated at the same time. Manganese carbonate, being somewhat soluble, may then be separated from the ferric oxide by leaching, either to be deposited as carbonate or perhaps to be later oxidized to MnO_2 and CO_2.[2]

Dieulafait gives the heat of formation of several manganese compounds as follows:

$$Mn + S = 22.6 \text{ Cal.}$$
$$Mn + O = 47.4 \text{ Cal.}$$
$$Mn + O + CO_2 = 54.2 \text{ Cal.}$$
$$Mn + O_2 = 58.1 \text{ Cal.}$$

Hence it appears that since the dioxide produces the most heat, it is also the most stable compound of the series and being the easiest formed becomes the principal manganese ore. Thus thermo-chemical and geological hypotheses are in harmony.

During a recent visit to Brazil, my attention was called to a manganese rock associated with granites and gneisses in a section not heretofore explored and many miles distant from the Lafayette district. On analysis this rock gave the following composition:

	Per Cent.
SiO_2	36.04
Al_2O_3	11.01
Fe_2O_3	9.90
MnO_2	37.00
Phosphorus	0.068

It will be noted that this material approximates closely in composition to that indicated in III, on page 13, and to typical spessartite which according to Rammelsberg contains 36.16 per cent. Al_2O_3 and 32.18 per cent. MnO.

[2] The Data of Geochemistry, *Bulletin of the U. S. Geological Survey* (1911), **491**, 511.

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In the case of the ores of the Lafayette type, it is possible there may be reason to assume the manganese to be a replacement of silica rather than of lime. I have myself seen in other parts of Brazil large bodies of manganese ores associated with shales and quartzites similar to the Miguel-Burnier deposits, but no limestones, although, of course, future development may show their presence or that of calcareous shales.

In personally discussing this subject with J. C. Branner, whose work on Brazilian geology stands second only to that of Derby, he referred to an instance where he broke what seemed to be a solid lump of manganese and found that it was a fragment of itacolumite covered with a crust of psilomelane. Under the microscope a section of this core rock showed that the silica is being replaced by a brownish mineral of undetermined character which in turn was being replaced by solid psilomelane. Dr. Branner, therefore, concludes that: "in this case the ore is clearly a replacement of silica by manganese brought in in solution."

Now as to the magmatic segregation hypothesis suggested by Derby to account for the origin of certain Brazilian manganese ores: It may be recorded that a manganese rock similar to that whose analysis is given on page 13, has been found in the neighborhood of chromic iron-ore deposits in areas composed almost wholly of granites and gneiss, which are probably Archean.

Chrome ores are almost invariably found in serpentine rock and usually serpentines have been formed by the alteration of basic igneous rocks, chiefly pyroxenites and peridotites.

I have never heard of any chrome ores having been found in Minas Geraes, but serpentine seems to be a not uncommon rock in the manganese as well as in the iron districts.

Goodchild[3] in his paper on the subject of "Laterization in Minas Geraes" speaks of serpentine boulders occurring in the gorges between the canga flats to the south of Caraca having the following composition:

	Per Cent.
SiO_2	39.30
Fe_2O_3	9.36
Al_2O_3	5.94
Mn_3O_4	0.65
CaO	2.25
MgO	30.71

This rock corresponds rather closely to the typical peridotite derivatives given by Kemp in his *Handbook of Rocks*, page 140. Goodchild (page 15) refers to it as outcropping in the low country at the base of the Caraca mountain in the gorges between the canga flats.

Shearer[4] in the discussion of this paper of Goodchild, says of "the

[3] *Transactions of the Institution of Mining and Metallurgy* (1914), **23**, 14.
[4] *Ibid.*, 31.

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serpentine rock found in the valleys on the Morro da Mina property southeast of the Serra do Caraca", the analysis of which is above quoted, that it "appears to be an altered basic lava flow overlying the iron formation and is the only late eruptive rock in the district and covers only a few square kilometers out of hundreds, so it cannot be supposed to have any genetic significance."

I am disposed to question this remark of Shearer's, for, on the contrary, I believe the existence of serpentines among the old rocks of central Brazil is an important matter. And in this connection I will venture to quote a remark upon the subject which Branner made in a recent letter to me. "A person well posted on the geology of Brazil sits up and takes notice whenever serpentine is mentioned, for it is supposed to have some genetic relation to the diamonds. I have only seen it in one place in Brazil, and if you know of occurrences, my suggestion is that you look it over carefully."

At the present time I am not at liberty to discuss this subject at length, save to say that I have myself found serpentines in several different localities in Brazil associated with granites and gneisses, and in every instance accompanied by minerals of economic importance.

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Manganese Ores of Russia, India, Brazil and Chile*

BY E. C. HARDER,† WASHINGTON, D. C.

(New York Meeting, February, 1917)

INTRODUCTION

THE situation in the United States, at the present time, regarding the supply of manganese ores and alloys of manganese is one of great seriousness and is likely to become increasingly so while the European war continues.

The two principal iron-manganese alloys, ferromanganese and spiegeleisen, are indispensable in steel manufacture, being used extensively in the Bessemer and open-hearth processes. Spiegeleisen, the lower alloy, contains between 15 and 25 per cent., and ferromanganese, the higher alloy, between 60 and 80 per cent., of manganese. Besides manganese the alloys contain iron and carbon. On the market ferromanganese is usually quoted on the basis of 80 per cent. manganese, and spiegeleisen on the basis of 20 per cent. manganese. The consumption of ferromanganese has been increasing during recent years at the expense of spiegeleisen, because larger quantities of the latter must be used to accomplish the same purpose with the result that the amount of carbon introduced with it into the molten metal is frequently injuriously great.

Manganese ore proper, in the form of manganese dioxide, is used extensively in many chemical industries, especially for oxidizing purposes. It is also used for the manufacture of both wet and dry electric cells and in the manufacture of paints and dyes.

The manganese ore used in this country has been derived hitherto from three principal sources: Southern Russia, Southern British India and East Central Brazil. The following tables show the quantity of manganese ore imported from these countries, and (for comparison) the domestic production, during 1912, 1913 and 1914.

The imported manganese ore has been used largely in the manufacture of iron-manganese alloys and in addition large quantities of the alloys themselves have been imported, chiefly from Great Britain, Germany and other European countries.

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TABLE I.—*Imports of Manganese Ore into the United States, Calendar Years* 1912 *to* 1914, *by Countries, in Long Tons.*[a]

	1912		1913		1914	
	Quantity	Value	Quantity	Value	Quantity	Value
British India.....	128,645	$644,162	141,587	$710,024	103,583	$504,981
Russia.....	83,334	490,306	124,337	712,324	52,681	490,339
Brazil.....	81,580	510,300	70,200	445,680	113,924	736,769
Other countries.....	7,102	124,416	8,966	161,652	13,106	292,031
Total.....	300,661	$1,769,184	345,090	$2,029,680	283,294	$2,024,120

TABLE II.—*Production of Manganese Ore in the United States,* 1912 *to* 1914, *by States, in Long Tons.*[a]

	1912		1913		1914	
	Quantity	Value	Quantity	Value	Quantity	Value
California.....						
South Carolina.....	.....	.....	.....	.....	911[d]	$8,812
Virginia.....	1,664[b]	$15,723	4,048[c]	$40,480	1,724	18,565
Total.....	1,664	$15,723	4,048	$40,480	2,635	$27,377

[a] D. F. Hewett: Production of Manganese and Manganiferous Ores in 1914. *Mineral Resources of U. S.*, 1914, pp. 170 to 173.

[b] Includes South Carolina and California.

[c] Includes small production in 1911 and 1912.

[d] Includes California.

The following table shows the production of iron-manganese alloys in this country during 1912, 1913 and 1914, and the quantity imported during the same period.

Table III.—Production and Imports of Iron-Manganese Alloys in the United States, 1912 *to* 1914, *in Long Tons.*[a]

	1912		1913		1914	
Production.....	227,939	.....	226,475	.....	177,356	
Imports.....	100,152	$3,935,014	128,147	$5,685,088	85,867	$3,690,754

[a] D. F. Hewett: *Loc. cit.*, pp. 178 to 179.

These tables indicate forcibly the dependence of the United States upon foreign countries for its supply of manganese ores and iron-manganese alloys.

Since the beginning of the present war the shipment of manganese

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ores and alloys from the belligerent nations and their colonies has been highly uncertain. The manganese ores from British India have gone largely to England, and exports to other countries have been curtailed. The exportation of Russian manganese ore has practically ceased since the entrance of Turkey into the war and the closing of the Dardanelles. The only one of the three important manganese-ore producing countries, therefore, whose shipments to this country have continued uninterruptedly is Brazil. Fortunately, the shipments from Brazil have increased considerably during the last year, although they are far from sufficient to make up the deficiency in shipments from Russia and India.

The shipment of manganese alloys from foreign countries has also been curtailed. England and Germany were the principal shippers of these alloys to the United States. At various times during 1915 the exportation of ferromanganese from England was stopped completely by embargo, and imports from Germany ceased, of course, when the war began. At present, however, England is permitting the shipment to this country of some ferromanganese, intended mainly for consumption in the manufacture of munitions of war.

It will be seen from the preceding tables that already in 1914 there was a great decrease in imports both of ores and alloys of manganese. While definite statistics are not yet available, it is known that the decrease in 1915 has been even greater than that in 1914 and the result is a marked increase in the price of these commodities.

During the summer of 1914, when commercial conditions were fairly normal, ferromanganese ranged in the neighborhood of $37 and $38 per ton on the basis of 80 per cent. manganese. During the latter part of August, after the war had begun, prices immediately rose and ranged from $100 to $140 per ton, although market quotations for future delivery did not go above $100. During the latter part of September and early part of October the quotations declined by degrees from $100 to $68 per ton and remained at this nominal figure until the middle of March, 1915. Small lots for immediate delivery, however, generally brought between $80 and $100 per ton during this time. In March the quotations rose to $78, and in May to $100 per ton, which figure was retained for imported ferromanganese up to the end of the year. Domestic ferromanganese, however, was quoted in September at $115 and by the end of the year at $105 per ton.

The price of manganese ores[1] depends on their purity, their mineralogical nature and their physical character. For high-grade ores used in chemical industries, in the manufacture of electric batteries and for other purposes, the prices fluctuate greatly, ranging from $20 to $100 per ton. The prices paid for medium-grade ores used in the metallurgical industries, however, are relatively stable. Their value is governed largely by specifi-

[1] *Mineral Resources of United States*, 1914, p. 176.

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cations announced from time to time by the Carnegie Steel Co., Pittsburgh, Pa., and varies with the content of manganese, phosphorus and silica. The price of these ores generally ranges between $8 and $13.50 per ton.

In the following pages I have attempted to get together and put into compact form the available information concerning the manganese deposits of the three principal producing countries, Russia, India and Brazil, which have yielded in recent years about 90 per cent. of the world's manganese-ore production, as is shown in the following table.[2]

TABLE IV.—*Production of Manganese Ore in Principal Producing Countries, 1909 to 1914, in Long Tons*

Country	1909	1910	1911	1912	1913	1914
North America:						
Canada	.....	.....	5	67	.....	25
Nova Scotia	(a)	22	134	208	(b)	(b)
United States	1,544	2,258	2,457	1,664	4,048	2,635
South America:						
Brazil[c]	236,982	249,954	171,172	152,431	180,738	(b)
Europe:						
Austria	17,761	15,447	15,703	12,275	16,280	(b)
Belgium	6,171	.....	.....	.....	.....	(b)
Bosnia and Herzegovina	5,602	3,937	3,543	4,577	4,626	(b)
France	9,252	7,874	5,906	5,488	7,610	(b)
German Empire	75,962	79,291	85,921	(d)	(b)	(b)
Greece	5,289	40	721	7,595	547	(b)
Hungary	11,733	13,061	14,523	10,662	18,706	(b)
Italy	4,626	4,134	3,460	2,599	1,596	(b)
Russia	726,978	719,907	e572,028	e911,742	1,289,370	(b)
Spain	7,703	8,471	5,519	17,126	21,254	(b)
Sweden	5,189	5,719	5,347	5,082	3,938	(b)
Turkey	f7,459	g12,008	(b)	(b)	(b)	(b)
United Kingdom	2,768	5,467	4,987	4,170	5,393	3,439
Asia:						
China	1,500	(b)	(b)	(b)	(b)	(b)
India	642,675	800,907	670,290	637,444	718,520	(b)
Japan	8,708	11,121	9,615	11,862	(b)	(b)
Philippine Islands	(h)	.....	.....	.....	.....	(b)
Portuguese India	(i)	(i)	(i)	(i)	(b)	(b)
Africa:						
Cape Colony	.....	(j)	c116	.....	(b)	(b)
Natal	.....	51	.....	.....	(b)	(b)
Oceania:						
Australia	603	815	1,151	328	27	6
New Zealand	6	5	.....	.....	.....	(b)

a Not stated.
b Statistics not yet available.
c Exports.
d Statistics not available for Empire. Prussia produced 91,018 long tons.
e Exported from Batum and Poti.
f Year ending March, 1909.
g Exported from Stratoni.
h Quantity not stated. Value of production, $6,250.
i Quantity not stated. Exports valued at $13,607 in 1909, $18,609 in 1910, $13,266 in 1911, and $779 in 1912.
j Quantity not stated. Exports valued at $7,786.

[2] *Mineral Resources of the United States*, 1914, p. 173.

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Wherever possible the figures have been taken from the official publications of the country concerned, which is the case with Canada, Belgium, France, German Empire, Italy, and Spain; otherwise, *Mines and Quarries*, London, has been used except for 1913, when figures for all countries except Italy and the United Kingdom are from *Mineral Industry*, New York.

Besides Russia, India and Brazil, there are several other countries that contain manganese-ore deposits of importance, chief among which are Spain, France, Germany, Austro-Hungary, Cuba and Chile. I have added a description of the Chilean deposits because Chile has produced considerable quantities of manganese ore in the past and may do so again in the future. Important deposits of both rich and medium-grade ores undoubtedly occur. Adequate descriptions of the Chilean deposits have never to my knowledge appeared in the literature.

I have not attempted to describe the manganese deposits of the United States because they have played only a very unimportant part in the manganese industry of the world. Besides, a fairly complete account of these ores has appeared only recently in a bulletin of the United States Geolgical Survey.[3]

Russia

General Statement

The manganese ores mined in Russia have come largely from the Tchiatouri (Chiaturi) deposits situated in the central part of the Caucasus chain of mountains (see Fig. 1). Small amounts have come from the Nikopol region in southern Russia north of the Black Sea and from the central part of the Urals. The following table shows the quantities of ore which have been produced annually in the different districts. For 1906 to 1910 the figures for separate districts have not been obtainable.

It will be seen from this table that by far the largest quantity of ore mined has come from the Tchiatouri district although considerable ore has been produced in the Nikopol district as well. Brief descriptions of these two districts are given in the following pages.

Tchiatouri District

The Tchiatouri deposits,[4] in the province of Kutais on the southern slope of the Caucasus, are reached by a 21-mile narrow-gage spur from the main line of the Trans-Caucasian railway, which runs from Poti and Batum on the Black Sea to Baku on the Caspian. The spur leaves the

[3] E. C. Harder: Manganese Deposits of the United States. *Bulletin* 427, *U. S. Geological Survey* (1910).

[4] Frank Drake: The Manganese-Ore Industry of the Caucasus, *Trans.*, vol. 28, 191 to 208 (1898).

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TABLE V.—*Production of Manganese Ores in Russia*

	Tchiatouri. Long Tons	Ural. Long Tons	Nikopol. Long Tons	Total. Long Tons
1879[a]	871	.....	.....	871
1880[b]	9,910	.....	.....	9,910
1881[c]	11,048	.....	.....	11,048
1882	12,287	1,900	.....	14,187
1883	15,700	1,063	.....	16,763
1884	20,338	1,422	.....	21,760
1885[d]	58,559	882	.....	59,441
1886	67,985	809	3,556	72,350
1887	51,890	827	3,650	56,367
1888	29,400	1,334	1,445	32,179
1889	68,439	2,888	5,508	76,835
1890	168,840	2,314	8,517	179,671
1891	98,371	1,906	10,651	110,928
1892	165,101	908	28,953	194,962
1893	166,419	3,001	76,455	245,875
1894	179,315	1,748	57,454	238,517
1895	116,268	2,713	36,881	155,862
1896	156,552	4,024	44,884	205,460
1897	197,287	4,884	55,115	255,673
1898	262,245	6,391	58,781	327,353
1899	549,233	1,864	95,400	646,497
1900	651,024	2,820	87,224	741,068
1901	369,428	3,479	68,443	441,350
1902	418,147	6,058	56,515	480,720
1903	370,558	4,629	33,734	408,921
1904	350,182	4,839[f]	59,566	414,587
1905	307,008	4,650[f]	115,155	426,813
1906[e]	.....	.....	.....	1,002,912
1907	.....	.....	.....	987,722
1908	.....	.....	.....	356,597
1909	.....	.....	.....	726,978
1910	.....	.....	.....	719,907
Total.....	.....	.....	.....	9,740,084

[a] F. Drake: *Op. cit.*, p. 191 (Statistics for 1879).

[b] *Mineral Resources of United States*, 1886, p. 205 (Statistics for 1880).

[c] *Mineral Resources of United States*, 1894, 16*th Annual Report, U. S. Geological Survey*, pt. III, p. 444 (Statistics for 1881–1884).

[d] *Mineral Resources of United States*, 1905, p. 105 (Statistics for 1885–1905. Changed from poods to long tons.)

[e] Reports of British Home Office. *Mines and Quarries*, 1907–1912. (Statistics for 1906–1910. Changed from metric tons to long tons.)

[f] By difference.

main line at the station of Kvrilli near Sharopan, and ascends the valley of Kvrilli River to the town of Tchiatouri located in the valley bottom below the mines. Most of the ore is transported by railroad to Poti or Batum and thence by steamers through the Dardanelles to various

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points of consumption. Much of the ore that goes to the United States is trans-shipped in England and Germany. The distance from the mines to Poti is about 90 miles[5] and to Batum about 126 miles.[6] The cost of manganese ores delivered at these ports is as follows:

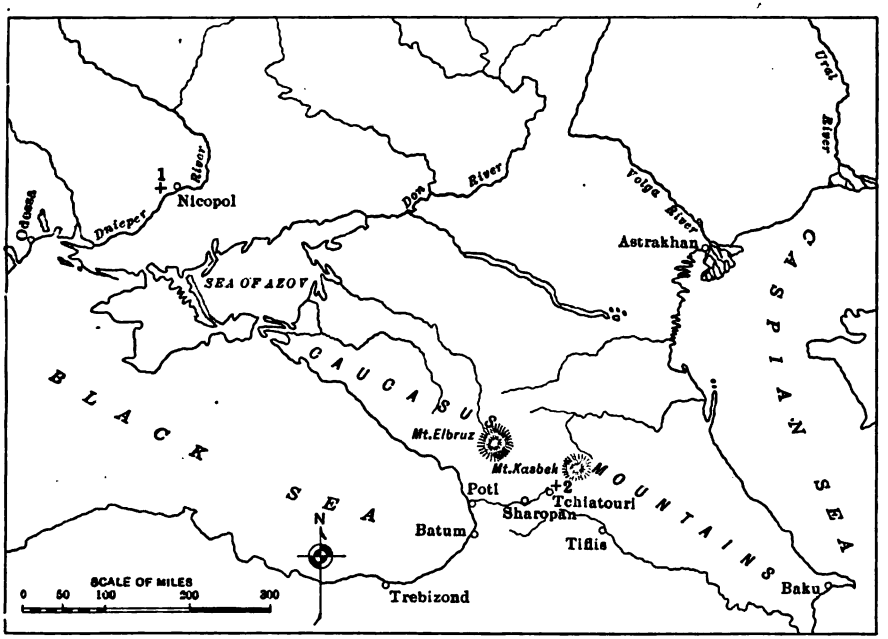

FIG. 1.—MAP OF SOUTHEASTERN RUSSIA SHOWING THE LOCATION OF THE MANGANESE-ORE DISTRICTS.

1. Nikopol. 2. Tchiatouri.

	Delivered at Poti	Delivered at Batum
Mining	$1.45	$1.45
Transportation to Tchiatouri	0.83	0.83
Transportation to Sharopan	1.77	1.77
Trans-shipment and loading	0.40	0.40
Transportation to Poti	0.52	....
Transportation to Batum	....	0.84
Port charges, taxes, etc.	0.72	
Port charges	....	0.46 (?)
	5.69	5.75

The mines are situated along the edge of a high dissected plateau, being on the average about 1,000 ft. vertically above the valley bottom

[5] E. de Hautpick: The Russian Manganese Industry and Its Regulation, *Mining Journal*, vol. 90, p. 932 (July 23, 1910); The Last Gasp of the Russian (Chiaturi) Manganese Industry, *Mining Journal*, vol. 91, p. 1310 (Nov. 12, 1910); Russian (Chiaturi) Manganese Industry, *Mining Journal*, vol. 92 (Mar. 11, 1911).

[6] F. W. Cauldwell: Manganese Ore in the Caucasus, *Daily Consular and Trade Reports*, Department of Commerce and Labor, May 7, 1912.

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of the Kvrilli river or its branches. The distances from the various mines to the station range from 1.3 miles to 3.3 miles; and, as the roads are very bad, the ore is transported by means of ox-carts or pack-horses, at a cost varying from $0.636 to $0.958 per ton.

The Tchiatouri deposits are said to have been discovered in 1848, but the first shipments were made in 1879.

The rocks of the region are Cretaceous and Tertiary[7] sediments, showing great regularity of bedding and having a slight dip of 2.5° to the northeast. They are well exposed in the high cliffs along the Kvrilli river. At the base are Cretaceous (Senon) chalk and shale, and these are overlain by Eocene sandstone and associated sediments which in turn are overlain by Oligocene and Miocene sandstone, shale and chalk.

The ores are clearly of sedimentary origin. They occur at the base of the Eocene in a horizontal bed with a slight northeasterly dip and are interlayered with sediments having the same attitude. Above the manganese-ore bed is brown sandstone of Eocene age; below it is Cretaceous chalk.[8] The ore and associated rocks are in a plateau region on the west slope of the Lesser Caucasus. Where the deposits occur, the Kvrilli River and its small branches have cut a network of deep canyons into the plateau, exposing the rocks along the sides. The ore bed is found on the upper slopes of the canyon walls both north and south of the main valley of the Kvrilli, and from 700 to 1,050 ft. above it. The side branches of the river have cut the plateau into a number of separate sections[9] of which four (Rgani, Sedergani, Mguemevi and Darkveti) are north, and three (Perevissi, Choucrouti and Itkhrvisi) are south, of the river. The ore bed outcrops along the rims of these plateaus, not far below the top. The richest ore has been found on the plateaus of Mguemevi, Sedergani and Choucrouti. Rgani has yielded rich wash ore. The total area in which mines occur is about 55 sq. miles, of which 22 sq. miles[10] are probably underlain by good ore. The ore bed contains intercalated layers of sandstone, or of loose, friable, calcareous and arenaceous rock, which vary in thickness from a small fraction of an inch to 10 in. or a foot. The average thickness of the entire manganese bed is 6 or 7 ft.;[10] but that of the ore is not much more than about 3 ft.[11] There are, however, no abrupt or extreme variations in thickness. The Tchiatouri deposits are estimated to contain about 110,000,000 tons (metric) of manganese ore.[12]

[7] Beyschlag-Krusch-Vogt: *Lagerstätten der nutzbaren Mineralien und Gesteine*, Bd. II, p. 592.

[8] Stelzner and Bergeat: *Die Erzlagerstätten*, p. 258.

[9] F. W. Cauldwell: *Op. cit.*

[10] Frank Drake: *Op. cit.*

[11] E. Schnass: Eine Studinereise durch den Kaukasus, *Glückauf*, Jahrg. 50, p. 921.

[12] Beyschlag-Krusch-Vogt: *Op. cit.*

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The deposit is distinctly stratified and is composed mainly of pyrolusite though psilomelane and wad occur in places. The ore is concretionary and oolitic, and in many places shows botryoidal surfaces and concentric structure, the concretions and oolites being imbedded in a matrix of soft powdery ore. Oolitic granules of manganese oxide also occur in the sandstone.

The ore contains in some places, without sorting or cleaning, from 48 to 52 per cent. of metallic manganese; and the average percentage is from 40 to 45.[13] Concentrates of exceptional purity, prepared by washing and used in chemical industries, contain from 80 to 90 per cent. manganese dioxide. As a general rule, the exported ore which has undergone sorting averages about 51 or 52 per cent. of manganese. Where the barren material has been thoroughly separated concentrates may carry as high as 61 per cent. manganese. Phosphorus averages about 0.16 per cent. and silica not above 8 per cent.

The physical characteristics of the Tchiatouri manganese ore are unfavorable in that the proportion of large pieces obtained is small, and much of the ore is quite soft, and grinds to a fine powder during the handling incidental to mining, cleaning and transportation. The loss of ore during concentration and transport is therefore considerable; and much of the fine ore shipped is considered objectionable by consumers.

The mines, owned by a large number of Georgian peasants, who are without capital or knowledge of mining,[14] have been worked until recently in the most primitive manner. Galleries were driven from the hillside into the manganese bed at short distances apart along the outcrop, and cross-tunnels branched from these in all directions. The ore was generally hauled from the tunnels in wheelbarrows or baskets. In a few places primitive mine cars were used. As a result of such methods only about two-thirds of the ore has been recovered.

Within the last eight or ten years, however, foreign firms have bought up some of the larger plots and have started mining by more modern methods. Much of the unworked land also is now in the hands of foreign capitalists, many of them German.

Nikopol District

The manganese-ore deposits of this district occur in the vicinity of Nikopol on the Dnieper River, about 100 miles from its entrance into the Gulf of Odessa. They were first worked in 1886, and have shown a steady and generally increasing production.

The deposits are horizontal beds in Oligocene rocks.[15] Glauconitic

[13] Frank Drake: *Op cit.*

[14] F. W. Cauldwell: *Op. cit.*

[15] Beyschlag-Krusch-Vogt: *Op. cit.*, p. 593.

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clay and sand occur in both hanging and foot walls, the latter being usually more sandy. Immediately underneath the Oligocene strata and not far below the ore bed, are granite and gneiss. At one point, near Horodizce, 18 km. north of Nikopol, the manganese bed is separated from the granite and gneiss basement by a bed of clay and sand only about a foot thick. The area underlain by manganese ore is said to be about 20 sq. km. and the beds are said to have an average thickness of from 1 to 1.5 m. The total tonnage of ore available is estimated at about 7,400,000 tons (metric).

The ore-bearing layer consists of sandy clay, much stained by manganese, which contains nodules of psilomelane and pyrolusite[16] with concentric or cellular structure. The better class of ore contains about 57 per cent. manganese.

India

General Statement

Manganese ores occur in India at the following localities:[17]

Central India.—Gwalior, Bhopal, Jhabua, Dhar and Indore.

Bombay.—Narukot, Panch Mahâls, Sátára, Bijapur, Belgaum, Ratnagiri, North Kanara and Dhárwar (Sangli State).

Central Provinces.—Hoshangábád, Nimár, Jabalpur, Chhindwára, Bálághát, Bhandára, Nágpur and Wun.

Haidarabad.—Bidar.

Madras.—Bellary (Sandur Hills), Ganjam, Nilgiri Hills and Vizagapatam.

Mysore.—Chitaldrug, Shimoga, Kadur and Tumkur.

Bengal.—Kálahandi, Gangpur and Singhbhum.

Burma.—Mergui.

Goa (Portuguese).—Bicholim.

Practically all the important Indian manganese-ore deposits occur in rocks of pre-Paleozoic age, which are classified as follows.

Pre-Paleozoic Rocks

Purana.—Unfossiliferous sediments, such as quartzite, sandstone, slate, shale, conglomerate and limestone. Here are included the Vindhyan series, Bijáwar series, Kadapah series and Kaládgi series.

Archean.—These rocks comprise:

1. Plutonic intrusives, such as Bundelkhand granite, banded granite and gneissose granite.

2. The Dhárwar sediments: (*a*) Less metamorphosed: Conglomer-

[16] Stelzner and Bergeat: *Op. cit.*, p. 259.

[17] L. L. Fermor: The Manganese-Ore Deposits of India, *Memoirs, Geological Survey of India*, vol. 37, pts. 1–4.

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ate, grit, phyllite, slate, quartzite, fine-grained limestone, and banded hematite and magnetite-jaspers. Here are included the Arávalli series, Chilpi Ghat series and Champáner series. (*b*) Strongly metamorphosed and recrystallized: Schistose gneiss, quartzite, quartzite-schist, mica-

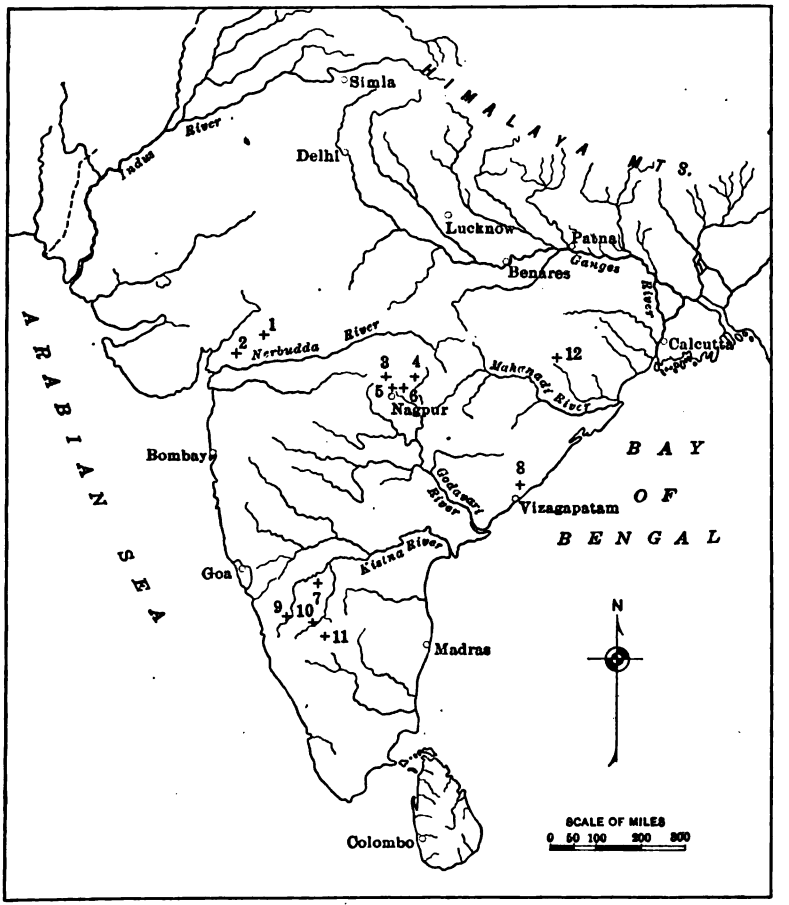

FIG. 2.—MAP OF THE PENINSULA OF INDIA SHOWING THE LOCATION OF THE PRINCIPAL MANGANESE-ORE DEPOSITS. (Adapted from a map by Fermor.)

1. Jhabua.
2. Panch Mahâls.
3. Chhindwára.
4. Bhandára.
5. Nágpur.
6. Bálághát.
7. Sandur.
8. Vizagapatam.
9. Shimoga.
10. Chitaldrug.
11. Tumkur.
12. Gangpur.

schist and crystalline limestone. Here are included also the gondite rocks.

3. The oldest gneisses, such as the Bengal gneiss, and metamorphosed sediments and igneous rocks, such as the charnockite, khondalite and kodurite series.

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TABLE VI.—*Annual Production of Manganese Ore in India in Long Tons*[18]

	1892	1893	1894	1895	1896	1897	1898	1899	1900	1901	1902	1903
Bengal:												
Gangpur	....	....	....	....	....	....	....	....	....	....	....	....
Singhbhum	....	....	....	....	....	....	....	....	....	....	....	....
Bombay:												
Belgaum	....	....	....	....	....	....	....	....	....	....	....	....
Panch Mahàls	....	....	....	....	....	....	....	....	....	....	....	....
Ratnagiri	....	....	....	....	....	....	....	....	....	....	....	....
Central India:												
Jhabua	....	....	....	....	....	....	....	....	....	....	....	6,800
Central Provinces:												
Bálághát	....	....	....	....	....	....	....	....	....	3,839	1,975	7,898
Bhandára	....	....	....	....	....	....	....	....	....	499	5,360	4,998
Chhindwára	....	....	....	....	....	....	....	....	....	....	....	....
Jubbulpore (Jabalpur)	....	....	....	....	....	....	....	....	....	....	....	....
Nágpur	....	....	....	....	....	....	....	....	47,257	76,925	68,819	95,051
Madras:												
Bellary	....	....	....	....	....	....	....	....	....	....	....	....
Sandur	....	....	....	....	....	....	....	....	....	....	....	....
Visigapatam	674	3,130	11,410	15,816	56,869	74,467	62,980	84,652	92,008	76,473	68,171	63,074
Mysore:												
Shimoga and other districts	....	....	....	....	....	....	....	....	....	....	....	....
Totals	674	3,130	11,410	15,816	56,869	74,467	62,980	84,652	139,265	157,736	144,325	177,821

	1904	1905	1906	1907	1908	1909	1910	1911	1912	1913
Bengal:										
Gangpur	....	....	1,000	2,000	20,000	55,060	41,958	25,152	27,173	11,215
Singhbhum	....	....				....	....	....	....	....
Bombay:										
Belgaum	....	640	234	22,125	23,232	....	....	....	....	....
Panch Mahàls	....	....	7,286			17,657	30,396	45,330	43,538	40,914
Ratnagiri	....	....	....	....	....	....	525	....	....	....
Central India:										
Jhabua	11,564	30,251	50,073	35,743	13,315	10,324	12,664	7,319	5,652	6,814
Central Provinces:										
Bálághát	10,323	16,246	102,260	167,713	135,487	134,577	161,987	144,642	135,435	219,139
Bhandára	8,559	35,238	96,017	285,248	110,673	110,856	159,164	119,606	115,365	89,818
Chhindwára	....	....	7,486	24,936	49,008	17,464	19,556	1,540	16,517	78,583
Jubbulpore (Jabalpur)	....	....	....	....	48	....	300	....	....	....
Nágpur	66,142	100,063	146,117	208,675	135,839	118,388	211,232	179,263	147,225	261,767
Madras:										
Bellary	....	....	....	....	....	....	500	....	....	....
Sandur	....	1,200	3,209	151,890	118,089	78,636	73,666	66,950	62,488	52,169
Visigapatam	53,602	63,789	111,501			59,818	46,441	58,915	54,758	44,127
Mysore:										
Shimoga and other districts	....	....	46,312	....	68,624	41,880	42,518	21,573	24,929	10,510
Totals	150,190	247,427	571,495	898,345	674,315	644,660	800,907	670,290	633,080	815,047

I am indebted to D. F. Hewett, U. S. Geological Survey, for assistance in compiling this table.

[18] L. L. Fermor: *Op. cit.*, p. 443 (Statistics for 1892–1907). *Records Geological Survey of India*, 1908–1913 (Statistics for 1908–1913).

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As regards the geologic horizons in which they occur, the Indian manganese deposits are grouped as follows:

Recent (lateritic, lateritoid, concretionary, and other surface concentrations).—Belgaum, Bijapur, Dhárwar, North Kanara, Jabalpur, Bidar, Ganjam, Nágpur, Bhandára, Sátára, Dhar, Indore, Nimár, Wun, Bhopal, Hoshangábád, Gawlior, and Nilgiri Hills.

Pre-Paleozoic.—(Interlayered beds and lenses in the following rocks.)

1. Dhárwar and related rocks: (*a*) Less metamorphosed facies (Dhárwar); Jhabua, Panch Mahál, Bálághát, Belgaum, Dhárwar (Sangli State), North Kanara, Jabalpur, Bellary (Sandur Hills), Chitaldrug, Kadur, Shimoga, Tumkur, Singhbhum, Kálahandi and Goa (Portuguese). (*b*) Strongly metamorphosed facies (gondite series): Bálághát, Bhandára, Chhindwára, Nágpur, Narukot and Gangpur.

2. The oldest gneisses and related rocks (khondalite and kodurite series): Vizagapatam and Ganjam.

Of the manganese occurrences enumerated above the only ones of proved economic importance are those of the Jhabua district of Central India, of the Panch Mahál district of Bombay, the Chhindwára, Bálághát, Bhandára and Nágpur districts of the Central Provinces, the Sandur and Vizagapatam districts of Madras, the Shimoga district in Mysore, and the Gangpur district in Bengal. However, small quantities of ore have been produced in the Singhbhum district of Bengal, the Belgaum and Ratnagiri districts of Bombay, the Jabalpur district of the Central Provinces and the Chitaldrug, Tumkur and Kadur districts of Mysore.

It will be noted that all the important manganese-ore deposits of India known at present, with the exception of those of Vizagapatam, occur in one or other of the various phases of the Dhárwar rocks. Brief descriptions of the principal districts are given below. The foregoing table gives the annual production of manganese ore in India, by districts, in long tons since 1892.

Jhabua District

The Jhabua district is situated in Central India about 350 miles E. of N. from Bombay. Only one mine of importance occurs in the district, that of Kájlidongri, operated by Messrs. Kiddle, Reeve & Co., who commenced shipping ore in 1903. The total production of manganese ore in the district is given in Table VI.

The rocks of the district[19] belong to the less metamorphosed phase of the Dhárwar series and consist of chloritic, talcose and sericitic slate, phyllite and schist, quartzite, schistose grit and conglomerate, crystalline limestone and dolomite and augengneiss. These have been folded and

[19] Fermor: *Op. cit.*, p. 687 ff.

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metamorphosed and later intruded by granite and pegmatite. The general strike of the rocks is north-northwest while the dips may be either east or west because of the sharp folding. The country is cut up by ravines into a large number of hillocks and mounds, partly under cultivation and partly covered by a thin jungle of bushes and small trees.

The Kájlidongri deposit, the only important one at present in the district, is 5 miles west-northwest of Meghnagar station (361 miles from Bombay on the Godhra-Ratlám railway), with which the deposit is connected by a 2-ft. gage railway. The deposit is about 1,000 yd. long and forms a long, low mound rising about 60 or 70 ft. above streams on either side. The manganese ore is intercalated with black and red vitreous quartzite, the whole forming a bed about 20 ft. thick, which has suffered folding and crinkling, so that a considerable length of it is near the surface. The ore-quartzite bed, of which probably 50 per cent. is merchantable ore, is enclosed between beds of silvery sericite or talc phyllites, with usually a thickness of sandy rock between them and the ore-quartzite bed. The sandy rock is usually lavender or lilac in color and very friable. In places it is argillaceous and has been metamorphosed to slate. The total tonnage of merchantable ore in the deposit is estimated at about 837,000.

The ore differs at the two ends of the deposit. The northern part consists of psilomelane with abundant and often coarsely crystalline braunite. Associated with these is an abundance of spessartite and some rhodonite. The southern part consists mainly of a very fine-grained mixture of psilomelane and braunite with no sign of spessartite or rhodonite. The following are average analyses of ore from different parts of the deposits.[20]

	Southern Part, Per Cent.	Northern Part, Per Cent.	
Mn	47.77	48.49	48.25
Fe	8.60	10.40	9.45
SiO_2	6.60	8.40	8.60
P	0.165	0.241	0.272

From the character of the ore and the association of spessartite and rhodonite, it is supposed that the larger part of the deposit is an alteration-product from an original manganese-silicate rock that was interlayered with the sediments. The southern part of the deposit is believed to have been in part laid down as original manganese oxide interlayered with quartzite, while in part it is a later replacement of the quartzite.

[20] L. L. Fermor: *Op. cit.*, pp. 683 to 684.

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Panch Mahâls District[21]

This district is near Sivarájpur about 270 miles north of Bombay, east of the Bombay, Baroda and Central India railway, with which it is connected at Chámpáner Road station by a mono-rail 17 miles in length. It was first explored for manganese ore in 1905, and in 1906 the first shipments were made by the Shivrájpur Syndicate, Ltd. In 1907, the Bamankua Manganese Co. began shipping ore from the district, and in succeeding years the shipments have steadily continued.

The rocks are siliceous slates, often sericitic and calcareous, and very fine-grained quartzites, in places jaspery. The slates are gray, while the quartzites are white, purplish, or reddish and in places are banded with limonite. The rocks belong to the less metamorphosed phase of the Dhárwar series.

The deposits occur east and southeast of Sivarájpur in hills rising from 200 to 300 ft. above the plains. Most of them are found outcropping on the tops of ridges. The ores appear to be replacements of the quartzites, and locally of the slates, by oxide of manganese, and are very irregularly distributed. In places, however, they appear to be interbanded with the quartzite. Small residual patches of original quartzite and slate often occur in them, in which respect they resemble the deposits of the Sandur Hills. Development by mining has shown the ores to extend deeper than was at first anticipated; and it is possible that original beds of manganese ore occur in addition to the material formed by surface replacement.

The ore is a mixture of psilomelane and braunite associated in places with pyrolusite. Braunite usually occurs as small shining specks disseminated through the psilomelane.

An average analysis of the ore as shipped[22] gives: Mn, 52; SiO_2, 4 to 6; and P, 0.17 per cent.

Chhindwára, Bálághát, Bhandára and Nágpur Districts

These districts are in the Central Provinces, a short distance north of a straight line between Bombay and Calcutta and nearer to the former. All four districts are within an area about 120 miles long east and west, by about 50 miles wide north and south. The Chhindwára district is in the northwestern, the Nágpur district in the southwestern, the Bhandára district in the central, and the Bálághát district in the northeastern, part of this area.

Until 1899 Vizagapatam was the only district in India producing manganese ore.[23] In that year, however, the Nágpur district was

[21] L. L. Fermor: *Op. cit.*, p. 651 ff.
[22] L. L. Fermor: *Op. cit.*, p. 655.
[23] L. L. Fermor: *Op. cit.*, p. 422 ff.

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prospected by the Central Provinces Prospecting Syndicate, formed by members of the Vizianagram Mining Company of Vizagapatam. The first shipments of ore were made by this company from the Nágpur district in 1900. In 1901 they started operations in the Bálághát district and in 1903 in the Bhandára district. Their production increased so rapidly that in 1906 it amounted to 223,823 long tons.

In 1902 a second company, Messrs. Chas. Jambon & Cie, began to open up deposits in the Nágpur district. The holdings of this company were later taken over by the Central India Manganese Company, Ltd., which in 1905 extended its operations to the Bhandára district. A third company, Gow Smith, Dundas Whiffin & Co., formed in 1903, and afterward changed to The Indian Manganese Co., Ltd., began operations in the Nágpur district in 1903, and at the same time took up various deposits in the Chhindwára district. Its first shipments from the latter district were made in 1906.

Besides those above mentioned, other operators have shipped small quantities of ore from the Central Provinces. The total production of manganese ore in the four districts is given in Table VI.

The deposits of the Chhindwára, Nágpur, Bhandára and Bálághát districts are tributary to the Bengal-Nágpur railway. The Nágpur and Bhandára deposits are north of the main line and are reached by several branch lines. Many of the mines are directly connected with the railway, but from some of them the ores must be carted for considerable distances to the railway stations. The Bálághát deposits are reached from the Satpura railway, a branch running northward from the Bengal-Nágpur railway to Neinpur and Seoni. Most of the ores have to be hauled by carts for considerable distances—some as much as 20 miles—to stations on this railway. The Chhindwára deposits are from 31 to 50 miles from Nágpur on the main line of the Bengal-Nágpur railway and from 27 to 45 miles from Chhindwára. A proposed railway line from Nágpur to Chhindwára will pass through the district.

The distance from Bombay to Calcutta on the Bengal-Nágpur railway is 1,221 miles. The city of Nágpur in the western part of the Central Provinces manganese region is about 520 miles from Bombay, while the station of Bálághát, on the Satpura railway, in the eastern part of the region, is 627 miles from Bombay.

The manganese-ore deposits of the Nágpur, Chhindwára, Bálághát and Bhandára districts are found in rocks of Dhárwar age.[24] With the exception of some of the Bálághát deposits, all are in the more highly metamorphosed and recrystallized facies of the series. These rocks are so difficult to distinguish from the oldest gneisses and granites that they are usually all classed together under the term "metamorphosed and crystalline complex." A few of the Bálághát deposits occur in less meta-

[24] L. L. Fermor: *Op. cit.*, pp. 691 to 977.

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morphosed Dhárwar rocks, which, as exposed in the Bálághát region, consist of slate, phyllite, mica schist, quartzite, quartz schist, and schistose conglomerate and grit. Not conspicuous in this district, but very characteristic of the Dhárwar rocks in other parts of India, are the banded hematite and magnetite jaspers.

The Bálághát manganese ores associated with these less metamorphosed Dhárwar rocks, usually occur as beds or lenses of varying thickness and extent. The longest of the known deposits is that of Ukua, which has a length of 3 miles and a maximum thickness of 19 ft., while the widest is that on which the Bálághát mine is located, which has a maximum thickness of 40 to 45 ft. and a length of 1.75 miles. These beds are interlayered with the metamorphosed sediments such as phyllite, schistose grit or quartzite, and correspond with them in strike and dip. In detail, the ore beds consist of interlaminated manganese oxide and dense compact quartzite, black, red, purple or gray in color. The ore is usually hard, dense psilomelane; but other minerals such as hollandite, a crystalline variety of psilomelane, and braunite are found as well. The occurrence of pyrolusite also is reported. In places where the ore has suffered somewhat more metamorphism silicates of manganese such as spessartite, the manganese-aluminum garnet, and rhodonite, the manganese pyroxene occur along the contact between the ore bed and the enclosing rock, and in such localities the ore bed usually contains a larger percentage of braunite, due to the combination with manganese oxide of some of the detrital silica present.

Where highly metamorphosed, the Dhárwar rocks of the Nágpur, Bhandára, Bálághát and Chhindwára districts consist of quartzite, quartz schist, mica schist, hornblende schist, crystalline limestone and various kinds of gneiss such as feldspathic, pyroxenic and epidotic.

The manganese ores associated with these highly metamorphosed rocks are in beds and lenses interlayered with the country rock, with which they conform in strike and dip. They differ from those in the less metamorphosed rocks, however, in the degree of alteration and recrystallization which they have undergone. While psilomelane still occurs abundantly where the ore bed was originally pure, braunite and the manganese silicates, spessartite and rhodonite form a much larger proportion of the beds than in the less metamorphosed deposits. Locally, where manganese-ore beds were originally impure and contained an abundance of argillaceous and siliceous material, the entire bed has been altered to manganese silicates. Usually, however, such alteration has occurred only for a certain distance inward from the wall rock, the center of the bed consisting of manganese oxide, chiefly braunite. Quartz is generally an abundant constituent of the beds. This association of minerals gives rise to a variety of rocks, such as quartz-spessartite rock, quartz-spessartite-rhodonite rock, spessartite-rhodonite-braunite rock, quartz-rhodo-

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nite rock, rhodonite-braunite rock, etc. The quartz-spessartite rock is the type rock of the series and is termed "gondite." The whole series is called the "gondite series." The gondite rocks occur areally as parallel bands of varying width and continuity, bounded by similar bands of the associated rocks such as gneiss, schist, quartzite, or crystalline limestone.

Besides the gondite type of ore these highly metamorphosed rocks contain another type of manganese ore, viz., that occurring as small lenticles and bands of nodules in the crystalline limestone, usually in association with piedmontite, the manganese-epidote, as in the Nágpur district, or with spessartite and rhodonite as in the Chhindwára district. The nodules or lenticles consist usually of brilliantly crystalline manganese ore with variable amounts of manganese silicates, chiefly piedmontite. In some cases entire nodules consist of silicates, but usually the silicates occur on the periphery while the interior consists of manganese ore. The latter is composed predominantly of one of the manganates, hollandite or psilomelane, intermixed with variable amounts of braunite. The crystalline manganate, hollandite, is more common than the amorphous manganate, psilomelane.

The three types of ore described above are known as primary ores. They are supposed to be simply the metamorphosed phases of original sedimentary beds of manganese oxide. Besides these, there are two other types of manganese ore found in the Dhárwar series of this region.

In the less metamorphosed Dhárwar rocks, many of which contain an appreciable content of manganese, surface-weathering causes a working over and concentration of materials, which frequently results in a replacement of parts of the formation by manganese oxide. Such deposits consist chiefly of psilomelane, pyrolusite and wad. They are of no importance in the districts under consideration.

The other type of ore is secondary after gondite rocks. When highly metamorphosed manganese-silicate beds suffer leaching and oxidation the result is their reconversion to manganese oxides. The minerals formed in this alteration are principally psilomelane, pyrolusite and wad. The importance of these secondary ores is considerable. Usually they are associated with primary ores of the gondite series, and it is often difficult to say what part of a deposit is primary and what part is secondary. There is no doubt, however, that a large portion of the ores of the Central Provinces is of secondary origin.

The degree of alteration of the manganese-silicate rocks varies in different places. Locally only veins and patches of manganese oxide occur through a mass composed largely of manganese silicates. Elsewhere the alteration has gone on to such an extent that only residual patches of pink rhodonite and yellow spessartite remain in the ore, and in many cases even such silicate remnants have disappeared.

The largest body of mixed ore and manganese-silicate rock occurring

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in the more metamorphosed phase of the Dhárwar rocks is that known as Hill 5 of the Ramdongri deposits in the Nágpur district. This body has a maximum width of 1,500 ft. and is nearly half a mile in length. Many deposits, however, are known in the different districts which range up to several hundred feet in width and up to several thousand feet in length.

It is believed by Fermor[25] that the manganese ore and associated rocks of the Dhárwar series were originally laid down as sediments, the manganese probably being in the form of oxides and the associated sediments being clay, marl and sand of varying coarseness. These, upon consolidation and metamorphism, have been changed to the rock types described above. Thus clays were altered to phyllites and mica-schists, sands to quartzites and quartz schists, and marls to crystalline limestones. Where beds of pure manganese oxide occurred the metamorphism resulted simply in hardening and frequently in crystallization and alteration to braunite. But where the manganese oxide was intermixed with much aluminous and siliceous material, intense metamorphism resulted in the formation of manganese-silicate beds. In places where ore was in excess of impurities, crystallized and hardened manganese oxides occur red with the silicates; but where impurities were in excess, quartz and other minerals occur red with the manganese silicates.

The following are analyses of manganese ore from the different districts:

	Nágpur,[a] Per Cent.	Chhindwára[b] Per Cent.	Bhandára[c] Per Cent.	Bálághát[d] Per Cent.
Mn	42.28–55.15	48.95–54.97	49.00–54.07	49.08–54.51
Fe	2.09–16.34	5.00–11.77	4.26–10.25	5.28– 9.10
SiO_2	2.90–18.48	4.98–10.63	2.08– 6.50	1.62– 6.02
P	0.04– 0.65	0.06– 0.28	0.06– 0.34	0.05– 0.24
Moisture	0.11– 1.32	0.00– 1.27	0.09– 1.15	0.12– 0.85

[a] Limits of 26 samples collected in different parts of the district. L. L. Fermor: *Op. cit.*, p. 843.

[b] Limits of 8 samples collected in different parts of the district. L. L. Fermor: *Op. cit.*, p. 772.

[c] Limits of 13 samples collected in different parts of the district. L. L. Fermor: *Op. cit.*, p. 735.

[d] Limits of 13 samples collected in different parts of the district. L. L. Fermor: *Op. cit.*, p. 697.

Sandur District

The deposits of the Sandur Hills in Bellary are in the Madras Presidency in the southern part of the Indian peninsula, approximately 300 miles northwest of Madras. They were discovered and first opened up

[25] L. L. Fermor: *Op. cit.*, p. 284 ff.

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in 1905 by representatives of Messrs. Jambon & Cie. Later the interests of this company were transferred to the General Sandur Mining Company, Ltd., which continued the operations. The total production of manganese ore in the district by years is given in Table VI.

The rocks of the Sandur district[26] with which the manganese ore is associated are of Dhárwar age. They consist of chloritic, sericitic and biotitic phyllites with interbedded ferruginous quartzite and some contemporaneous basic flows. The ferruginous quartzite is apparently similar to certain rocks which in occurrences farther north are described as magnetitic and hematitic banded jaspers. The iron present may be in the form of magnetite, hematite or limonite.

These rocks are folded into a northwest-southeast canoe-shaped syncline, 36 miles long and 12 miles wide at the widest part. The Sandur Hills, the summits of which rise more than 3,000 ft. above sea level, occupy the rim of this syncline and are formed by the outcrops of Dhárwar rocks. Inside is a basin containing the town of Sandur while outside is the Bellary plain, from 1,800 to 1,900 ft. above sea level, underlain by granite, gneissose granite and gneiss. The gneiss is probably mainly of pre-Dhárwar age, while the granites are later intrusions. All the manganese ore deposits lie in the western and southern part of the synclinal area, usually on the summits or slopes of ridges.

The deposits are grouped into four subdistricts: Rámandrug, Kannevihalli, Kumáraswámi, and Kamátaru. Of these the Rámandrug deposits are the principal ones and were the first operated. The manganese ore, as well as the iron ore, of which much occurs in this region, usually outcrops along the ridges, and wherever it occurs presents a rugged, rough and irregular surface without much tall vegetation. The outcrops frequently show traces of bedding which corresponds in strike and dip to that of the surrounding rocks. These, which are largely phyllites and slates, with subordinate quartzite, weather with comparative rapidity and leave the ore outcropping prominently. Iron and manganese ore are frequently associated in the same outcrop.

Although the manganese-ore deposits show traces of bedding at the surface and the indications are that they are interlayered with the surrounding phyllite, the deposits opened up do not, as a matter of fact, continue in depth, but give place to variegated and banded lithomarge at depths of from 50 to 100 ft. This lithomarge is supposed by Fermor to be a residual product from the weathering of the phyllite. The supposition, therefore, is that the ores are formed by the local replacement of the beds of phyllite. Only a few of the deposits have been developed, however, and it is possible that interbedded lenses of manganese ore may yet be found.

[26] L. L. Fermor: *Op. cit.*, p. 995 ff.

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The deposits are roughly elongated, tabular in shape, forming surface cappings similar to laterite. The larger of the known deposits average 90 or 100 ft. in width and vary up to 700 ft. in length. In all, more than 20 deposits of different sizes are known. It is thought probable that the district may contain 10,000,000 tons of manganese ore, some of it, however, quite ferruginous.

The ore consists principally of psilomelane and wad with subordinate amounts of pyrolusite and manganite. The psilomelane and wad are irregularly mixed and form a more or less cavernous ore in the cavities of which manganite and pyrolusite occur abundantly. Fragments of partly altered phyllite or slate are found in the ores, and disseminated octahedrons of magnetite, which are abundant in the country rock, also occur. The average composition of the ore is given as follows:[27] Mn, 45; Fe, 12; SiO_2, 1; and P, 0.02 per cent.

Vizagapatam District

This district is in Madras on the east coast of the peninsula of India, about 500 miles southwest of Calcutta. The ores, among the earliest found in India (having been known to exist as early as 1852), were also the first to be mined and exported. Mining operations commenced in 1891 and a small quantity of ore was exported in 1892. Since then the production has rapidly increased as is shown in Table VI.

The ore deposits occur on a belt of low-lying plains bordering the sea coast, varying from 30 to 50 miles in width, and bounded in the interior by a range of wild hills known as the Eastern Ghâts, the summits of which rise 3,000 and 4,000 ft. above sea level.

Geologically[28] the region consists of alternating bands of gneissose, garnetiferous, biotite granite and certain metamorphosed sediments known as the khondalite series. The khondalite rocks are schists consisting essentially of garnet, sillimanite, quartz and graphite. At the contact of these two types of rocks, closely associated with the khondalite rock, is a curious highly calcareous gneissoid rock, consisting principally of pyroxene, wollastonite, scapolite, garnet, calcite and sphene. Quartzite, also in part garnetiferous, occurs locally with the khondalite rocks. The manganese-bearing rocks, known as the kodurite series, are associated with the above-described rocks, especially with the calcareous gneisses. The surface expression of all these rocks is that of parallel bands.

The kodurite rocks are supposed by Fermor[29] to be of intrusive origin. Although variable in composition, they consist typically of potash

[27] L. L. Fermor: *Op. cit.*, p. 1019.
[28] L. L. Fermor: *Op. cit.*, pp. 1042 to 1115.
[29] L. L. Fermor: *Op. cit.*, p. 247.

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feldspar, manganese garnet (spandite) and apatite, with or without manganese pyroxene or quartz. The manganese garnet is of variable composition intermediate between spessartite and andradite. Manganese pyroxenes of several varieties occur. The phases of this rock rich in manganese minerals are altered by surface oxidation and leaching to ore high in manganese oxides.

On the plains in the region of the manganese mines the rocks are largely obscured at the surface by alluvium, but locally they are exposed in isolated hills. The same is true of the associated manganese ore which generally occurs outcropping as low mounds and hillocks, although frequently it is not exposed at all.

The principal mines of the district are those of Kodur, Garbhám and Perapi, operated by the Vizianagram Mining Company, Ltd. These mines together have produced more than 90 per cent. of all the ore mined in the district. The ore is carted to the station of Garividi on the Bengal-Nágpur railway, the distance of the mines from the station varying from 1 to 8 miles. From Garividi the ore is taken by rail 56 miles to the port of Vizagapatam.

The Kodur mines are located along a belt of manganiferous rock about 3 miles long and averaging a quarter of a mile in width, bounded on both sides by wollastonite-scapolite-diopside gneiss, the whole being enclosed between bands of khondalite rock. The principal country rock is gneissose granite. The manganiferous band consists of kodurite rock, altered at the surface to manganese ore, lithomarge, wad, yellow ocher, quartz, and other decomposition-products. The manganese orebodies are irregular in shape and of varying size. Large bodies, usually roughly lenticular, occur at intervals along the manganese-bearing belt and are embedded in soft lithomarge and other products of decomposition, which are also frequently intermixed with the ore. Besides the large bodies, small bands, veins and nodules of manganese ore occur abundantly throughout the decomposed soft rocks, especially in the lithomarge and wad.

The decomposition of the kodurite rock is apparently quite irregular, masses of the fresh rock occurring at different depths within the decomposed materials. It seems that in places where the orebodies now occur the original rock was rich in spandite, and where now the products of decomposition occur, the original rock consisted largely of feldspar, apatite and quartz. Some enrichment, however, has probably taken place.

The situation at the Perapi and Garbhám mines is geologically very similar to that at the Kodur mines. At Perapi much unaltered rock occurs, mainly spandite-feldspar rock and spandite-rhodonite rock. In some portions of the deposits chert is abundantly associated with cavernous psilomelane and yellow ocher. The Garbhám deposit is said to be

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the largest yet discovered in India. It is 4,500 ft. long and 167 ft. in thickness and dips 40° S. Within this body, however, are enclosed masses of wad and lithomarge, so that the ore bed proper has a thickness of only about 100 ft. In addition to this a mass of vein quartz or coarse quartzite occurs in the western portion of the deposit and cuts it into two parts. Unaltered kodurite rocks occur locally, but are not conspicuous. The country rock is mainly gneissose granite, with local areas of quartz rock and quartz-garnet rock. In both the Kodur and Garbhám mines manganese ore has been found at depths greater than 100 ft.

Other manganese-ore outcrops and mines of the Vizagapatam district occur in the neighborhood, the entire manganese-bearing district occupying an area of about 100 sq. miles. Most of the important mines are operated by the Vizianagram Mining Co., but recently a few have been worked by the Bobbili Mining Co. and other firms.

The principal ore mineral of the district is psilomelane,[30] often very cavernous and frequently containing scattered granules of braunite. The latter occasionally occurs in big plates and becomes the predominant mineral. Kidney-shaped and stalactitic varieties of psilomelane are also found in different places, and pyrolusite is frequent. The ore as mined ranges in metallic manganese from 35 to about 50 per cent. and much of it contains considerable iron. That containing less than 43 per cent. manganese and more than 15 per cent. iron is classed as ferruginous manganese ore; that containing 43 per cent. manganese or more, as manganese ore. The range in composition of 12 samples of ore, taken from the principal mines of the Vizianagram Mining Co., Ltd.,[31] is: Mn, 32.21 to 49.05; Fe, 4.80 to 15.70; SiO_2, 1.10 to 10.30; P, 0.13 to 0.48; and moisture, 0.50 to 1.85 per cent.

Shimoga, Tumkur and Chitaldrug Districts

Of the deposits of Mysore State[32] those of Shimoga, Tumkur and Chitaldrug districts are the most important. They are located SE of the Portuguese colony of Goa, at distances varying from 130 to 250 miles. The ore districts are tributary to the Southern Mahratta railway, running between Goa and Madras. Ore has been known to occur in this region since 1855, but the first discovery of economic importance was made in 1899 in the Shimoga district, where indications of the big Kumsi deposit were found. Attempts to open up the district began a few years later and culminated in the formation of the New Mysore Manganese Co., Ltd., which began shipping ore in 1906. Several other companies started operations in the district soon after, and the production has been fairly

[30] L. L. Fermor: *Op cit.*, p. 1048.
[31] L. L. Fermor: *Op. cit.*, p. 1049.
[32] L. L. Fermor: *Op. cit.*, p. 1117 ff.

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steady. The operations in the Tumkur and Chitaldrug districts began about the same time as those in the Shimoga district, the principal operator being the Peninsular Minerals Co. of Mysore, Ltd. The deposits of all the districts occur in less metamorphosed rocks of Dhárwar age and are very similar to those of the Sandur Hills.

The Shimoga district is the most important. It occurs in a plains country underlain by granite and gneiss, from which spring up ranges of hills composed of rocks of Dhárwar age. The granite, as a rule, is younger than the Dhárwar rocks, and is intrusive into them; but some of the gneisses are of pre-Dhárwar age. The general level of the plains is from 1,900 ft. to 2,200 ft. above sea level, while the ranges rise above this to elevations varying from a few hundred to a thousand feet.

The deposits occur in the Dhárwar rocks and are generally in the hilly areas. The Dhárwar rocks consist of phyllite, slate, quartzite, dolomite and limestone. The manganese ores occur as cappings on phyllite or quartzite or their decomposed equivalents. Often they are found on the tops of hills, and almost all of them, when quarried into, show a passage from mixed manganese ore and iron ore into decomposed wad, lithomarge and friable quartzite. They seem to be superficial replacements of decomposed phyllite and quartzite, and closely resemble the laterite blankets which are so common in areas of Dhárwar rocks. Nearly all the deposits become poor with depth, and show this gradation downward into decomposed rock. Many of them include masses of quartzite and phyllite, or masses of these rocks partly replaced by manganese oxides.

The chief minerals in the ore are psilomelane, wad and pyrolusite. The pyrolusite seems to form directly by replacement of quartzite, while the wad is associated more often with lithomarges, being formed by their gradual replacement. Wad gradually passes into psilomelane by further addition of manganese oxide. A common variety of ore shows oolitic and pisolitic structure, concentric concretions of psilomelane being embedded in softer material, probably wad. The range in composition of the ore[33] from the Shimoga district is shown by analysis to be: Mn, 30 to 56; Fe, 2 to 20; SiO_2, 1 to 6; P, 0.01 to 0.14; and moisture about 1 per cent.

The largest single deposit in the Shimoga district is that of Kumsi, about 1,000 ft. long by 320 ft. wide. On the assumption that the ore goes to a depth of 50 ft., Fermor estimates a maximum tonnage of 300,000 for this deposit.

The deposits of the Tumkur and Chitaldrug districts resemble those of Shimoga in their geological occurrence. Those of Chitaldrug occur in fairly thin laterite-like surface replacements consisting of a mixture of psilomelane and limonite, which overlie lithomarge, ocher, and wad. The latter are sometimes associated with fine-grained quartzite, which in places is impregnated with limonite and replaced by pyrolusite. The

[33] L. L. Fermor: *Op. cit.*, p. 1132.

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ores of Tumkur are irregularly scattered through the rocks in which they occur, and contain much limonite and residual patches of quartzite and argillaceous rock. Much of the ore is ferruginous manganese ore.

Gangpur District

In this district[34] which lies in the province of Bengal, along the Bengal-Nágpur railway, about 250 miles south of west from Calcutta, manganese ores were first discovered in 1907 and mining was started in 1908. The mines are not far from the railway, with which they are connected by a light steam tramway. The shipments of ore are shown in Table VI.

The ores occur in mica schist, mica phyllite, quartzite, and gray dolomite of the Dhárwar series. They are interbedded with quartzite and schist and associated in many places with manganese-silicate rocks, such as gondite, spessartite rock, spessartite-rhodonite rock, etc. Any of these may contain braunite as well. The ores themselves consist mainly of psilomelane and braunite, mixed in various proportions. The braunite shows varying degrees of coarseness. The principal deposits are at Gariajhor and Birbira.

The following are analyses of the ore.

Analyses of Ore from the Gangpur District

	Gariajhor (Picked Specimens) Per Cent.	Gariajhor (Picked Specimens) Per Cent.	Gariajhor (Average Cargo) Per Cent.
Mn	55.35	58.64	51.81
Fe	3.60	1.70	6.38
SiO_2	3.03	1.18	3.52
P	0.089	0.079	0.09

The deposits at Gariajhor, which are the principal ones now mined, exhibit seven exposures of ore and manganese-silicate rock along a northeast-southwest line of about 1,400 yd. The most important is at the southeast end, and consists of an exposure 720 ft. long and from 10 to 20 ft. wide. The country rock is interlayered quartzite and phyllite, the former being frequently interbedded within the ore band. The quartzite is red to gray vitreous, while the schist is white feldspathic. The ore consists typically of layers of a hard gray mixture of braunite and psilomelane. Wad occurs also and in cavernous places in the ore pyrolusite tends to develop.

Brazil

General Statement

Manganese ore is known in Brazil in the states of Bahia, Minas Geraes and Matto Grosso. The Bahia ores are in the Nazareth district

[34] L. L. Fermor: Notes on the Manganese-Ore Deposits of Gangpur State, Bengal, and on the Distribution of the Gondite Series in India, *Records, Geological Survey of India*, vol. 41, p. 12 ff, (1911).

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about 50 miles west of the city of Bahia. The ores of Minas Geraes are chiefly near the stations of Lafayette and Miguel Burnier, respectively 283 miles and 310 miles north of Rio de Janeiro on the Central of Brazil railway. Some ores also occur along the branch railway running from Miguel Burnier eastward to Ouro Preto. The Matto Grosso deposits are in the southwestern part of that state not far from Corumbá, the head of navigation on the Paraguay River. The deposits of Minas Geraes are by far the most important and have probably yielded more than 95 per cent. of all the ore shipped from Brazil. Small quantities, however, have been shipped from the Nazareth district, while the Matto

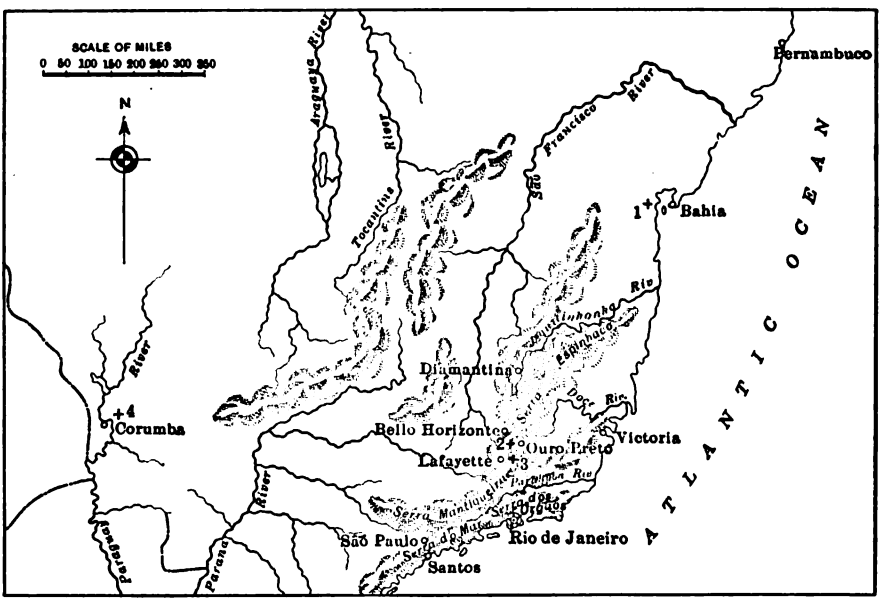

FIG. 3.—MAP OF PART OF BRAZIL SHOWING THE LOCATION OF THE PRINCIPAL MANGANESE-ORE DISTRICTS.

1. Nazareth.
2. Miguel Burnier-Ouro Preto.
3. Lafayette.
4. Corumbá.

Grosso ores have thus far been unable to compete, on account of their distance from the coast (see Fig. 3).

Table VII gives the annual exports of Brazilian manganese ore from the beginning of production to the present. It has not been possible to get reliable statistics of production for the separate districts. Bahia has produced a relatively small quantity of ore, while of the other two districts, the Lafayette district has probably produced in the neighborhood of three times as much ore as the Miguel Burnier-Ouro Preto district. Short descriptions of the Minas Geraes and Bahia districts are given in the following pages.

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TABLE VII.—*Annual Exports of Manganese Ore from Brazil*

	Long Tons		Long Tons
1894[a]	1,430	1904	204,260
1895	5,570	1905	220,843
1896	14,710	1906	119,420
1897[b]	15,801	1907	233,048
1898	26,001	1908	163,506
1899	63,976	1909	236,982
1900	106,639	1910	249,954
1901	98,832	1911	171,172
1902	154,817	1912	152,431
1903	159,375	1913	180,738
		Total	2,579,805

[a] H. K. Scott: The Manganese Ores of Brazil, *Journal of the Iron and Steel Institute*, No. 1, 1900, p. 189 (Statistics for 1894–1896).

[b] Reports of British Home Office, *Mines and Quarries*, 1898–1913 (Statistics for 1897–1913, Changed from metric tons to long tons).

Minas Geraes District

The presence of manganese ore in the Miguel Burnier-Ouro Preto district was discovered in 1888[35] when the Central of Brazil railway branch from Miguel Burnier to Ouro Preto was under construction, ore being encountered in one of the cuts east of Miguel Burnier. Mining operations were begun near this locality in 1894 by Costa and Almeida, the mine being called Usina Wigg. In 1897 another firm, Airoso and Co., started operations in the same neighborhood. After the manganese-ore discovery near Miguel Burnier, much exploration was done, and during 1896 and 1897 many deposits were discovered in the Lafayette district, among them the Barrosa, Morro da Mina, Piquery and São Gonçalo deposits. Of these the São Gonçalo mine commenced operating in 1900, the Piquery mine in 1902, and the Morro da Mina mine in 1904, while in 1910 the Rodeio mine east of Miguel Burnier installed an aerial tramway and joined the ranks of producers. The Wigg and Morro da Mina mines are the principal producers in Brazil. The reserve tonnage of these two mines is estimated at about 7,000,000.[36]

The manganese ores of Minas Geraes may be separated into two classes: (1) those occurring in a complex of granite, gneiss and crystalline schist, and (2) those occurring in overlying metamorphosed sediments, with which the important Brazilian iron ores also are associated. The deposits in the region around Lafayette belong to the first class, while those in the neighborhood of Miguel Burnier and Ouro Preto are of the second type. The centers of the two districts are not more than 17 or 18 miles apart, and some of the mines in the one can be seen from the

[35] H. K. Scott: *Op. cit.*, pp. 189 and 198.

[36] *Mineral Resources of United States*, 1905, p. 99.

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other. Two of the mines in Minas Geraes, the Morro da Mina, north of Lafayette, and the Wigg, east of Miguel Burnier, are in continuous operation, while several smaller mines produce ore intermittently. Among the latter are the Rodeio, east of Miguel Burnier, the Cocuruto, southwest of Lafayette, and the Queluz das Minas, near the Morro da Mina, north of Lafayette. In the general vicinity of these mines, there are many abandoned or inactive ones, among which are the Piquery, São Gonçalo, Agua Limpa, and Barrosa mines in the Lafayette district and the Bocaina, Rodrigo Silva and Tripuy mines in the Miguel Burnier-Ouro Preto district.

The distances to Rio de Janeiro from the five manganese mines at present operating in Brazil are 438, 462, 468, 501, and 508 km. respectively. The railway freight is 5 milreis ($1.65) per metric ton for distances not exceeding 500 km. and for distances in excess of this limit, 10 reis per ton-kilometer.[37] This gives a uniform rate of 5 milreis to all the mines except the Wigg and Rodeio mines, the former paying 5 mil 10 reis and the latter 5 mil 80 reis per ton. The rate of 10 reis per ton-kilometer is equivalent to about 0.33c. per ton-kilometer, or about 0.54c. per ton-mile.

The rocks underlying the portion of Minas Geraes in which the manganese and iron ores occur are all of supposed pre-Cambrian age. They have been classified as follows:[38]

Probable Algonkian

Itacolumi quartzite. Mainly quartzite, but locally containing schists of various kinds—mostly argillaceous.

Piraçicaba schist and quartzite. In the lower part mainly schist with lenses of iron formation and carbonate rock. In the upper part contains much quartzite.

Itabira iron formation. A banded hematitic quartzite, known as itabirite, with local beds and lenses of hematite and occasional beds of ferruginous schist.

Batatal schist. Gray and red argillaceous schist.

Caraça quartzite. Quartzite with locally much sericite schist.

Probable Archean

Gneiss, granite and schist.

The iron ore of Minas Geraes is associated principally with the Itabira iron formation, although a few small deposits occur in iron-formation lenses near the base of the Piraçicaba schist. The manganese ore of the first type described above occurs in Archean gneiss, granite and crystalline schist; those of the second type occur in the upper part of the Itabira iron formation and in the lower part of the Piraçicaba schist, being frequently associated with iron ores.

[37] D. Rocha: Cost of Transporting Manganese Ores in Brazil, *Engineering and Mining Journal*, vol. 91, p. 553, Mar. 18, 1911.

[38] E. C. Harder and R. T. Chamberlin: Geology of Central Minas Geraes, Brazil, *Journal of Geology*, vol. 23, pp. 341 to 378 and 385 to 424 (1915).

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The various sedimentary rock formations are complexly distributed through the central part of Minas Geraes, due in part to irregularity in original deposition, but more especially to extensive folding and faulting, followed by erosion. A profound unconformity separates the sedimentary rocks from the underlying crystalline complex.

In general, the metamorphosed sediments extend as a belt of varying width from central and southern Minas Geraes northward and slightly eastward into Bahia. This belt is characterized by a prominent mountain chain, the Serra do Espinhaço which contains some of the highest peaks and ridges in this part of Brazil. Its prominence is due largely to the superior resistance to weathering of the Caraça quartzite, which is in area by far the most extensive of the metamorphosed sediments. The iron formation generally occurs in well-marked foothills along the sides of the quartzite ridges, though locally, where well developed, and especially hard, it itself forms the main ridges, while the quartzite occurs on the slopes. The Piraçicaba schist, being soft, is inconspicuous topographically, but the Itacolumi quartzite forms a number of conspicuous peaks and ridges. The mountainous region of metamorphosed sediments is bounded by an irregular, undulating region of hills and valleys, underlain by rocks of the crystalline complex. This is most typically developed east and southeast of the sedimentary belt.

Manganese ores are found in the crystalline complex as large irregular bodies of manganese oxide inclosed in, or bounded by, gneiss, granite or crystalline schist. Individual masses, such as that at Morro da Mina, may be several hundred yards in larger diameter. While irregular in shape, they are usually somewhat elongated, suggesting lenses. They occur scattered through the crystalline complex without any apparent regularity, but most of them appear to have either gneiss or crystalline schist on one or both bounding walls.

The manganese oxide composing these lenses is usually in the amorphous form, occurring mainly as psilomelane and wad, though pyrolusite also is found with these. According to detailed studies made by Dr. Derby,[39] it appears that these oxides are surface-decomposition products resulting from the weathering of other manganese minerals, which, in one or two cases, notably in the Piquery mine, have been encountered below the zone of oxidation. Of these minerals the principal ones are tephroite (the manganese-olivine) and spessartite (the manganese-garnet); and with these occur rhodochrosite (the carbonate of manganese), and sparingly rhodonite (the manganese-pyroxene). These minerals

[39] O. A. Derby: On the Original Type of Manganese-Ore Deposits of the Queluz District, Brazil, *American Journal of Science*, 4th ser., vol. 25, pp. 213 to 216, 1908.

O. A. Derby: On the Manganese-Ore Deposits of the Queluz (Lafayette) District, Minas Geraes, Brazil, *American Journal of Science*, 4th ser., vol. 12, pp. 19 to 23, 1901.

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are intricately intermixed in varying proportions, one being more abundant in one place and another elsewhere; and together they form a reddish manganese-silicate and carbonate rock, in general appearance like quartzite, being hard and dense with platy structure. The relation of the manganese rock to the inclosing crystalline rocks has not been definitely determined; it may be interlayered with the gneiss or crystalline schist, or perhaps intrusive into them.

From the one or two instances noted it is judged that all the manganese-oxide deposits in the areas of the crystalline complex are surface-oxidation products of such masses of manganese-silicate and carbonate rock. In many of the deposits where the original rock has not been encountered, the oxide ores have textures which are duplicated in the manganese-silicate and carbonate rock elsewhere, and therefore suggest a similar origin for such deposits. In the Morro da Mina mine, where the original rock has not yet been encountered, the massive black ore of the upper levels passes with depth into a lustrous black ore that shows minute isolated specks of spessartite in a ground-mass of amorphous manganese oxide (psilomelane). Dr. Derby concludes that this groundmass was formed by the alteration of the more readily decomposed tephroite and rhodochrosite, specks of spessartite remaining unaltered.

During the process of decomposition, more or less solution and redeposition has taken place, with the result that certain portions of a deposit are composed of high-grade manganese oxide, while other portions contain admixtures of other products of decomposition, such as clay and silica. Most of the ore is hard, but soft material, mainly wad and pyrolusite, also occurs in abundance, irregularly intermixed with the hard ore.

The following are average analyses of ore from the Lafayette district.

Analyses of Manganese Ore from the Lafayette District[a]

	Piquery Mine, Per Cent.	São Gonçalo Mine, Per Cent.
Mn	49 00 to 51.00	50.00 to 52.00
SiO_2	5.00 to 7.00	1.00 to 2.00
P	0.08 to 0.10	0.12 to 0.15
Moisture	3.00 to 5.00	3.00 to 5.00

[a] *Min. Res. U. S.*, 1901, p. 142.

The manganese-ore deposits in the sedimentary series occur as definite beds associated with iron formation. The principal bed, that on which the Wigg mine is situated, is 2 or 3 miles in length, and at its maximum reaches a thickness of over 6 ft. It strikes east-west, parallel with the strike of the inclosing sediments, and corresponds with them in dip, making it apparent that the manganese-ore bed was laid down as a sedimentary bed like the inclosing rocks. The bed at the Wigg mine is bounded on one side by soft siliceous iron formation, with a contact

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zone of mixed soft, crystalline hematite and manganese oxide, and on the other side by a ferruginous schist associated with the iron formation.

The dip is generally steep varying on both sides of vertical.

The manganese-ore bed at the Rodeio mine is of smaller longitudinal extent, but of greater thickness than that at the Wigg mine and shows less definitely its relation to the inclosing rocks. In the vicinity of both of these deposits, beds of carbonate rocks consisting of a mixture of calcium, magnesium, iron, and manganese carbonates, are found, but at different horizons from the manganese-ore beds.

The manganese ores associated with the sedimentary rocks consist of finely crystalline or amorphous manganese oxides, probably largely a mixture of pyrolusite and psilomelane. From their occurrence it must be assumed that they are similar in origin to the associated rocks, that is, that they are original sedimentary deposits of manganese oxide which have been somewhat altered and re-crystallized by subsequent metamorphism. The source of the manganese is doubtful, but it may very well have been derived from deposits of manganese ore in the crystalline complex, such as now occur to the south near Lafayette. Decomposition of such deposits may have yielded a large amount of residual manganese oxide which was worked over, transported and deposited as beds or lenses in the sedimentary series. Their origin would, therefore, be very similar to that of the iron ores with which they are closely associated.

The ores associated with sedimentary rocks are of somewhat better grade than those occurring in the crystalline complex. The following are typical analyses of the ore.

Analyses of Wigg Mine Ores

	Average,[a] Per Cent.		Cargo,[b] Per Cent.	
Mn	50.00 to 54.00		55.14	55.02
SiO_2	1.00 to 2.00		0.53	1.25
P	0.01 to 0.03		0.03	0.021
Moisture	15.00 to 20.00	H_2O	4.95	4.74

[a] *Mineral Resources of United States*, 1901, p. 142.

[b] H. K. Scott: *Op. cit.*, p. 205.

Bahia District

The manganese-ore deposits of Bahia are about 16 miles west of Nazareth, a town on tidewater, about 30 miles west of Bahia, and reached from that city by steamer by way of the Jaguaripa River.[40] Ore has been shipped from three mines in the district, Sapé, Onha and Pedras Pretas. The first two are connected with the Nazareth railway by a narrow-gage line 5.25 miles long, over which the ore is hauled to a

[40] J. C. Branner: The Manganese Deposits of Bahia and Minas, Brazil, *Trans.*, vol. 29, 756 (1899).

D. F. Hewett: *Mineral Resources of United States*, 1914, p. 181.

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siding on the main line, where it is dumped into cars and hauled to Nazareth. The Pedras Pretas mine is only about half a mile from the Nazareth railway and the ore is run down by gravity on a narrow-gage track, the empties being hauled back by mule. At Nazareth the ore is taken on sailing lighters and carried out to the ships. The cost of the ore on board ship, including mining and transportation, is given as $2.80 per ton. The mines are situated in the comparatively low, hilly, densely forested coastal region of eastern Brazil.

The city of Bahia stands upon crystalline schists cut by eruptive dikes. Immediately west of the city is a basin of Cretaceous rocks which extends westward almost to within a mile of Nazareth, where the crystalline rocks again appear. In the vicinity of Nazareth the schists are so decomposed that solid rocks are to be seen in place only in the stream beds, and here and there in the hills. It is in the decomposed material derived from the crystalline schists that the manganese ores occur west of Nazareth. Nothing definite is known as to the age of these schists, but elsewhere in Brazil they are associated with granite and gneiss and have rocks, presumably Paleozoic, resting on them unconformably.

The ore is mainly psilomelane, occurring as lumps and large masses in clay and soft earth. Some masses are botryoidal in form, but most of them are angular. Occasionally masses are taken from the mines which weigh a ton and a half but usually they are smaller, ranging from mere particles to a foot or two in diameter. The smaller ones shipped have about the size of a fist.

The Pedras Pretas mine, which has been the principal producer, is mainly in soft earth. Part of the ore occurs as a horizontal sheet, spread out almost on the surface of the ground, and part occurs in a steeply dipping sheet or vein which connects above with the horizontal sheet. The steeply dipping deposit stands at an angle of about 60° and varies in thickness from 1 to 30 ft. It appears that the ore in the surface sheet is an accumulation derived from the underlying bed by its weathering and the removal of the clays about it.

The following is an average analysis of the ore:[41] Mn, 43 to 49; SiO_2, 3 to 4; P, 0.016; and moisture, 2 to 3 per cent.

CHILE

General Statement

Manganese ores were extensively mined in Chile for about 15 years before the close of the last century, during which period, perhaps, somewhat more than half a million tons was exported, most of it going to the United States. At about the beginning of the present century

[41] D. F. Hewett: *Op. cit.*, p. 181.

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the extensive deposits of manganese ore in central British India commenced to be exploited. These being richer and cheaper to handle than the Chilean ores soon displaced them in the market, and as a result the mines in Chile ceased operating.

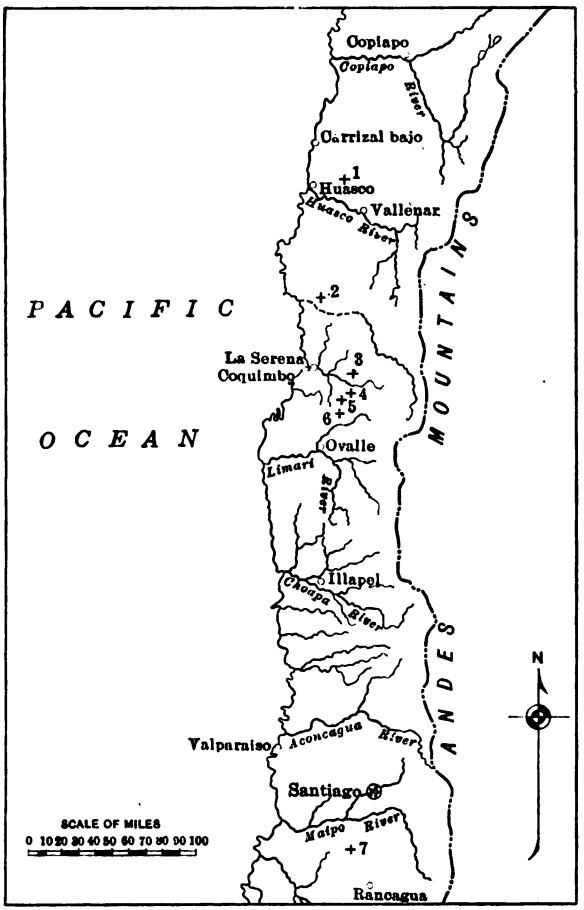

FIG. 4.—MAP OF PART OF CENTRAL CHILE SHOWING THE OCCURRENCES OF MANGANESE ORE.

1. Carrizal.
2. Los Chorros.
3. Las Cañas.
4. La Liga.
5. Arrayan.
6. Corral Quemada.
7. Aculeo.

Since 1905, practically no manganese ore has been mined in Chile. The mines have been abandoned and many of them have caved, while the works are neglected and in ruins. Yet the deposits are by no means exhausted, and there is little doubt that at some future time the mines will be re-opened. It is even possible that the present activity in the

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development of the Chilean iron ores and the opening of the Panama Canal may have some effect upon the manganese situation.

Manganese ores occur in Chile in the Huasco and Carrizal districts of Atacama, the Los Chorros, Las Cañas, La Liga, Arrayan and Corral Quemada districts of Coquimbo and the Aculeo district of Santiago. During the spring of 1914, while making an examination of the Chilean iron-ore deposits, I had an opportunity to visit nearly all of the manganese districts above mentioned (see Fig. 4).

TABLE VIII.—*Exports of Manganese Ore from Chile*

	Exported from Coquimbo	Exported from Carrizal	Total Exports
	Long Tons	Long Tons	Long Tons
1885[a]	4,041	.....	4,041
1886	23,701	227	23,928
1887	38,234	9,287	47,521
1888	12,132	6,581	18,713
1889	9,145	19,538	28,683
1890	23,409	24,577	47,986
1891	16,462	18,000	34,462
1892[b]	.....	.....	50,871
1893	.....	.....	36,162
1894	.....	.....	47,238
1895	.....	.....	23,696
1896	.....	.....	25,740
1897	.....	.....	23,156
1898	.....	.....	20,522
1899	.....	.....	40,285
1900[c]	.....	.....	25,309
1901	.....	.....	18,188
1902	.....	.....	12,785
1903	.....	.....	16,840
1904	.....	.....	2,287
1905	.....	.....	1,303
Total.....	.....	.....	549,716

[a] *Mineral Resources of United States* 1894, 16*th Annual Report, U. S. Geological Survey*, pt. 3, p. 441 (Statistics for 1885 to 1891).

[b] *Mineral Resources of United States*, 1901, p. 145 (Statistics for 1892 to 1899).

[c] Reports of British Home Office, *Mines and Quarries*, 1900–1906 (Statistics for 1900 to 1905. Changed from metric tons to long tons).

Most of the ore exported from Chile has come from the Carrizal and Corral Quemada districts, but considerable ore has been shipped from Las Cañas and La Liga also. Table VIII shows the quantities of ore exported annually from Chile while the mines were active. For the first seven years the exports from Coquimbo and Carrizal are given

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separately. After that date the separate quantities are not available, only the total being given. The Carrizal and Huasco ores were shipped from Carrizal, while the Corral Quemada, Arrayan, La Liga and Las Cañas ores were shipped from Coquimbo. In all cases when necessary the figures have been changed from other denominations to long tons.

Chilean manganese ores, according to their geological occurrence, may be classed under three heads: (1) ores occurring interbedded with jasper and chert in a limestone-chert formation; (2) ores occurring interbedded with red sandstone, shale, and limestone which in turn are interlayered with massive volcanic flows; and (3) ores occurring as veins in volcanic flows. To the first type belong the ores of Huasco and Carrizal, to the second type, those of Las Cañas, La Liga, Arrayan and Corral Quemada, and to the third type those of Aculeo and Los Chorros. The sediments and volcanic rocks with which the ores are associated are of Mesozoic age. Short descriptions of the principal districts follow.

Carrizal District

This district is situated between Huasco and Carrizal about 350 miles north of Valparaiso in a rugged barren part of the Coast mountains, which are here separated from the main Andean ranges by a flat "pampa" or desert valley of considerable width.

The ore occurs in an approximately north-south belt, continuous in its northern part, but broken in its southern part for a distance of a mile or more. In the northern part of the belt, which is several miles in extent, there are from one to three beds of manganese ore, close together, generally separated by a few feet only of chert or jasper. They are quite continuous and may be seen cutting across ridges and valleys. Locally, however, beds pinch out and new ones come in. The attitude is nearly vertical, varying from this to a dip of 60° or 70° E, or locally, W. The inclosing rocks have the same attitude as the manganese-ore beds, and consist mainly of interlayered gray chert and gray limestone in beds of varying thickness with local beds of gray shale. Occasional sills of dark green basic igneous rocks occur which are in places quite wide, but rarely of any great longitudinal extent.

The manganese beds vary in thickness from less than a foot to 4 or 5 ft. They are inclosed by pink or dark red jasper which usually extends several yards from the ore contact and then gives place to gray chert. The jasper usually has abundant veins of crystalline hematite and owes its color to hematite impregnations. The ore is dense, hard, black psilomelane with conchoidal fracture. It usually contains considerable silica in the form of chert, stained black so as to be hardly distinguishable from the ore itself. Locally masses of pink or red jasper occur within the ore beds.

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The principal mines along the northern portion of the Carrizal ore belt are, going from north to south, La Bruja, Negra, Porvenir, Huasquina and Coquimbana.

The southern portion of the ore belt is comparatively short and shows several breaks of continuity. The principal part of it consists of four parallel ore beds striking approximately northeast-southwest and dipping about 45° NW. The two lower beds have been extensively worked, the upper one of these having an average thickness of 3.5 ft., buf being considerably mixed with jasper. The ore beds are inclosed in beautifully banded red jasper, which, however, does not extend far from the contact. In general the country rock is interbedded gray or white chert and gray limestone, locally crystalline. Some distance below the ore horizon is a thick sill of green basic rock.

The ore is hard bluish-black braunite, with some psilomelane and an admixture of soft black oxides. The principal mines in this part of the belt are the Beatriz and Venus. The following are analyses of ore from the Carrizal district.[42]

Analyses of Manganese Ore from the Carrizal District

	Coquimbana, Per Cent.	Huasquina, Per Cent.
Mn	45.82	37.08
Fe	2.74	3.21
SiO_2	5.42	11.97
P	0.093	0.116
H_2O (combined)	3.78	3.48

The Carrizal deposits were first operated in 1886 and operations continued until about 1901. Since then little or no ore has been shipped from the district. The manganese mines occur along a spur of the railway line that ascends from the port of Carrizal to the copper camp of Cerro Blanco, and the ore was taken by rail to the port, a distance of about 50 miles.

Las Cañas, La Liga, Arrayan and Corral Quemada Districts

The Las Cañas, La Liga, Arrayan and Corral Quemada districts are related to each other in geological occurrence and are found in the same general region. They are situated in the western foothills of the Andes in a rough, barren, mountainous country, and occur, with intervening non-ore-bearing areas, along a general north and south line of 35 to 40 miles. The northern part of this area, that is, Las Cañas district, is 20 or 25 miles east of La Serena, while the southern part, the Corral Quemada

[42] Analyzed by Lerch Bros., Virginia, Minn.

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district, is northeast of Ovalle. The other two districts are between. La Serena is about 230 miles north of Valparaiso.

The foothills of the Andes in this region consist largely of massive volcanic flows, some of them many hundred feet thick. They are composed chiefly of trachyte and related rocks, which have a prevailing dark red or purple color. Green, gray and lavendar volcanics, however, occur abundantly. With these flows there are locally interbedded thin sheets of manganese-bearing sediments which sometimes extend over considerable areas, but more commonly are of limited extent. They range in thickness from a few feet to 50 ft. or more and consist of red and pink sandstone, shale and limestone and locally jasper. Occasionally thin beds of volcanic rock are interlayered within the sediments.

The manganese ores are interbedded with the sediments. In places only one bed occurs while elsewhere two or three parallel beds are found, usually within a few feet of each other. In most places the ore beds consist of pure, granular to finely crystalline bluish-gray or bluish-black pyrolusite, generally compact but soft. They range in thickness from mere seams up to 4.5 ft., but where they are mined they usually average from 1.5 to 3 ft. in thickness. Such beds may be inclosed between beds of red sediments or between sediments and lavas. The contact with the sediments or lavas may be sharp or, as is frequently the case, the inclosing rocks may be partly replaced by manganese oxides for some distance from the contacts, and in that case may consist of all gradations between fresh rock and the impure amorphous mixture of manganese oxides, wad. Such impure layers may be as much as 10 ft. thick, although the ore itself occupies only a very small portion of them. This material, however, is not mined, only the pyrolusite being utilized.

The manganese ore is generally closely associated with limestone. In many localities a manganese-ore bed consists of half ore and half limestone, the two being intricately intermixed. Generally the limestone is black, by reason of the manganese oxide it contains, but almost as frequently it is pink or red. There are places, however, where limestone is absent from the manganese-bearing horizon, as in parts of the Las Cañas district; and usually in such places the ore is of inferior quality.

The manganese-ore bed may occur at any horizon in a series of sedimentary beds. In places it is at the base, resting directly on trachytic flows; at other places it is at the top, with trachytic flows lying on it; while in still other places it occurs at various horizons within the sedimentary series. Thin partings, which may consist of limestone, sandstone or shale, frequently occur between two manganese-ore beds. Such partings often pinch out laterally and the two beds unite into one.

All the districts have numerous old workings; but the principal ones occur in the Las Cañas and Corral Quemada districts. In the former the Potosi, consisting of both open cuts and underground workings, is

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one of the larger mines. In the latter a large open cut occurs on the Elsie, Injo and Tomas claims, smaller open cuts on the Guillermo, Sallie, Maria and Adele claims, and underground workings on the Mercedes claim. The Corral Quemada district is said to have been operated for about fifteen years, from 1888 to 1903, during which period several hundred thousand tons of ore were shipped from it, being carried 20 miles on pack mules to the railway station of Angostura, whence it was taken by rail to the port of Coquimbo. The Las Cañas mines were operated from about 1887 to 1899, but produced much less ore, while the La Liga and Arrayan mines were only operated for a few years.

The following are analyses of ore from Corral Quemada, Las Cañas and La Liga.[43]

Analyses of Manganese Ore from the Province of Coquimbo

	Mina Alta, Las Cañas, Per Cent.	Mina Potosi, Las Cañas, Per Cent.	Mina Estrella, La Liga, Per Cent.	Elsie Cut, Corral Quemada, Per Cent.
Mn	40.31	52.85	49.54	50.00
Fe	3.38	1.09	1.29	0.78
SiO_2	11.20	7.74	5.00	9.43
P	0.022	0.007	0.010	0.013
H_2O (combined)	1.15	0.90	1.00	1.18

Discussion

Herbert K. Scott, London, England (communication to the Secretary*).—I am acquainted with the manganese-ore deposits of Russia, India and Brazil, and agree generally with the statement contained in the paper regarding those deposits. The information given by Mr. Harder regarding the Chilean deposits is very welcome.

When examining the Caucasian deposits, I endeavored to determine the quantity of ore originally contained in the deposit, and by deduction, the amount remaining for extraction. These calculations were rendered possible by the great extent of the outcrop and the extensive work that had been carried on in the deposit.

In making the first estimate, the quantity of manganese ore and associated sterile material in the bed was measured in a large number of places, and the total quantity of ore originally contained in the deposit was calculated equal to 57,000,000 tons.

With regard to the second figure, it was found that only about 15 per cent. of the mineral contained in the deposit was exported owing to (*a*)

[43] Analyses by Lerch Bros., Virginia, Minn.

* Received Aug. 16, 1916.

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the crude pillar and stall method of mining, (*b*) the large quantity of gangue associated with the ore, (*c*) the friable character of the mineral, (*d*) its low market value, and the exigencies of the purchaser.

In recent years, beginning in 1900, and more particularly since 1905, the introduction of longwall working and the stowing in the goaf of the associated sandstone, as well as the construction of numerous washing plants, has resulted in a larger proportion of the orebody finding its way to consumers. Already in 1906, workings were being reopened and pillars and poor ore withdrawn which had been hitherto abandoned, so that in all probability a large part of the ore in the old workings will be eventually recovered, although the measure in which this is done will depend upon the market value of the mineral at the time.

With longwall working 90 per cent. of the ore can be obtained, but for the purpose of calculation 75 per cent. may be estimated as likely to be realized by reason of the amount of crushed ground. Of the ore available, about 50 per cent. will be washed, with a loss of 33 per cent., so that the total quantity of ore likely to be extracted from these deposits will be as follows:

Mineral originally in deposit	57,000,000 tons.	
25 per cent. loss in working	14,250,000 tons.	
	42,750,000 tons.	
Lump ore 50 per cent. of above		21,375,000 tons.
Washable ore 50 per cent. of above	21,375,000 tons.	
Less loss of 33 per cent. in treatment	7,125,000 tons.	14,250,000 tons.
		35,625,000 tons.
Quantity excavated to end of 1914		13,500,000 tons.
Quantity of ore available for extraction		22,125,000 tons.

This figure differs appreciably from that of 110,000,000 tons on page 38 of the paper.

Assuming that the value of the ore permits 1,000,000 tons per annum to be marketed after peace is declared, the deposits should be able to furnish that quantity of ore for over 20 years.

In addition to the minerals, pyrolusite, psilomelane and wad, mentioned by the author, an appreciable quantity of a dull reddish mineral was mined, which although originally rejected by reason of not being black, was subsequently found to be of better quality than the general run of ore. A complete analysis of some of the first-quality stuff indicated that it was probably manganite ($Mn_2O_3.H_2O$), and as the complete analysis may be of interest, it is given herewith:

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	Per Cent.
Silica	3.02
Manganese peroxide	48.02
Manganese monoxide	36.00
Ferric oxide	0.64
Alumina	0.91
Baryta	0.84
Lime	1.05
Magnesia	0.22
Phosphoric acid	0.337
Arsenic acid	nil
Sulphuric acid	0.542
Oxide of copper	0.03
Oxide of lead	nil
Oxide of zinc	trace
Combined water	8.40
	100.009
Manganese (metal)	58.25
Iron (metal)	0.45
Phosphorus	0.147
Sulphur	0.217

I visited the deposits of the Nicopol district in 1907, and found that manganese ore had been proved to exist over a large area. No calculation even reasonably near the truth can, however, be made regarding the quantity of mineral likely to be contained in the area underlain by manganese ore, inasmuch as the outcrops of the ore are limited in extent, and the details of the thickness and composition of the bed over the area have not, so far as I know, been obtained on a sufficiently comprehensive scale. I am convinced, however, that the quantity of mineral available is very large.

The totals given by Mr. Harder regarding the area of the deposit and the mineral available, apparently taken from the Beyschlag-Krusch-Vogt treatise, represent, in my opinion, but a fraction of the correct figures, and, further, do not appear to be in correct relation with each other, for an area of 20 sq. km. underlain by a manganese-ore bed 1 to 1.5 m. in thickness would not give a total quantity of mineral available, of 7,500,000 tons. Dr. N. Sokolow,[1] who studied these deposits in 1901, shows the manganese-ore area as exceeding 200 sq. km. in extent, and in one deposit that I saw, with an area of over 50 sq. km. in which many test pits and bore holes had been made, the quantity of ore available, estimated by a Russian engineer of standing, was many times greater than the total given by Mr. Harder for the whole zone.

Generally, the mineral excavated consists of 20 per cent. lumpy and

[1] Dr. N. Sokolow: Die Manganerzlager des Gouvernements Jekaterinoslaw, *Russia, Memoires du Comité Géologique*, vol. 18, No. 2 (1901).

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80 per cent. small ore. The establishment of washing plants is general and approximately 50 per cent. is lost in treatment.

At one property of which "the run of mine" contained 34 per cent. Mn, the whole of the output was washed, giving the following results:

	Per Cent.	
First grade.....	50.00	Manganese (metal).
	8.00	Silica
	10.00	Moisture
	0.16	Phosphorus
Second grade.....	40.00	Manganese (metal)
	28.00	Silica
	10.00	Moisture
Tailing.....	20.00	Manganese (metal)
	38.00	Silica

Regarding the Indian deposits, I consider Dr. Fermor's comprehensive treatise from which the author quotes as the best statement regarding manganese ores hitherto published.

The quantity of ore likely to be contained in these deposits is great, particularly in those of the Central Provinces which are so large and numerous, with in many cases enormous outcrops. If Dr. Fermor's theory regarding the genesis of the deposits of the Gondite series be correct, and the ore continues in depth as at surface, the quantity of mineral available will be enormous.

The Sandur and Mysore State deposits of India classed as lateritoid by Dr. Fermor have much in common with those of Rodriga Silva and Ouro Preto in Brazil, mentioned by the author on page 58.

This class of deposit has often resulted, both in Brazil and India, in financial loss, and so possesses interest mostly of a negative character. While the outcrops of these deposits are generally striking in appearance and seem to contain large quantities of merchantable ore, exploration generally proves them to be superficial in character, the manganese ore giving place at a shallow depth to iron ore, and subsequently to the original rock.

The precise origin of the manganese and iron in these deposits, as in all those of a lateritic character, is obscure, but it is generally agreed that a large part of the material is derived from the underlying rock by the action of ground waters, although Dr. Fermor, in order to explain the formation of the larger deposits, suggests that some of the manganese and iron has come from surrounding rocks. Beyschlag[2] points out that in oxidation precipitation the iron is thrown down before the manganese, which explains the presence of the manganese ore on the surface with iron beneath.

[2] Beyschlag, Vogt and Krusch: *Deposits of the Useful Minerals and Rocks*, translated by S. J. Truscott, vol. 2, p. 852 (1916).

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Analyses of Brazilian and Indian types of this class of ore, in the dry, are given below:

	India		Brazil	
	Sandur State, Per Cent.	Mysore State, Per Cent.	Botafogo, Per Cent.	Tres Cruses, Per Cent.
Manganese (metal).....	40.92	38.24	41.23	37.25
Iron (metal).....	14.95	16.85	16.23	19.42
Silica.....	1.50	10.40	3.11	5.40
Phosphorus.....	0.002	0.184	0.193	0.102

With regard to the Miguel Burnier deposit, the late O. A. Derby held that the manganese was derived from a limestone and suggested that the associated earthy ores, as he termed them, were residual from limestone. Certainly the limestone which did not show at grass and was some 15 m. from the deposit about 50 m. below surface, at a depth of 150 m. was in contact with the manganese ore bed, which was much thinner, and a similar condition was observed at Rodeio.

The extraordinary development of the Morro da Mina mine is an example, somewhat rare, of a property proving to be much more valuable than suggested by surface indications. This area remained for some years undeveloped after manganese mining was initiated in Brazil, principally because the lateritic covering of the hill consisted in great part of a partly altered spessartite rock, a siliceous manganese ore which was also found in an exploratory crosscut some 50 m. in length, driven into the hill near its base, at some remote period. In 1900, 10 years after the deposit was discovered, crosscuts were made on the hill in several places at a depth of some meters, below the lateritic cap, and the huge lenses, since developed, were discovered. Derby suggested that they were the oxidized portions of a manganese carbonate similar to that which produced the deposit of Piquery, and which differed from the more siliceous and resistant mineral-containing spessartite.

Had the explanatory crosscut at the base of the hill been continued for several meters, it would have cut one of the lenses at present being so profitably worked.

I saw the Bahia deposits some years ago, and at that time the Onha property was the principal producer. Indeed, more mineral has been quarried from it than from the Pedras Pretas property mentioned by Mr. Harder. Up to the end of 1908, I estimated that about 70,000 tons of ore had been got from these deposits and I have not heard that this total has since been appreciably increased. The Onha deposit consisted of a vertical lense, 150 m. in length, and a maximum width of 25 m. It was composed of a mixture of merchantable ore and a partly altered man-

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ganese garnet rock. In depth this latter appeared to be increasing in quantity and the better ore was more difficult to obtain.

The war prevented me from contributing to the discussion on Mr. Harder's paper on The Iron Industry of Brazil, read in October, 1914, but I would ask now to be permitted to express my appreciation of the excellent work of Mr. Harder and other American engineers in the study of these deposits.

I do not, however, agree with the author when he asserts that the massive iron ores were laid down as found today and practically ignores the possibility of the massive ore having been formed by alteration from the general average of the iron formation.

No one visiting the iron-ore district can fail to notice that great changes must have taken place in the character and composition of the rock, and which are indeed still going on, although in an infinitesimal degree compared with former times.

This is recognized to some extent by the author, and his statement that the laminated ore is altered to a greater depth by surface agencies, elimination of silica, hydration, etc., than the hard ore, suggests the possibility of the massive ore having been formed in the same way, and afterward suffering dehydration.

So extensive and numerous, however, are these deposits that it is obvious to anyone seeing them that they contain enormous quantities of ore. Hence the question of their origin has little economic interest for the present generation.

Notwithstanding the evidence of a large number of analyses of samples, almost all of which are probably from outcrops, I am of opinion that these ores will be found to contain, when exported in quantities, less iron owing to hydration, and more phosphorus than is generally supposed, and in this resemble the Chilean ores, of which Mr. Harder says that a large proportion will be found to be of non-Bessemer grade.

With regard to the transport of the ore, I believe it could be carried on the Central Railway, if the line were improved and an endeavor made to handle it.

I do not consider that the Brazilian Government will assist the iron-ore export business (and it will require sympathetic treatment) to the exclusion of the establishment of steel manufacture in the country, even though as yet no coal suitable for coke manufacture has been found in Brazil.

The old rule that ore should be taken to fuel rather than the reverse is not now strictly applied anywhere—for have not iron works been constructed at Duluth to utilize empty Lake ore boats as fuel-carriers, and, further, has not the Gary plant been constructed between the ore and fuel, and following the industrial centers moving west?

Contemporaneously with the initiation of the export of iron ore from

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Brazil, a commencement should be made with steel manufacture in the country. It would be easy to introduce the fuel necessary as return freight in iron-ore boats. (The success of steel works in Japan, Canada, British India and Australia will act as an incentive to Brazilians insisting that some steel should be manufactured in the country when the iron ore exports commence.)

J. T. SINGEWALD, JR., Baltimore, Md.—I might say a word further in regard to the Miguel Burnier district. I am sorry Mr. Harder is not present at this meeting. I had hoped to be able to ask him a few questions in regard to some of the features of those deposits.

This Miguel Burnier district is the other district that I referred to that lies in the State of Minas Geraes. The interpretation that Mr. Harder gives to these deposits is the same as that given by Harder and Chamberlin in a previous paper, and that is that they consist of lenticular deposits of sedimentary origin in a series of rocks that also includes lenses of limestone, but they seem to think that the manganese ores occur at a definite horizon and at a horizon different from the limestone. We found that the manganese ores occur at several horizons, as also do the limestone lenses, and the limestone masses are of the same shape as the manganese orebodies, so that our own feeling was that these manganese ores simply represent concentrations within the zone of oxidation formed as a result of decomposition and replacement of this limestone.

To the east of Miguel Burnier there is a big bend in the railroad which cuts through, at right angles to the strike, this series of rocks. Here is shown very well both the local character of the limestone lenses and the local character of the manganese lenses in that, though the distance across the bend is only a couple of hundred yards, the limestone and manganese lenses do not match up at all, and it also shows plainly that both occur at more than one horizon. Consequently there is no reason why the manganese ore could not represent alterations of this limestone. The mine workings do not as yet give very definite information on this point in that they are all comparatively shallow and within what would be the zone of oxidation, so that what they will grade into below that depth, we do not know.

On the other hand, the character of the ore suggests that it may represent such a product in the zone of oxidation in that it tends to be quite earthy; whereas, if the manganese had been there originally as such it would have undergone the same metamorphism that the other rocks have undergone and one would have expected that it would have been converted into a hard manganese ore that would today stand out pretty much as do the iron ores of that same region and not have the soft, earthy texture which is a prevailing characteristic.

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E. C. HARDER (communication to the Secretary*).—I am very glad Mr. Singewald has brought up the question of the origin of the bedded manganese-ore deposits in the Miguel Burnier region. My descriptions of these deposits have been so brief that I did not feel justified in saying much about their origin. It has been mentioned, however, that one main manganese-ore bed is found in the district and that possibly other horizons carry manganese ore. It has been stated also that limestone has not been found at the main manganese horizons but that it occurs elsewhere in the sedimentary series. Many persons who have seen the deposits, among them O. A. Derby and H. K. Scott, are of the opinion that they were formed by the weathering and replacement of beds of carbonate rock. The view, as Mr. Singewald says, is based mainly on the fact that the ore occurs in the same sedimentary series with lenses and beds of manganese-bearing carbonate rock (or limestone) and that it is prevailingly soft. Mr. Chamberlin and I have come to a somewhat different conclusion for a number of reasons.

1. Carbonate rock or limestone is found outcropping at the surface in the immediate neighborhood of the places where the manganese-ore bed crops out. The manganese-ore bed, on the other hand, has been mined by underground methods to a considerable depth and no carbonate rock has been encountered. There is no apparent reason why weathering and replacement should have occurred to such a depth along this particular bed while other carbonate-rock beds in the vicinity were almost untouched. If it is a replacement deposit, might not one expect to find carbonate rock within a reasonable distance of the surface somewhere along the extent of several miles that the bed has been developed?

2. The ore comprising the manganese bed, although soft, does not have the consistency one would expect a replacement deposit of limestone or an oxidized decomposed manganese carbonate bed to have. In replacement deposits the manganese ore usually shows geodal, nodular, and concretionary forms. Some residual impurities from the limestone, such as clay or silica, are generally present. Manganese ore derived by the alteration of manganese carbonate is commonly earthy and porous. Material of this type is of common occurrence throughout the manganese-bearing areas near Miguel Burnier and Ouro Preto, resulting from the surface decomposition of carbonate rock layers associated with the sedimentary series. Mr. Singewald mentions these occurrences as tending to show that manganese ore occurs at several horizons. The ore in the main manganese bed is generally bluish black and finely crystalline and is more or less compact and uniform. It consists of very pure manganese oxide. The bed is not hard and does not crop out prominently as many of the iron-ore beds do because the manganese minerals composing it are by nature soft. This is true of sedimentary manganese-ore beds in most

* Received Mar. 7, 1917.

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parts of the world. Many of the iron-ore beds of Minas Geraes, on the other hand, are composed of hard hematite which resists weathering. However, even in the case of iron ore, it is well known that by far the largest and most numerous deposits of this region consist of soft "jacutinga" ore (soft crystalline hematite) which does not crop out.

3. Most of the so-called limestone beds of this region are not limestone but consist of a mixture of carbonates of calcium, magnesium, iron, and manganese. If such a bed were replaced by manganese oxide the calcium and magnesium would be carried away but the iron would tend to remain behind as hydrated oxide and would form an appreciable proportion of the manganese bed, which is not the case in the Miguel Burnier district. The manganese oxide may have replaced a bed of pure limestone, but that is merely speculative. Limestones as a rule are not so pure that they do not leave residual material such as clay and iron oxide behind.

4. The manganese-ore bed occurs regularly interlayered with the other sedimentary rocks and behaves like them in every respect. On one side is schist and on the other side is a bed of itabirite. Between the itabirite and the manganese-ore bed and directly in contact with the latter there is in many places a layer of soft crystalline hematite which is believed to be an original sediment. This close association would tend to indicate that the manganese-ore bed also is an original sediment.

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Geology of the Iron-Ore Deposits of the Firmeza District, Oriente Province, Cuba

BY MAX ROESLER, PH. D.,* SANTIAGO DE CUBA, CUBA

(New York Meeting, February, 1917)

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I. INTRODUCTION

THE following article concerns the geological occurrence of the iron-ore deposits on the south coast of Cuba. The article is based on a detailed field study, made in the hope that some information would be gained which might be of value in the search for further orebodies, or in the economic development of the ore already found.

LOCATION

The iron-ore deposits of the Firmeza district lie near the coast, in the southeastern part of Cuba, in the Province of Oriente. The orebodies of this district form part of a belt of deposits that extends from Sigua, 25 miles east of Santiago, to Sevilla, 5 miles east of Santiago, and lies on the seaward slope of the Sierra Maestra range of mountains.

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This range roughly parallels the coast in the southern part of Oriente Province. Its crest is about 6 miles north of the mines near Firmeza. The town of Firmeza lies 9 miles east of Santiago, and about 2½ miles from the Caribbean Sea. The mines included in the Firmeza district are the Ocania Mine and a group of mines that extends from West Five Mine to the Concordia Mine. The elevation of the mines is from 400 to 1,000 ft. above sea level. Their exact location is shown on the map (Fig. 1) which is a copy of a map furnished by the Juragua Iron Co.

Immediately east of the Firmeza district lies the Daiquiri district of the Spanish-American Iron Co.

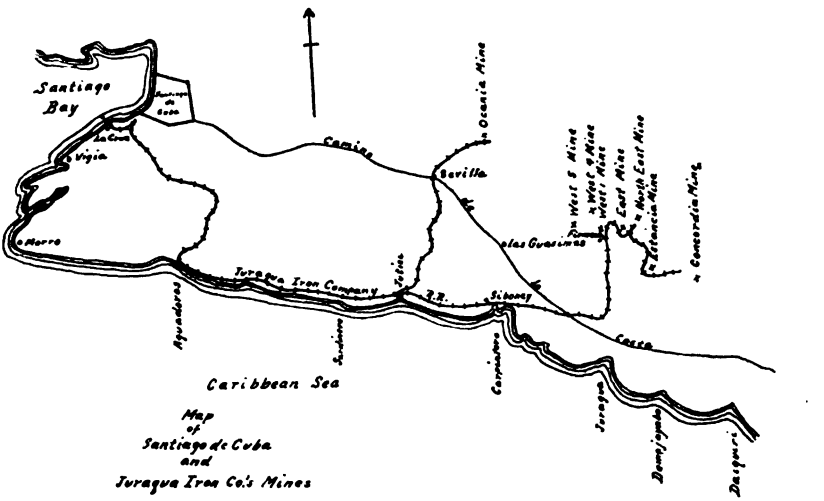

FIG. 1.

SCOPE OF WORK AND ACKNOWLEDGMENTS

Detailed examinations have been made by J. P. Kimball,[1] A. C. Spencer,[2] and J. F. Kemp,[3] and many others have contributed to our knowledge of the deposits. The writer has made use of all the available information and will endeavor to give full credit for it.

The present article has been written under the first award of the S. F. Emmons Memorial Fellowship, and the writer takes this occasion to thank the committee in charge of the fellowship for their assistance and criticism. Thanks are also due to the Juragua Iron Co. for permission to make the examination, for the use of their maps, and for their kindness in giving access to all the available data and facilities. The Spanish-American Iron Co. also made possible the visiting of neighboring

[1,2,3] See bibliography at end of paper.

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properties and most grateful acknowledgment is hereby made to the officials and staff of both companies.

The Geological Faculty of Yale University, and Professor C. P. Berkey of Columbia University, have been freely consulted on different phases of the problem, and their advice has been of much service.

History and Mining

The Firmeza orebodies have been worked continuously since 1884, except for a short period during the Spanish-American war, and the total production of the mines, from 1884 to 1913 inclusive, has been 6,776,171 tons.[4] The mines of the Firmeza district belong to the Juragua Iron Co., which is now controlled by the Bethlehem Steel Co., though it was at one time also allied with the Pennsylvania Steel Co.

The mining of the ore is done by hand, the stripping by steam shovel. Where the orebody is small or intimately associated with waste, it is found advantageous to lease the workings to contractors and pay for the ore on the basis of tonnage and grade, delivered in mine cars.

The ore from the mines is put through the crusher at Firmeza, from which the major part is shipped direct to La Cruz, the company's shipping point on Santiago harbor. A small part of the output, high in sulphur, is roasted in the valley south of Firmeza, near Siboney, before it is shipped to La Cruz.

II. TOPOGRAPHY AND ITS INTERPRETATION

The topography of the Firmeza district falls naturally into three main divisions which have a lateral extent parallel to the sea coast and the main range of the Sierra Maestra,—that is, in an east-west direction. Beginning at the coast there is a range of terraced cliffs rising to uniform height of about 300 ft. The terraces all face the south, to the sea, and appear to be of wave-cut origin, excavated in flat-lying coral limestone. There is no beach, the sea beating directly against the limestone cliffs, except at the mouth of the Rio Carpintero at Siboney, where there is a small beach made up of coral fragments and an arkosic sand in which feldspar predominates.

Back of the terraces there is a fairly gentle landward slope to a silt-covered, flat, east-west valley. In this valley the streams from the mountains are generally lost in lagoons and swamps. Some of the larger ones, such as the Rio Carpintero, find their way to the Caribbean through comparatively narrow gorges cut in the cliffs that border the sea. These streams appear to be of an intermittently torrential type. In the dry season they show very small streams of water flowing through stream beds covered with boulders up to several tons in weight.

[4] D. B. Whitaker: *Engineering and Mining Journal*, vol. 97, p. 677 (1914).

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North of this valley rise the foothills of the Sierra Maestra—sharp, steep hills, covered as a rule with dense forest growth. They are connected with the main range of the mountains by ridges that have steep slopes and narrow crests. The connecting ridges are less heavily forested than the foothills but are covered with high grasses and an occasional grove of trees. The mountains themselves are thickly covered with pines and rise to an elevation of about 3,500 ft.

The limestone cliffs, with their sea-cut terraces, have been interpreted as evidence of periodic movements of the island, but it seems to the writer that the widespread occurrence of a terrace about both Jamaica and Cuba at the same elevation above sea level indicates a movement of the sea surface, rather than of the land.

In speaking of the Seboruco (the local name for the coastal limestone) R. T. Hill says:[5]

"Nowhere have I seen the elevated reef rock folded or otherwise disturbed except by the gently sloping coastward inclined elevation it has undergone.——The Seboruco as a whole represents a recent and uniform elevation of the whole periphery of the island——."

The following terrace elevations have been taken from the report of C. W. Hayes, T. W. Vaughan, and A. C. Spencer,[6] and arranged in tabular form by the writer:

Havana, Feet	Matanzas, Feet	Gibara, Feet	Baracoa, Feet	Manzanillo, Feet	Santiago, Feet
4–5	5–6	5–20	5–6	5–20	20
10–15	30	40	90	100	100
100	140	100	250	200	280
200	200	150–180	.....	.....	.....
.....	300	.....	.....	.....	.....

R. T. Hill[7] in the report on Jamaica has given less definite figures for the terrace elevations, and the following conclusions:

"In general the old reef rock of Jamaica consists of three distinct formations occurring at three levels, 70, 25, and 10 ft. (or less) respectively. From the persistency of these three levels on the north, east and southwest end of the island, it is evident that their present position above the water is due to continuous epeirogenic elevation after the present outlines of the island had been chiefly defined."

For Hayti and Porto Rico no exact data are available to the writer

[5] R. T. Hill: Notes on the Tertiary and Later History of the Island of Cuba. *American Journal of Science*, Ser. 3, vol. 48, p. 203 (1894).

[6] *Op. cit.* (in Bibliography), pp. 18, 19.

[7] R. T. Hill: Geology of Jamaica. *Bulletin* of the Museum of Comparative Zoology at Harvard, vol. 34, pp. 92–100 (1899).

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The data quoted show that on the north, northeast, and southeast coast of Cuba, and on the north, east, and southwest coast of Jamaica, there is a marine terrace from 5 to 20 ft. in elevation. It seems to the writer that this evidence might suggest that, at least in part, the differential movements of land and sea in Cuba have been due to movement of sea level rather than of land surface. The slight variations in elevation would then be explained by unequal erosion of the gently sloping surface of marine planation. The slight seaward slope of the coast limestone, in the neighborhood of Santiago, is no greater than is to be expected of a near shore surface of marine planation.

There is further evidence of emergence of the island, in respect to sea level, in the tuff-limestone found at an elevation of about 1,400 ft., and the steep-sided, sharp, mountain topography points to fairly recent rejuvenation.

The silt-covered, east-west valley, in which the streams are aggrading, suggests that the last movement of land with respect to sea level has been a downward one, and this is in accord with the evidence of the harbors of Santiago and Guantanamo, which have been interpreted as bays formed by drowning.[8]

The topography seems to be almost entirely independent of the lithology. An exception to this is the coarse crystalline marble that forms a capping to many of the foothills. Apparently the massive marble offers greater resistance to tropical weathering than do the fine-grained, diabasic or dioritic rocks which marmorized it, for the arroyos are cut into the latter. The occurrence of marble as a capping is too frequent to be entirely fortuitous.

III. PETROLOGY

Sedimentary Rocks

Description

The sedimentary rocks are represented in the Firmeza district entirely by limestones. At the coast, and rising in three sea-cut terraces to an altitude of about 350 ft., are recent limestones. According to the report of Hayes, Vaughan, and Spencer,[9] this rock is

> "replete with the remains of numerous species of corals which are all, so far as examined, at present living in the surrounding Antillean seas."

There seems to be one rather striking point of difference between the higher landward terraces and the lower terrace bordering the Caribbean. This is, that while all three terraces contain coral remains, the landward

[8] Hayes, Vaughan, and Spencer, *Op. cit.*, p. 17.
[9] *Op. cit.* pp. 23–24.

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terraces are almost entirely massive, while the seaward terrace is made up of more loosely cemented coral remains. On the land, where the limestone comes into contact with the underlying igneous rock, there are boulders of rock and of iron ore cemented by the lime, showing that erosion of the orebodies had begun before these coral rocks were formed.

In the foothills of the Sierra Maestra, which vary in altitude from 600 to 1,300 ft., there occur, usually as a capping on the hilltops, bodies of massive limestone now largely marmorized. Bedding is almost entirely lost, but where thinner masses have been involved in the volcanic rocks it is possible to discern a pitch toward the southeast at an angle of about 30°. The freshest pieces of this limestone, taken from boulders on the north side of the hill north of West Five Mine, show it to be a blue, dense, fine-grained limestone. The microscope shows no evidence

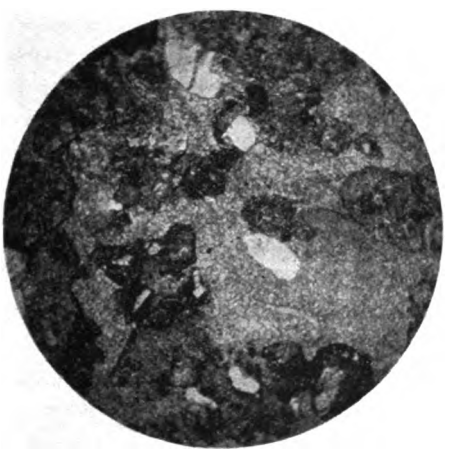

FIG. 2.—TUFF-LIMESTONE FROM RIDGE CONNECTING FOOT HILLS WITH MAIN RANGE OF THE SIERRA MAESTRA. Limestone—groundmass. Plagioclase fragments—white. Volcanic rock fragments—dark. × 18.

of organic remains in the thin section, but only a granular aggregate of calcite.

On one of the ridges connecting the foothills with the main range, at an elevation of about 1,400 ft., an unaltered limestone of unusual type was found. This rock has the deeply pitted surface typical of the weathered outcrops of all the limestone in this region. The color of the rock on a fresh fracture surface is blue-gray. Closer examination shows minute areas with a vitreous luster embedded in a granular calcite matrix, giving to the rock the appearance of a porphyry. A thin section of the rock shows that the crystals are fragments of plagioclase, of about andesine-labradorite composition. There are also angular fragments of a diabasic rock (Fig. 2). The fragments are all about 0.5 mm. in diameter, and their presence in the limestone shows that the sediment was

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formed at a time of volcanic activity on the adjacent land. The perfect freshness of the feldspars is probably due to the fact that the sea water was already saturated with calcium carbonate, and so was not chemically active in so far as the calcic feldspars were concerned. The ferromagnesian minerals of the diabasic fragments are entirely altered. This tuff-limestone seems to be very similar to that described by C. P. Berkey[10] among the "probably" pre-Tertiary rocks of Porto Rico.

Age

The nature of this rock shows that at least part of the volcanic activity was contemporaneous with the sedimentation. As the andesitic rocks accompanying this tuff-limestone are believed to represent the initial stages of the igneous cycle, the determination of the age of the sediment must also fix the age of the igneous rocks.

The age of these limestones and the associated volcanics has been a rather vexed question. J. F. Kemp speaks of an article by H. Wedding,[11] in which he places them in the Jurassic, in the horizon of Quenstedt's Beta, of the upper White Jura. This determination was made upon material furnished by G. W. Goetz. The only other definite information on this subject of age is that given by J. T. Singewald, Jr., and B. L. Miller.[12] They found a fossiliferous limestone in the Daiquiri district, and submitted some of the fossils to T. W. Vaughan, who correlated one of the species with the Cretaceous of Jamaica and determined the age of the limestone as "Mesozoic, probably Cretaceous."

W. Lindgren[13] says:

"The idea of the geologists who have done the most work in this section seems to be that the lavas and tuffs and associated limestones are of Eocene age——."

R. T. Hill,[14] speaking of the volcanic clastics of the Jamaican Blue Mountain Series of Cretaceous age, says:

"In Cuba these clastic rocks constitute the high divides of the Oriente——."

Experience during historic time shows that it would not be justifiable to infer volcanic activity in one of the islands of the Antilles on the basis of such activity in another. In the present case there is, however, the evidence of Cretaceous sedimentation in Jamaica determined by R. T. Hill, the direct correlation of the Daiquiri specimen with the Jamaican

[10] C. P. Berkey: Geological Reconnaissance of Porto Rico. *Annals of the New York Academy of Sciences*, vol. 26, p. 20 (1915).

[11] H. Wedding: Die Eisenerze der Insel Cuba. *Stahl und Eisen*, vol. 12, No. 12, pp. 545–550 (June 15, 1892).

[12] J. T. Singewald, Jr., and B. L. Miller: The Genesis and Relations of the Daiquiri and Firmeza Iron-Ore Deposits, Cuba. *Trans.*, vol. 53, pp. 67–74 (1916).

[13] W. Lindgren and Clyde P. Ross: The Iron Deposits of Daiquiri, Cuba. *Trans.*, vol. 53, p. 41 (1916).

[14] R. T. Hill: The Geology and Physical Geography of Jamaica. *Bulletin* of the Museum of Comparative Zoology at Harvard, vol. 34, p. 170 (1899).

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fauna by T. W. Vaughan, and R. T. Hill's reference to the clastic rocks of Oriente. E. T. Hodge has informed the writer that he found Comanche fossils among the pre-Tertiary.

Igneous Rocks

The igneous rocks of the Firmeza district form a natural series of differentiation products from a basic magma. They range from a fine-

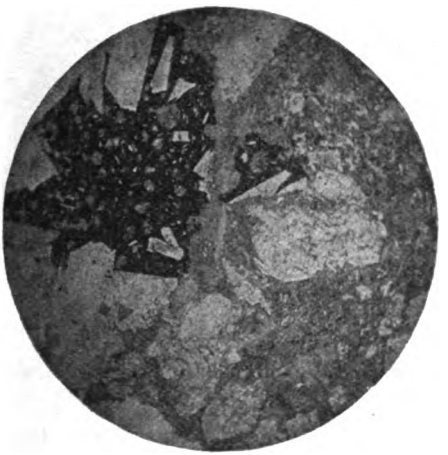

Fig. 3.—Diabase Porphyry with Included Fragment of Earlier Volcanic Rock. West Four Mine. × 18.

Fig. 4.—Diabase Porphyry with Epidote-Filled Amygdule. From Railroad Cut Opposite West One Mine. × 18.

grained diabase to a highly quartzose aplite. J. F. Kemp[15] has given a discussion of the nomenclature used in former articles on the district.

[15] The Geology of the Iron-Ore Deposits In and Near Daiquiri, Cuba. *Trans.*, vol. 53, pp. 3–38 (1916).

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For purposes of mapping, the writer divides the igneous series into four groups:

1. The diabasic rocks.
2. The dioritic group.
3. The granitic group.
4. The later dike rocks.

FIG. 5.—DIORITE FROM LEVEL 1 OF WEST FIVE MINE. CROSSED NICOLS. × 18.

FIG. 6.—QUARTZ DIORITE FROM EAST MINE. THIS IS THE BASIC MEMBER OF THE GRANITIC SERIES. CROSSED NICOLS. × 18.

Diabasic Rocks

The diabasic rocks are fine-grained, as a rule, and porphyritic. They are the representatives of the original magma. Megascopically they are dense, dark rocks. Under the microscope they show as felty aggregates of plagioclase laths with interstitial ferromagnesian minerals. They are

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usually fragmental (Fig. 3) and frequently show epidote-filled amygdules (Fig. 4).

These diabasic rocks show a bedded structure in places, with intercalated limestones and are undoubtedly in part extrusives.

FIG. 7.—GRANITE SHOWING MICROGRAPHIC QUARTZ FELDSPAR INTERGROWTH ABOUT PLAGIOCLASE NUCLEUS. FROM THE JURAGUA VALLEY. CROSSED NICOLS. ×18.

Dioritic Rocks

The wall rock of many of the orebodies is a gray, fine-grained, and

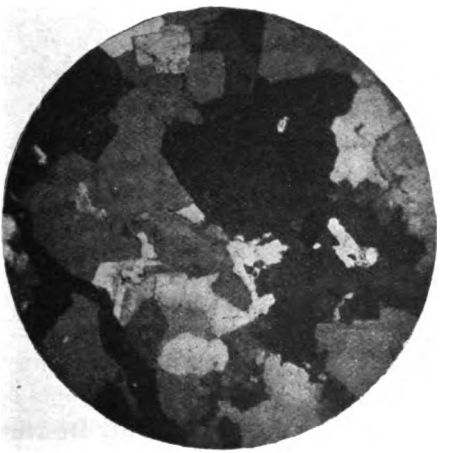

FIG. 8.—SPECIMEN B FROM DIKE IN WEST SIDE OF WEST FIVE MINE. ELEVATION 520 FT. NOTE CORROSION OF PLAGIOCLASE AND DOMINANCE OF QUARTZ. CROSSED NICOLS. × 18.

even-textured rock. Plagioclase, varying from labradorite to bytownite, is the most abundant mineral, with much hornblende between the feldspars. On the basis of the feldspars it is a gabbro, but in the older

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classification the hornblende makes of the rock a diorite. Since that terminology has been adopted by earlier writers on the district it is accepted here. Fig. 5 shows a typical diorite.

The diorites and diabases are frequently found merging into each other without any visible contact. Both are the hosts of the orebodies.

Granitic Rocks

J. F. Kemp[16] has used the term granite for the quartz-bearing diorites and the true granites "to avoid all confusion of this rock with the diorites which are associated with the ore." That usage is adopted in this paper, and the scope of the term granitic rocks is enlarged to include the aplites and quartz porphyries. This inclusive usage seems justified by the fact that the quartz-bearing rocks form a group later than the ore-bearing rocks, though pre-mineralization.

All the rocks of this group are hornblende-bearing except the most acid aplites, and even the most acid show very little potash feldspar. The basic members of the series are gray, coarse-grained, feldspathic rocks (Fig. 6) while the acid members are made up of striking feldspar quartz intergrowths (Fig. 7), or else are fine-grained quartz aplites (Fig. 8). The extremely rapid variation in chemical composition is shown by three microscopic analyses of rocks from the same dike at different elevations and ascending order.

	Feldspar	Quartz	Ferromag.	Magt.
A.....	61.8	28.0	5.5	4.7
B.....	54.0	41.6	3.4	1.0
C.....	52.7	45.2	1.1	1.1

Later Dike Rocks

The entire mineralized area is cut by basic dikes that vary from basalt to andesite. They are post ore, but usually carry pyrite. A detailed description would serve no definite purpose in this connection.

IV. AREAL GEOLOGY

Rock Types Found in the District

The study of the petrographic features shows that there are several types of igneous rocks and two limestone formations in the Firmeza district. The igneous rocks form a continuous series from diabasic extrusive and intrusive rocks, through diorite and granite, to highly acid

[16] *Op. cit.*, p. 12.

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aplites. This series is cut by dike rocks of various types, which range in composition from basalt to andesite. The sedimentary rocks fall naturally into two divisions—earlier marmorized limestone, probably Cretaceous in age, and now exposed only in scattered outcrops, and younger recent coral limestones deposited as a fringe on eroded igneous rocks.

Distribution of the Various Rock Types

Surface Distribution

The two appended maps show the surface distribution of the rocks and orebodies. Owing to the lack of topographic maps, and the difficulties attending work in an area covered by tropical verdure, they cannot lay claim to great accuracy of detail. Future work will undoubtedly shift the contacts in many places and add a number of dikes. But enough work was done, and enough outcrops plotted to justify the making of the maps and the belief that they show the general relationship of the formations. Acknowledgment is made to Dean Corsa, whose unpublished map in the possession of the Juragua Iron Co. was in part used.

The different formations lie, as shown by the map, in belts of irregular width, roughly parallel to the coast line.

Nearest the sea is the belt of coral limestone. This belt is about 3,000 ft. wide in the area mapped. To the west, where the railroad runs to the Ocania Mine, the fringing limestone extends inland nearly 2 miles. Apparently the width of the belt is largely determined by the slope of the pre-deposition erosion surface, the wider belt lying on the more gently sloping surface.

North of the coral limestone belt, the main mass of granitic rocks is exposed. This forms a belt from 6,000 to 8,000 ft. wide. Most of the granite area is covered by an alluvial deposit, through which rise knobs of the igneous rock. These knobs are all granite, usually with inclusions of dioritic material.

North of the granitic area lies the diorite. This forms a very irregular belt on the lower part of the foothills of the Sierra Maestra. From the Demajayabo River south of the Concordia Mine, to south of Loma Alta, the width of diorite is from 1,200 to 3,000 ft. At West Five Mine the diorite area runs back into the hills north of the region mapped. In the Juragua valley the diorite is exposed about 1 mile north of Firmeza.

The diabasic rocks lie higher in the hills than the diorite and in general their exposures on the surface are north of the diorite. No attempt has been made to show on the map the contact between the extrusive and intrusive facies of the diabases. This contact is exceedingly indefinite and rarely distinguishable.

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Bodies of granitic rock, that differ from the main granite mass, cut through the diorite and the diabases in the region mapped. These bodies are apophyses of the main granite massif and form dikes, sills, or irregular bosses. They lie in a roughly east-west belt in the foothills, and are of great importance because the orebodies are scattered irregularly as a fringe to these apophyses. This can be seen clearly both on the map of the district and on the larger scale map of the mines near Firmeza.

The older limestones lie in the diorite and diabase area, rarely in contact with the granite. They have been metamorphosed to a dense marble in the area mapped, and are preserved as irregular masses capping the foothills, or as beds interbedded with the extrusive rocks. The outcrops vary in size from small blocks to areas that are as much as 1,200 ft. in diameter. An even larger area at the Ocania Mine does not show on the map.

All the formations are cut by the later basic dikes. Except in the mines these dikes have not been mapped. They fall into two systems: one almost vertical, striking northerly, or slightly east of north; the other an almost horizontal system, that might equally well be classed as a system of sills. Petrographically there is no difference between the two systems.

Vertical Distribution

The vertical arrangement of the different rock types is even more marked than their linear distribution, as it shows on the surface maps.

The coastal limestones rise in terraces to an elevation of about 300 ft., where they terminate in a flat top.

The granite massif has an upper surface which rises gradually from an elevation of about 350 ft. at the eastern end of the district, to about 700 ft. at the Ocania Mine. The granitic apophyses, as now exposed, are rarely more than 150 ft. above the top of the granite massif, and their downward extension where exposed by erosion can be seen to merge into the granite.

The diorite and the diabasic rocks must be considered together, because, while the lower limit of the diorite is determined by the top of the granite intrusion, the upper limit is, as a rule, indeterminate, and the diorite merges into the diabases. The two rocks were mapped separately in the field and a contact drawn, but it is generally arbitrary. More rarely the contact mapped represents an observed intrusive contact. As established, the contact between diorite and diabase rises from 500 ft. elevation in the eastern to 800 ft. at the Ocania end of the area. Fragmental igneous rocks are found at 700 ft. in the East Mine, and at 1,000 ft. elevation at Ocania. No continuous contact between intrusive and extrusive diabase was mapped.

The older limestone masses have not been found in this district, at

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a lower altitude than 400 ft. The highest body of limestone examined was the tuff-limestone found at 1,400 ft.

The lack of contour maps makes it difficult to draw accurate sections. But the elevations can be tabulated as follows:

West End of District (Ocania Mine), Feet		East End of District Feet
1,000	Fragmental igneous rocks	700
800	Diorite-diabase contact	500
700	Top of granite massif	350
300	Top of coastal limestone	300

This table shows the distinct vertical distribution of the igneous rock types, and a well-defined pitch of their surfaces of contact.

Causes of Present Areal and Vertical Distribution of the Rock Types

Faulting.—The linear distribution of the rock types, as shown on surface maps, suggests at once a direction of major faulting. But except for two minor faults in the East Mine, the one northeast-southwest, the other northwest-southeast in strike, and a somewhat larger east-west fault in West Five Mine, no displacements worthy the name of fault were found in the area. The nearest approach to a fault system is shown by the later basic dikes. These, with the one vertical north-south trend and a horizontal system, appear to have filled a definite fissure system, but there is no evidence of any appreciable movement along most of them. In West Five Mine an aplitic dike is offset 6 in. on the opposite sides of a 2-ft. wide basic dike. If the dikes do represent an older fissure system, it is a system due to contraction during cooling of the igneous rock, rather than to faulting. The Firmeza district must be added to the long list of those in which ore deposits are connected with fissures of minor or no displacement.

It is the writer's opinion that the present areal and vertical distribution is due to three different causes—magmatic differentiation, tilting, and erosion. Each of the three must be considered in an attempt to solve the problem.

Magmatic Differentiation.—To show that the vertical distribution of the rocks is due to magmatic differentiation it will be necessary to establish the comagmatic origin of the igneous rocks and their age relationship.

The detailed petrographic study has shown that there are present in the district igneous rocks showing all the stages of a gradual transition from diabase porphyry to highly acid aplites. The merging of the types can also be found in the field.

Northeast of Estancia hill the transition of a granite-porphyry sill into the main granite and quartz-diorite massif can be followed. There is no contact between the two types. The granite massif itself contains numerous inclusions of more basic material. These inclusions are an-

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gular, subangular, or rounded. J. F. Kemp[17] found apparently similar inclusions in the Daiquiri district and his interpretation is:

"Apparently the inclusions represent some older solidified rock."

With this interpretation the writer does not feel in entire accord. The inclusions certainly represent early differentiates, either early crystal

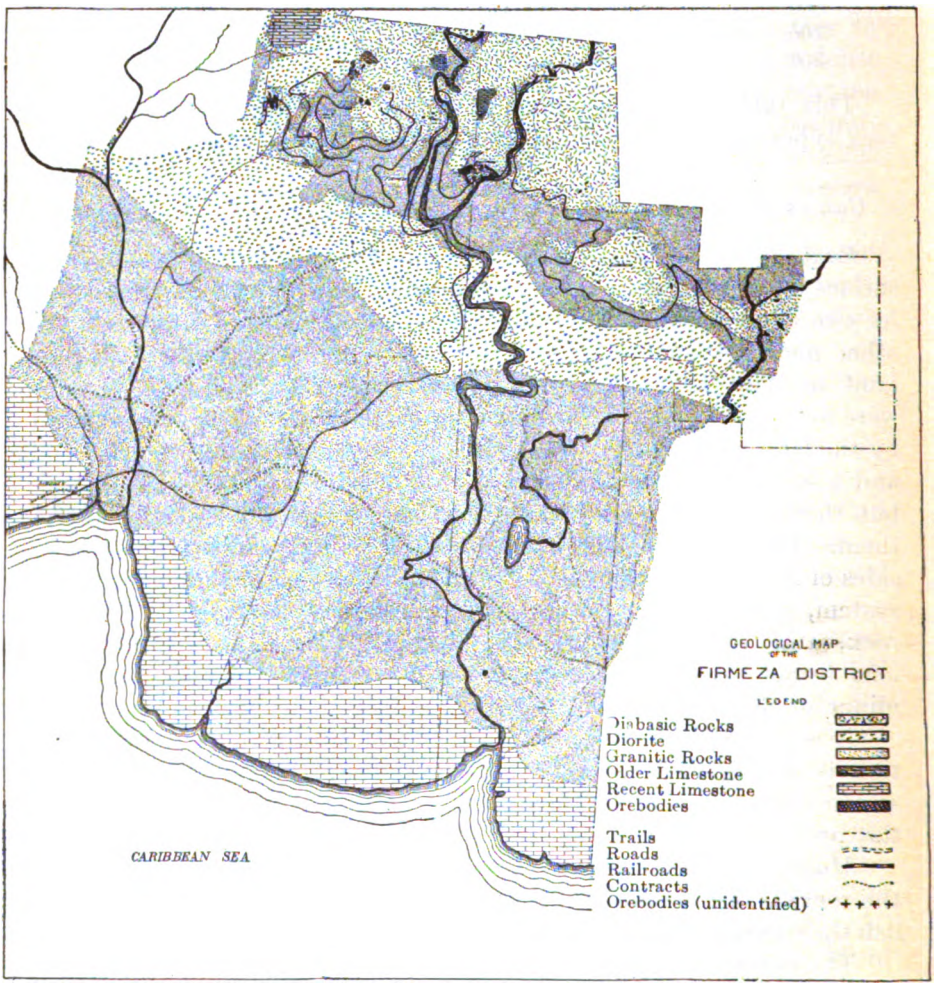

FIG. 9.

accumulations, or possibly segregations in the still liquid state which had an opportunity to crystallize; but either way they are more or less reabsorbed portions of the magma itself, and so endogenous inclusions.

[17] *Op. cit.*, p. 16.

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After initiation of crystallization the character of the magma changed until it had power to reabsorb part of the already crystallized material. The result of the same action was observed on a small scale in a microphotograph of a specimen from the lower part of the aplite dike in West Five (not reproduced in this paper). It shows a zonal plagioclase, calcic in the central part, sodic at the edges, and much corroded. Obviously the plagioclase must have formed before the liquid was of such a composition that it could corrode it. Similarly it seems possible that, in a slow-cooling magma, crystals form and gather in clusters, and the interstitial liquid, changing in composition, could reabsorb them more or less completely.

Beside the evidence of the endogenous inclusions, the transition from quartz-bearing to quartz-free diorite can be observed in the upper levels of West Five Mine, and that from diorite to diabase in West Four Mine. That the extrusive diabases and intrusive diabases are of the same magmatic origin can be seen from the complete similarity of the two rocks both megascopically and microscopically. The only difference is the greater fineness of the groundmass in the volcanics.

Other features that seem to point toward comagmatic origin of the igneous rocks are the almost total absence of orthoclase in the entire series, and the predominance of hornblende as a ferromagnesian constituent. The latter is considered an indication of the presence of abundant crystallizers which would aid in holding the magma fluid at comparatively low temperature.

The age relationship of the igneous rocks in a country lacking in sediments that are chronologic guides must be determined by intrusive contacts. The intrusive nature of the granitic apophyses in the diabases and diorites can be established in almost any of the mines. That the main granite massif is later than the diorite is well shown in La Posa brook just north of its junction with the Rio Carpintero, where dikes of massive, even-grained granite cut the diorite. Diorite cutting diabase is seen at the contact of the two rocks in the Juragua valley north of the mines. Diabase containing fragments of the volcanic diabases is found all through the district.

The order of formation is, then, the normal one from the basic to the acid end of the series; *i.e.*:

1. Diabasic extrusive.
2. Diabasic intrusive.
3. Diorite.
4. Granite.
5. Aplite.

The origin from a common parent magma and the age relationship established, it can be shown how this would account for the observed

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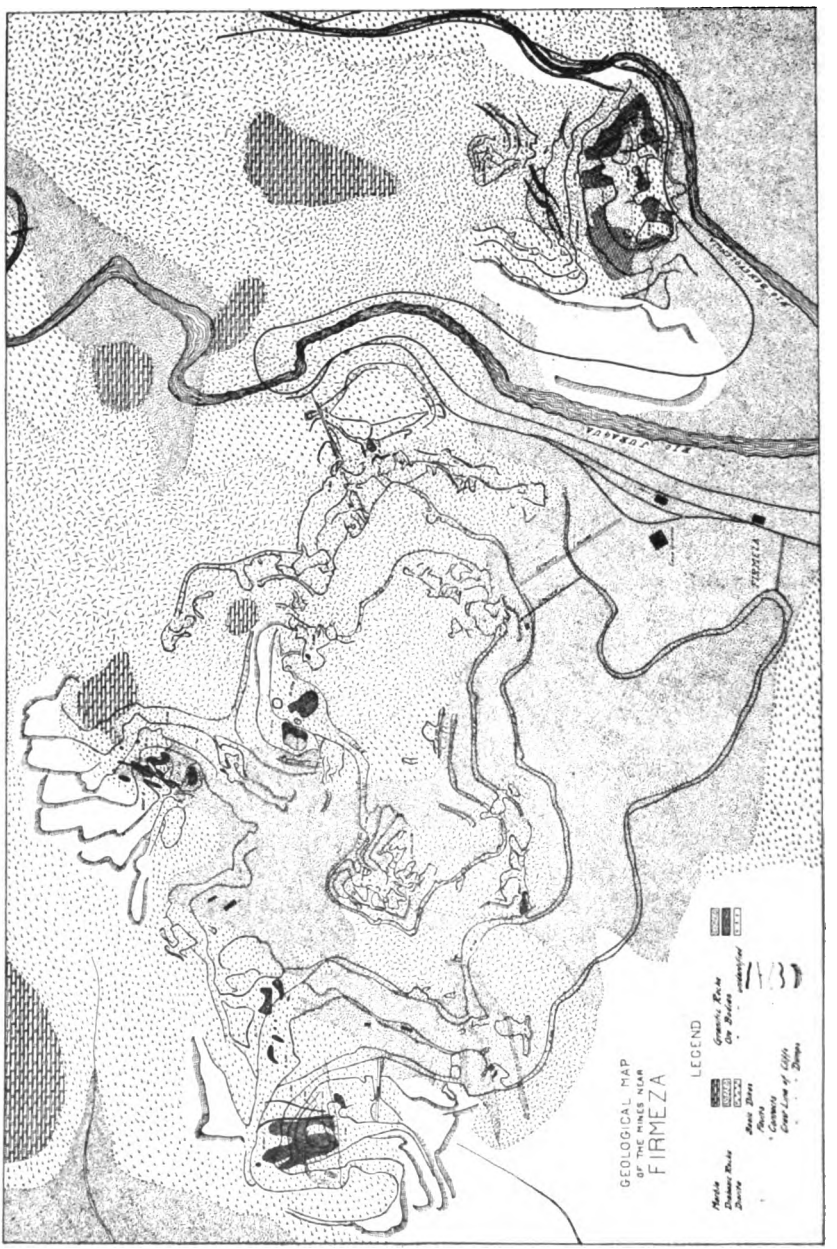

FIG. 10.

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vertical distribution of the igneous rock types. The series from diabase to granite agrees so completely with the series described by N. L. Bowen[18] as the normal result of fractional crystallization in a basaltic magma, that this mode of differentiation seems to be the probable one in this case.

The diabase extrusives mark the initiation of igneous activity, and the diabase intrusive and the diorite mark early chilled phases of a magma becoming more and more acid. The granitic massif is believed to be the batholythic invasion of the more completely differentiated acid liquid, and the granitic apophyses part of the final differentiate from the granite. They will be discussed more fully with the genesis of the ore deposits.

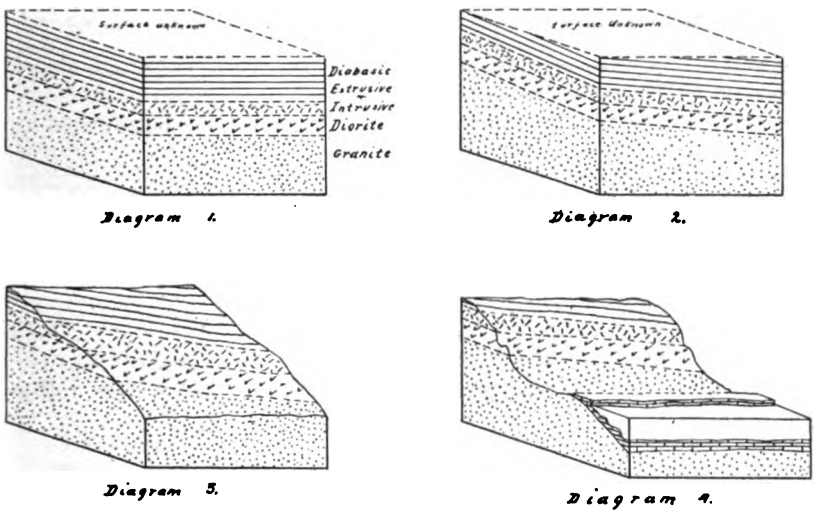

FIG. 11.—GENESIS OF PRESENT ROCK TYPES.

The position of the granite at a definite horizon is thought to be due to the fact that the cooled shell of the magma had developed cooling cracks to that depth, and as a result the crystallizers were able to escape from the granite, which could then no longer remain liquid.

Thus differentiation may account for the horizontal arrangement of the rock types observed in the field.

Tilting.—However, the arrangement is not entirely horizontal. There is a distinct slant toward the southeast of all the surfaces of contact. The extrusives and the limestones, where bedding can be determined, all show a pitch to the southeast. Just when the tilting took place is difficult to establish, but since it does not affect the coastal limestone it may be placed as previous to its deposition.

[18] N. L. Bowen: The Later Stages of the Evolution of the Igneous Rocks. *Supplement to Journal of Geology*, Vol. 23 (November–December, 1915).

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Whether tilting accompanied or followed the igneous cycle and the ore deposition cannot be determined. As the orebodies in the eastern end of the district are lower than those at the western end, it seems more probable that the tilting took place later than the period of ore deposition. The evidence is inconclusive and indefinite, but it is unimportant as the time of the tilting cannot affect any of the major hypotheses.

Erosion.—Since the time of ore deposition erosion has produced a surface that slopes down in general from the north to the south. In this way it has cut across the basic rocks and into the granitic massif.

Fig. 11 shows a diagrammatic representation of the way the present arrangement of the rock types is believed to have been brought about. The diagrams are not to scale, and are intended merely as an aid to visualizing the conditions.

Diagram 1. Shows conditions after differentiation and before tilting or erosion.
Diagram 2. Shows conditions after tilting and prior to erosion.
Diagram 3. Shows condition after partial erosion but prior to deposition of coastal limestones.
Diagram 4. Shows conditions as they are at present.

V. GENERAL DESCRIPTION OF ORE DEPOSITS

Nature of Ore

The ore mined in the Firmeza district is a mixture, in varying proportions, of magnetite and hematite. The ore is remarkably pure and contains little foreign matter. J. P. Kimball[19] gives the following figures:

	Per Cent.
Moisture (in part hygroscopic)	0.24 – 0.81
Silica and insoluble	5.00 –10.50
Phosphorus	0.009– 0.065
Sulphur	0.045– 0.248
Iron	61.00 –68.50

Although present mining methods make it possible to ship ores somewhat lower in iron content, and greater depth in mining has exposed ores somewhat higher in sulphur, the figures Kimball gave in 1884 are substantially correct for the ores now being mined. The low phosphorus and the absence of appreciable amounts of titanium make the ore a very valuable one for the manufacture of high-grade steel.

Shape and Size of Orebodies

In shape the deposits are extremely irregular. Whatever their size they show one common feature throughout the area—a far greater extension in two dimensions than in the third dimension. They resemble a

[19] J P. Kimball: Geological Relations and Genesis of the Specular Iron Ores of Santiago de Cuba. *American Journal of Science*, Ser. 3, vol. 28, p. 426 (1884).

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series of scattered lenses of irregular outline, that lie in every conceivable position. Fig. 12 shows a series of horizontal projections of the ore-bodies and a few vertical sections. The projections are drawn to scale, and the sections are sketched from exposures in the walls of the mines. The outlines represent the boundary of the ore of commercial grade. They do not represent the edge of the mineralization. There is generally a transition from ore to rock, not a definite contact between them. This subject will be more fully discussed under the mineralogy of the ore deposits.

The size of the ore deposits varies from pockets containing a few tons to lenses whose larger diameters are measured in hundreds of feet and whose thickness is from 10 to 50 ft.

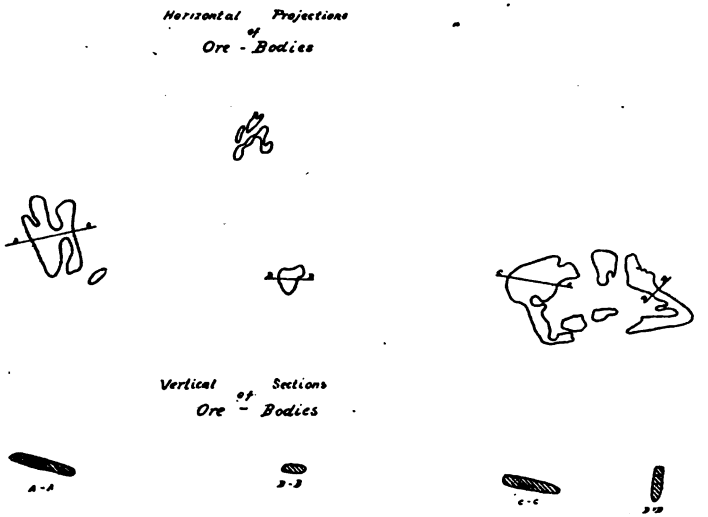

FIG. 12.

GEOLOGIC POSITION

With the possible exception of the Ocania and Chicharron Mines, the ore deposits lie within 200 ft. of the granitic apophyses. These exceptions are probably more apparent than real and may indicate that the aplite has not yet been exposed, not that it is absent. In most cases the ores are directly in contact with aplitic or pegmatitic rock.

The relation of aplite to ore is exceedingly intimate. In most of the mines the ore lies against a floor or wall formed either of aplite or of pegmatitic granite with much micrographic quartz-plagioclase intergrowth. Frequent aplitic dikes traverse the ore, and form prominent features because of their light colors, in strong contrast to the black ore and the green igneous rocks. In spite of their apparent position as dikes, they are *not* to be considered as later than the mineralization. This will be shown in the discussion of the genesis of the ores.

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The host rock of the ore is either one of the basic igneous rocks, or limestone. By far the greater part of the ore is in diorite or intrusive diabase; some is found in the diabasic extrusive rocks, and only a minor amount in the limestones. In at least three of the mines the ore deposits are very close to the overlying limestone masses, but there has been no localization in the limestone, and the larger masses are separated from the limestone by basic igneous rock.

Mineralogy

The ore minerals are magnetite and specularite, with some amorphous, more or less hydrated, hematite in the upper parts of the ore deposits. The common gangue minerals are quartz, wollastonite, epidote, and lime-iron garnet. The epidote comes in two clearly distinct forms. The one is a well-developed, crystalline form, transparent and pleochroic from green to yellow. The other is a fibrous and scaly form, lacking in well-defined terminations. The well-crystallized form is the one that

Fig. 13.—Pyrite Crystal from West One Mine.

is associated with the ores, and other lime silicates. In some of the mines calcite is intimately intergrown with the ore. Apatite, titanite, and a mineral determined with some doubt as scapolite, were also found in rare cases. Secondary calcite, introduced by recent weathering, is not uncommon. Pyrite and chalcopyrite are found in most of the mines. They are almost always introduced and rarely if ever original constituents of the ore deposits. The pyrite occurs in well-developed crystals. One of these from the West One Mine is shown in Fig. 13. Associated with them is chlorite and some sericite.

Distribution

The distribution of the minerals appears to be haphazard and extremely irregular, but natural phenomena are not haphazard, they are logical and sequential. Detailed study has convinced the writer that the minerals in these deposits are distributed according to a definite order. The most clearly localized material is the massive, fine-grained

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intergrowth of magnetite and specularite. This massive ore lies in, and close to, fissures, or else directly next to aplitic or pegmatitic granite. In the minute intergranular spaces of this ore occur wollastonite and some quartz. As the ore becomes less massive toward the margin of the deposit the proportion of specularite to magnetite increases, wollastonite becomes more abundant, as does quartz, and epidote and garnet appear. Still further out from the massive ore, garnet predominates, with some epidote, little or no wollastonite, very little quartz, and some more coarsely crystallized specularite and magnetite. In some cases this zone is composed entirely of massive garnet with very few impurities. Beyond the garnet is an area in which epidote and quartz are prevalent. Mineralization of this type is the most widely diffused, and merges gradually into partially chloritized and epidotized country rock. The above description holds only for the typical deposits in igneous rocks. The complete sequence is rarely well-exposed. It can best be observed in the East Mine and in West Five Mine.

Those deposits which are obviously in limestone show two kinds of mineral distribution. One is shown in the Chicharron Mine. It is a dike-like mass of magnetite and specularite, with interstitial wollastonite. The contact with the marble is sharp, and no contact minerals are found. The other form of orebody in limestone is a central mass of almost pure specularite, in rosette-like clusters, or granular masses, surrounded by an intimate intergrowth of quartz well-crystallized calcite, garnet and epidote. This form of orebody is best exposed in the North Mine. The transition to unaltered marble is not exposed there. In the Ocania Mine this transition is exposed and shows a sharp change from garnet rock to unaltered marble.

Apatite is so rare that it is a curiosity in the district. Fig. 14 shows a photomicrograph of the one section in which it was found in appreciable amount. It appears in the usual hexagonal basal, and elongated prismatic sections. Later than the apatite in time of crystallization are magnetite and the minute garnet-epidote intergrowth. The scarcity of apatite and so of phosphorus in the ore is of the greatest economic importance. It is also an indication that phosphorus had little or no share in the mineralization.

The secondary calcite, the kaolin, and the amorphous and hydrated hematite, are found either very close to the surface or in channels to which meteoric waters have obviously had access. Pyrite is common in all the mines and lies close to the surface in an unaltered, well-crystallized condition.

Interpretation of the Mineralogy

The first inference to be drawn from the mineralogy of the ore deposits is that they were formed at a high temperature. The temperature of

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mineral formations is not yet so well known that accurate conclusions can be drawn from mineralogy as to the exact temperature at which ore

FIG. 14.—SPECIMEN FROM WEST FIVE MINE. × 18. Magnetite—black. Epidote and garnet—gray. Apatite—clear white.

deposits were formed, but the occurrence of lime-iron garnet and of wollastonite indicates high temperatures. The association with quartzose pegmatitic dikes, which, as will appear later, are believed to be con-

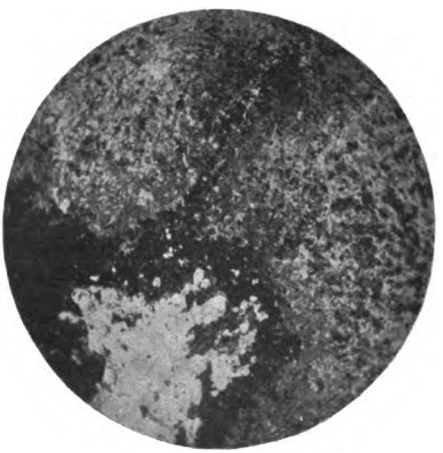

FIG. 15.—DIABASE PORPHYRY, SHOWING INTRODUCED QUARTZ AND EPIDOTIZATION. FROM EAST MINE. × 18. Quartz—white. Epidote—dark.

temporaneous with the ore formation, places the temperature at from 575° to 800°C.[20]

[20] F. E. Wright and E. S. Larsen: Quartz as a Geologic Thermometer. *American Journal of Science*, Ser. 4, vol. 27, p. 421 (1909).

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The second inference drawn from the mineralogy is that the iron, lime, and quartz are either entirely, or in part, introduced material. The large quantity and great concentration of the iron oxides makes their formation from the rock in which they are found highly improbable, and it can be assumed that they represent, almost entirely, introduced material.

The lime silicates, epidote, wollastonite, and garnet are so abundant that an addition of lime seems extremely probable. Because the rock, now entirely replaced by ore, was a calcic igneous rock, it is impossible to determine whether the lime, which was added to form garnet zones, came from the magma or from the replaced rock. Since, in places like Estancia Hill, the garnet zones are out of all proportion to the size of the orebodies, an addition of lime from the magma seems probable. The major part of the lime is deposited in a zone beyond the iron. In this section the lime silicates always show a later crystallization than the iron oxides. This makes it appear as if the mineralizing agent deposited most of the iron oxides while still able to retain much lime in solution. Further evidence of the addition of lime from the magma is afforded by the apatite, intergrown with the magnetite, in the specimen from West Five Mine described above (Fig. 14). The epidotization and silicification in the outermost zone seems to be largely recrystallization, and represents a hydrothermal alteration, more or less *in situ*, rather than an addition of material. The chloritization is also an effect of this hydrothermal metamorphism. Six samples of diabase showing more or less epidote were analyzed for lime. They were selected at various distances from a limestone inclusion, in order to show a supposed absorption of lime. The variation in the lime content is so slight that no addition of lime in this zone can be postulated. The quartz is probably largely a byproduct of the alteration of hornblende to epidote. It is also in part introduced, as is shown in the microphotograph of a specimen from the diabasic porphyry in the East Mine (Fig. 15).

In regard to the chemical form of the mineralization, inferences are largely negative. Evidence of the participation in the mineralization of any of the halogens is practically lacking. The apatite and dubious scapolite indicate some chlorine, but the occurrence of chlorine-bearing minerals is so rare that the introduction of the iron as chloride cannot be shown. CO_2 would be expected to show its presence in the formation of carbonates in the zone of hydrothermal alteration. No appreciable amount of carbonates was found, except in the occurrences in limestone. The presence of much water is shown, however, by the extensive hydrothermal alteration in the outer zone of mineralization. If the inferences in regard to the temperature of the formation of the ore deposits is correct, it is more than 200° above the critical temperature of water (358°C.). The fact that this water carried material in solution makes it impossible

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to prove that it was in the form of vapor; but the high temperature and the fact that the mineralizers were able to permeate dense rocks so readily makes it probable that they were in the gaseous rather than the liquid phase. The mineralizers are, therefore, believed to have been dominantly water vapor, carrying with it iron, lime and silica. The variation in mineralization with decreasing intensity is believed to be as shown in the diagram, Fig. 16.

A later mineralization is represented by the pyrite and chalcopyrite. Field observation shows that the pyrite in the ore lies in channels along which solutions have passed. In the ore it is usually accompanied by chlorite. Where the pyritization has taken place in the wall rocks, as at the west side of the Ocania Mine, both chlorite and sericite occur. The pyrite seems to represent a very much later stage of mineralization than the magnetite and hematite. It is impossible to prove any definite

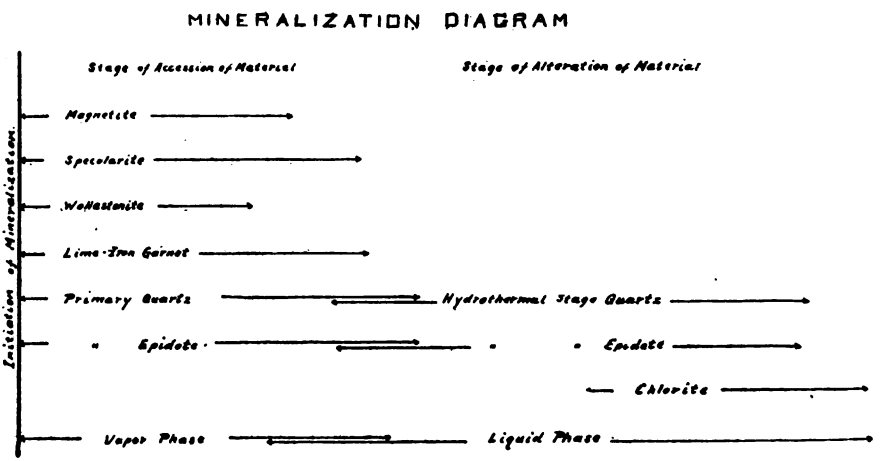

FIG. 16.

connection between the pyritization and the mineralization that produced the orebodies. It seems probable that the solutions that brought the pyrite came in at the time of the intrusion of the later dikes.

Superficial Alteration

W. Lindgren believes that the hematite is the result of the alteration of magnetite by surface agencies. He says:

"——the hematitization is probably a low-temperature process developing gradually under the influence of oxidizing atmospheric waters."[21]

The occurrences in the Firmeza district are in entire accord with those

[21] W. Lindgren and C. P. Ross: The Iron Deposits of Daiquiri, Cuba. *Trans.*, vol. 53, p. 52 (1916).

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recorded by Lindgren at Daiquiri. There is an increase of hematite in the higher levels of the mines. A polished specimen of the massive ore from the Chicharron Mine shows very clearly the intimate intergrowth of specularite and magnetite, and some of the specularite forms a later veinlet through the magnetite; but to accept without question specularite as an oxidation product of magnetite is difficult. The writer has been unable to find any record of the synthesis of specularite except at high temperatures, or of the occurrence of specularite as the result of surface oxidation except under superimposed regional metamorphism. It is not the crystalline facies of hematite that forms under surface agencies, as the result of dehydration of limonite, in the ores of Mayari. Lindgren does not state that the process of hematitization produces the crystalline facies of hematite (specularite). But since it is the crystalline phase that is abundant, and the earthy phase rare, it must be the specularite to which he refers. He does say:[22]

"The pits in the hematite are probably caused by the local development of a softer or earthy facies of the mineral."

In spite of the fact that all the observations accord with the theory advanced by Lindgren, the writer desires to suggest another hypothesis—that is, that both the magnetitization and the hematitization are due to a primary mineralization by gaseous solutions deficient in ferrous oxide.

F. W. Clarke says:[23]

"Ferric oxide can crystallize out as hematite only when ferrous compounds are either absent or present in quite subordinate amounts, for ferrous oxide unites with it to form magnetite."

The crystallization of specularite as a later phase, from a gaseous solution containing insufficient ferrous oxide to produce all magnetite, is in accord with Clarke's observation. Deficiency of FeO could account for a change from magnetite to specularite about centers of crystallization. It could also account for a diffusion of the hematitization to a higher level, further from the centers of mineralization than the magnetitization. Local variations in the mineralizing solutions account for the variations in the proportion of hematite to magnetite in bodies equally close to the parent magma.

The only important effect ascribed to surface waters by the writer is the oxidation of the pyrite near the surface, and the resulting decrease in the undesirable sulphur content.

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[22] *Op. cit.*, p. 52.

[23] F. W. Clarke: Data of Geochemistry. *Bulletin* No. 616, *U. S. Geological Survey*, p. 347.

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VI. GENESIS OF THE ORE DEPOSITS

Previous Theories

The preceding description of the ore deposits, and discussion of their mineralogy, makes it possible to consider the larger features of their mode of formation. As this is purely a matter of interpretation, it is necessary to consider first the interpretation given by others. J. P. Kimball, writing in 1884–85, and F. F. Chisholm, in 1890, formed their conclusions from observations made in the Firmeza district. A. C. Spencer based his theories on work done at both Firmeza and Daiquiri in 1901. The papers by J. F. Kemp, and by W. Lindgren and C. P. Ross, were written more largely from evidence gathered at Daiquiri, with some specimens and a brief visit by Kemp to the Juragua mines in 1914. J. T. Singewald, Jr. and B. LeRoy Miller made a brief visit to the Firmeza and Daiquiri districts in the fall of 1915.

J. P. Kimball[24] recognizes two types of ore deposit in the Juragua district. These are "replacements" of the coral limestone by iron-bearing solutions, and "concentrations" of ferric oxide in the diorite, "almost *in situ*." Both replacement and concentration are ascribed to the action of circulating meteoric waters.

F. F. Chisholm[25] discusses the theory advanced by J. P. Kimball, and disagrees with it. He states his own opinion as follows:[26]

"My conclusions, after going in detail over most of the exposure made by the Union cut, were that, whatever the exact character of the ore deposit, the present position of the ore cannot properly be considered the result of local metamorphism of limestones by the action of surface waters containing iron leached from the overlying mass of iron-bearing diorite. I am much more strongly inclined to consider the ore here either the result of concentration within a diorite dyke which was originally characterized by the presence of a large percentage of iron, or else a distinct band forming a portion of a larger dyke. In other words, I am strongly of the opinion that the source of the ore is from below, and consequently that the loyalty of these deposits may be relied on below the limits of atmospheric action. I regret that I was unable to go into the question in detail, and get positive facts in support of my belief, but that I am obliged to admit that my examination was too superficial to enable me to prove my views."

A. C. Spencer,[27] after considering several possible explanations of the genesis of the ore deposits, comes to the conclusion that they are parts of

[24] J. P. Kimball: Geological Relations and Genesis of the Specular Iron Ores of Santiago de Cuba. *American Journal of Science*, Ser. 3, vol. 28, p. 426 (1884).

The Iron-Ore Range of the Santiago District of Cuba. *Trans.*, vol. 13., pp. 613–634 (1884–85).

[25] F. F. Chisholm: Iron-Ore Beds at the Province of Santiago, Cuba. *Proceedings of the Colorado Scientific Society*, vol. 3, part 3, pp. 259–263 (1888).

[26] *Op. cit.*, p. 262.

[27] C. W. Hayes, T. W. Vaughan, and A. C. Spencer: *Report on a Geological Reconnaissance of Cuba*, 1901.

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an older limestone-schist series which has been completely involved in the later igneous intrusions. The following quotation shows how completely Spencer supposed this older series to have been immersed in the igneous rock.[28]

"A mass of many million tons weight floated upward by the buoyant effect of molten rock in motion from the interior toward the surface of the earth, is the only conception which adequately accounts for the mode of occurrence of the orebodies of the Magdalena and the Lola Mines at Daiquiri; while, though less strikingly shown, at Firmeza it is likely that the masses of schist, marble and ore have been likewise actually suspended in the molten lava."

J. F. Kemp[29] divides the orebodies into two types—the "Distinctive Contact Zones" in limestone, and the orebodies in the diorite. The former he regards as due to contact-metamorphic effects produced in the limestones by the granite. Of the orebodies in the diorite he says:

"One is led to the conclusion that while the diorite mass was still hot in the depths or after it had consolidated and had been penetrated by some other and still hot intrusive in depth, a pronounced northwest and southeast fissured zone was formed, up through which came the emissions, fluid or gaseous, which brought the iron for the ore, the pyrite, the garnet and the epidote; the sulphur for the pyrite; and the silica for the quartz, the garnet and the epidote. The lime required by the garnet and the epidote may have been derived from the plagioclase and hornblende of the diorite, or from included blocks of limestone, or deep-lying limestone, or from several of these sources."

Another quotation from the same article might indicate that Kemp suspected what has become the conviction of the writer:[30]

"One cannot help associating the granitic or pegmatitic dikes with some large parent body. The natural one is the intrusive granite mentioned at the outset. Yet this granite has produced contact zones on the older limestone with orebodies, whereas the granitic and quartz-porphyry dikes are, in two cases at least, later than the large orebodies. We can only suspect the possible connection without being able to prove it."

Kemp apparently bases his conclusion that the aplite dikes represent a later intrusion into the ore on the fact that the dikes appear to cut the ore. That another hypothesis is possible will be shown later.

W. Lindgren and C. P. Ross,[31] on evidence gathered by the senior author, deduced the following conclusions:[32]

"From the above it is clear that the primary iron oxide of the deposits at Daiquiri is a magnetite, which subsequently has been altered more or less completely to a

[28] *Op. cit.*, p. 82.

[29] J. F. Kemp: The Geology of the Iron-Ore Deposits In and Near Daiquiri, Cuba. *Trans.*, vol. 53, p. 30 (1916).

[30] *Op. cit.*, p. 21.

[31] W. Lindgren and C. P. Ross: The Iron Deposits of Daiquiri, Cuba. *Trans.*, vol. 53, p. 52 (1916).

[32] *Op. cit.*, p. 53.

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hematite. The discussion of genesis may therefore be divided into two parts: the first relating to the origin of the magnetite; the second to its subsequent hematitization.

The mineral association of the magnetite, particularly the presence of much garnet, shows that it originated under high-temperature conditions.

On the other hand, the hematitization is probably a low-temperature process developing gradually under the influence of oxidising atmospheric waters."

The problem of hematitization has already been discussed.

Further on three theories discussed by A. C. Spencer are rediscussed by the authors. After dismissing the other two theories, the second theory is accepted, as follows:

"There remains the second theory, accounting for the large masses of magnetite included in the diorite by contact-metamorphic limestone, by which the latter has become almost entirely replaced by magnetite derived from magmatic emanations rich in iron. While it is freely admitted that the genesis of the Daiquiri deposits is difficult to explain, it will be shown that the view outlined in the previous sentence has much in its favor."

J. T. Singewald, Jr. and B. LeRoy Miller discuss the theories advanced by J. F. Kemp, and by W. Lindgren and C. P. Ross in the papers cited above and come to the conclusions summed up in the following paragraph:[33]

"To sum up our opinions, the Cuban iron ores are contact-metamorphic deposits localized about engulfed blocks of limestone in diorite. In such cases, where there was a limited supply of magmatic emissions, there resulted the contact metamorphism of only a part of the limestone block. Where the supply was ample and the action most intense, not only was the block of limestone completely replaced, but complete endomorphism of the igneous rock on a large scale occurred in the vicinity."

The above are the interpretations offered by others who have studied the field more or less closely. But a study of different exposures and a different viewpoint, have led the writer to conclusions which are somewhat at variance with those quoted.

Hypothesis of the Genesis of the Ore Deposits

Any comprehensive theory concerning the formation of an ore deposit in a rock of which it is not an original part—*i.e.*, an epigenetic deposit—must account for four things:

I. The source of the material forming the deposit.

II. The vehicle that introduced the material.

III. The channel through which the material was introduced.

IV. The cause for the deposition of the material.

Source of the Iron Ore

The ultimate source of the iron ore was the diabasic magma. The extruded part of this magma, with the involved limestones, together with

[33] J. T. Singewald, Jr. and B. LeRoy Miller: The Genesis and Relations of the Daiquiri and Firmeza Iron-Ore Deposits, Cuba. *Trans.*, vol. 53, p. 73 (1916).

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the chilled upper part of the magma (the diabases and diorite) form the host rock of the ore deposits. It has already been shown, in the discussion of the areal geology of the district, that this magma was differentiated, that the earlier igneous rocks represent the parent magma and its early differentiation products, and that the latest rock differentiate was the granite massif with its aplitic apophyses. These aplites, which are, under anhydrous conditions, extremely viscous, must have contained large quantities of crystallizers, to keep them fluid and enable them to penetrate the basic rocks as sills and dikes. It is believed that these crystallizers were mostly composed of water vapor well above the critical temperature, and that they held in an ionized state the ore minerals.

Vehicle to Carry the Material

These concentrated crystallizers were also the vehicle to carry the mineral burden into the rocks which act as host for the ore deposits, *i.e.*, the crystallizers plus the minerals are the mineralizers. Their nature has already been discussed under the interpretation of the mineralogy.

Channels through which the Materials Were Introduced

One of the striking features of the larger Firmeza deposits is that the ores have almost obviously avoided the masses of marmorized limestone. This can be explained by the fact that the channels, through which the mineralizers entered, were not lines of weakness or shear zones produced by structural faulting, but were cooling cracks in the igneous rocks.

Causes of Deposition

The causes of deposition may be of either a chemical or physical nature, or a combination of the two. It has been the tendency of geologists to ascribe to chemical action the dominant rôle in contact-metamorphic actions. This makes it difficult to account for ore deposits in two rocks as chemically different as diabasic igneous rock and marmorized limestone, except by postulating different theories. If it is believed that the dominating cause of deposition is physical, and is inherent in the mineralizing solutions, whether liquid or gaseous, this difficulty is obviated. It is the writer's opinion that such was the case in the formation of these deposits. The mineralizers were under such great pressure and at such high temperature that they had the power to diffuse through, and either partially or wholly absorb, any rock with which they came into contact. This diffusion and partial absorption would rob the mineralizers of a large part of their activity, and would cause deposition of the minerals in a definite order. The order would be dependent on the

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saturation of the gases or solutions with the material to be deposited. The field evidence for such an order is clear, and as shown in discussing the mineralogy of the deposits, magnetite and specularite are the first minerals deposited. As a result, the magnetite and specularite are localized in and near the fissures through which the solutions had access, while the epidote, which is probably only in part due to accession of material from the magma, and is more largely a recrystallization of material present in the host rock, is much more widely diffused. Joseph Barrell, in conversation with the writer, suggested that such a hypothesis may also account for the association of aplitic and pegmatitic dikes with basic mineral segregations. The dikes are regarded as the residual, acidic rock material left in fissures, from which the mineralizers had gone into the rockbody, and in the process had effected a separation of acidic and basic material.

Under the hypothesis advanced, the orebodies owe their localization in igneous rock or limestone almost entirely to the accessibility of the rocks to the mineralizing solutions. The same causes govern all the cases.

Briefly stated, it is the writer's belief that the orebodies in the Firmeza district are due to mineralizers concentrated by further differentiation from the granitic massif, which is itself a differentiate of the diabasic magma. These mineralizers diffused from the igneous-rock material into those rocks to which fissures gave access, and, in diffusing, deposited their mineral burden, and left the rock material with which they had been mixed in the fissures, as aplites and pegmatites.

Under this hypothesis the mineralization becomes a definite event in the igneous cycle, which cycle may be restated as follows:

1. Extrusion of diabasic rock material.
2. Intrusion of diabasic magma.
3. Formation by differentiation of more acidic residual liquid in magma reservoir, with concentration of mineralizers.
4. Batholythic invasion of granitic material, together with mineralizers.
5. Further differentiation of the granitic batholith and escape of mineralizers with some acidic rock material, into cooling cracks in country rock.
6. Separation of basic mineralizers, and acidic rock material, with contemporaneous formation of ore deposits and aplitic dikes and pegmatitic apophyses.

The hypothesis submitted above is given prior to a detailed description of the ore deposits. It must be tested by its application to the phenomena observed in the field, and by its ability to meet the objections raised by other writers on the same subject.

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Discussion of the Different Hypotheses

The hypotheses of the different geologists who have visited the region have been given, either as abstracts or by quotations. The earliest of these, that by J. P. Kimball, does not account for the formation of what are now known to be high-temperature minerals: garnet, wollastonite, etc. Kimball recognized, however, that some of the ores lay in diorite and could not be accounted for by a hypothesis which depended entirely on limestone as a precipitant. F. F. Chisholm does not attempt to formulate a complete theory of the genesis of the ore deposits. With his conclusion that the mineralizing solutions came from below, and were not of meteoric origin, the writer entirely agrees.

The theory accepted by A. Spencer requires an older limestone-schist series. Limestone was found in the Firmeza district in considerable abundance, but schist could not be found by the writer. The occurrence of tuff-limestones interbedded with the volcanic extrusive material makes it seem probable that the period of sedimentation, and the initiation of the igneous cycle, were not separated by a time interval sufficient for the forming of surface orebodies and their entombing in igneous rock. This theory also fails to account for the forming of ore in diorite. In a tunnel under West Five Mine the transition from fresh diorite to granular magnetite can be traced. The transition is gradual and there is no contact which could possibly be interpreted as the margin of a partly assimilated block.

Later writers all agree in ascribing an igneous origin to the mineralizing solutions. The principal divergence is in the amount of influence ascribed to the limestone. Lindgren and Ross believe the ore to be definitely localized in limestone. Singewald and Miller regard the ore as in, and about limestone. Kemp believes that some of the deposits are independent of limestone. In regard to this feature the writer agrees entirely with Kemp. The conception of the orebodies as metamorphosed, engulfed limestone masses is difficult. The shape and position of the ore masses demand that the entombed blocks should have been comparatively thin, and that they should have come to rest in every conceivable position. This theory completely fails to account for the gradual transition from ore to diorite shown at its best in West Five Mine. Such a deposit could be accounted for only on the basis of complete assimilation of the limestone, and a deposition of magnetite about the locus of assimilation. Any theory that places the burden of producing the orebodies upon the diorite, is open to one great objection. It demands that a magma of almost gabbroic basicity shall have produced contact-metamorphic phenomena of the most intense variety. Kemp recognized this, and ascribes the orebodies in limestone to the granite, and the orebodies in the diorite to solutions from "some other" intrusive. But Lindgren and

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Ross, and Singewald and Miller regard the diorite as the source and as the cause of the mineralization. Another objection is the fact that, under that theory, the diorite must have exercised a strong selective tendency. Unaltered marble within 20 ft. of massive ore, and separated from the ore by recognizable diorite, can be seen at West Four. Such a case is not rare, it is a common feature. In one of the old cuts above West One Mine there is exposed a small block of marble, completely surrounded by diabase containing small ore masses. But the marble is entirely unaffected. Contact-metamorphic phenomena are undoubtedly capricious, but if another explanation will solve the problem in such a way as to show that the capriciousness is more apparent than real, it must at least be considered.

The great objection to ascribing the source of the ores to the granitic batholith lies in the interpretation of the aplitic and pegmatitic dikes. Field observation clearly establishes the direct connection between these rock types and the granitic massif. If these most acidic facies represent a later intrusion into the ore, the ore cannot come from the granite. Kemp believes they are later. He says:[34]

"——whereas the granitic and quartz-porphyry dikes are, in two cases at least, later than the large orebodies."

Lindgren and Ross[35] agree with this:

"The dikes of igneous rock intruded into the iron ore appear to be somewhat different from the diorite and are either granite porphyry or aplite; that is, complementary dikes of a later generation."

F. Klockmann, writing on contact-metamorphic magnetite deposits in general, puts the matter even more strongly:[36]

"Wiederholt wird angegeben, dass aplitische Gänge und Granitapophysen die Magnetitlagerstätte durchsetzen. Gibt es denn für den Unbefangenen dafür noch andere Deutung als die, dass der Granit in eine von ihm vorgefundene Lagerstätte eine Apophysen entsendet hat?"

As a general proposition, the very fact that aplitic dikes and granite apophyses are repeatedly reported as traversing magnetite deposits is the strongest possible argument against the purely fortuitous intrusive nature of such apophyses. It was this constant relationship observed in the field study, that first convinced the writer that there must be definite connection between the forming of the orebodies and of the granitic apophyses.

J. H. L. Vogt recognizes a connection between the granite and its apophyses, and the magnetite deposits of Kristiania. He says:[37]

[34] *Op. cit.*, p. 21.

[35] *Op. cit.*, p. 54.

[36] F. Klockmann: Über Kontaktmetamorphe Magnetitlagerstätten. *Zeitschrift für Praktische Geologie*, vol. 12, p. 81 (1904).

[37] J. H. L. Vogt: Problems in the Geology of Ore Deposits. *Trans.*, vol. 31, p. 138 (1901).

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"A study of the Kristiania contact-deposits indicates that the formation of the ores preceded the solidification of the granitic magma. Even when the ores occur in slates immediately adjacent to the granite, or in the small Silurian fragments completely surrounded by granite, they are never found also in the granite itself. This is to be simply explained by the supposition that from the still liquid magma the ores were 'blown into' the adjoining rigid rocks. —The presence in these deposits of granitic apophyses, already mentioned, is another proof that they were formed before the solidification of the granite."

He regards the occurrence of the apophyses with the ore in the Kristiania district as proof that the granitic magma had not yet solidified.

The absence of ilmenite in the Firmeza occurrence is also an indication of their intimate connection with the granitic magma. Vogt has given some very complete studies of the relationship between the different

FIG. 17.—APLITIC GRANITE BORDERED BY MAGNETITE AND EPIDOTE. TUNNEL AT ELEVATION OF 299 FT. UNDER WEST FIVE WORKINGS. MAGNETITE CAUGHT IN APLITE = M. ACTUAL SIZE. Aplite—white. Magnetite—dark.

magnetite deposits and their sources. A quotation that shows some of his conclusions follows:[38]

"Die Erfahrung ergiebt, dass bei der 'oxydischen' Erzaussonderung es namentlich das 'Eisenoxyd-mineral'—bei den Gabbros Titaneisen oder Titanomagnetit, bei den Peridotiten Chromit—ist, welches concentrirt wird. Es ist somit a priori anzunehmen, dass etwaige 'oxydische' Erzausscheidungen in den Graniten einen ähnlichen niedrigen TiO_2-Gehalt führen müssen wie die in dem Granit normal ausgeschiedenen (wohl namentlich Magnetit und Eisenglanz, untergeordnet Titaneisen.)"

The strongest evidence of the intimate relation between the granitic apophyses and the mineralization is based on field observation. Some of the small dikes are reproductions in miniature of the larger occurrences. Figs. 17, 18 and 19 are photographs of two of these small dikes.

Fig. 17 is of a specimen from the ore in the tunnel under West Five

[38] J. H. L. Vogt: Zur Classification der Erzvorkommen. *Zeitschrift für Praktische Geologie*, vol. 2, p. 394 (October, 1894).

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Mine. This is as deep-seated an ore deposit as has been exposed, and is the one most clearly independent of limestone. It is noticeable how entirely clear-cut is the contact between aplite and magnetite in the lower part of the picture. At the same time the other contact is less sharply defined and clearly shows some magnetite trapped in the aplite.

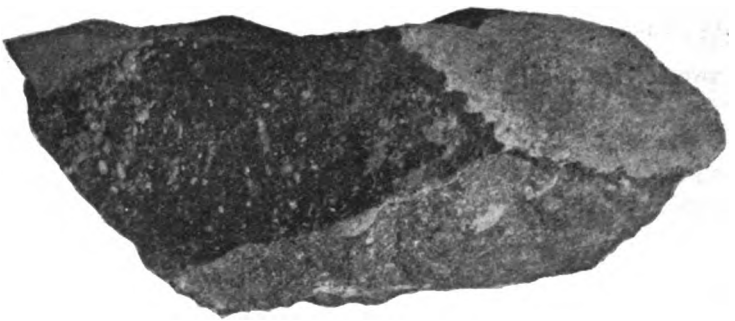

FIG. 18.—PHOTOGRAPH OF SPECIMEN FROM EAST MINE, SHOWING RIM OF GARNETS DEVELOPED BETWEEN APLITIC GRANITE (WHITE) AND DIABASE PORPHYRY (DARK). ACTUAL SIZE.

Fig. 18 is a photograph and Fig. 19 a photomicrograph of a specimen from the East Mine. This specimen comes from the area of diabasic rocks, that lies between the ore in the East Mine and the ore in the pit above, known as North Mine. The specimen shows garnet, merging

FIG. 19.—GARNET BAND BETWEEN APLITIC GRANITE (LOWER RIGHT) AND DIABASE PORPHYRY (UPPER LEFT). FROM EAST MINE. × 18.

on one side into diabase porphyry, on the other into aplite. The writer can see only one explanation for this specimen, and that is a change in the mineralizing solutions from intermixed mineralizers and acidic rock material to aplite.

It is on the basis of such occurrences, and for the reasons given, that

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the writer believes: that the aplite and ore are phenomena produced at approximately the same time; that both were formed from a mixture of mineralizers and rock material derived from the granitic magma; and that the iron oxides, lime and the major part of the silica, held in aqueous solutions above the critical temperature, diffused into the wall rock, leaving the residual rock material in the fissures as aplitic dikes.

VII. DETAILED DESCRIPTION OF THE ORE DEPOSITS

In order to test the hypothesis stated above in regard to the genesis of the ore deposits, it must be applied to the different occurrences of ore in the district. To do this, a detailed description of the more carefully studied mines will be given, and the mode of formation of the orebodies discussed.

Ocania Mine

The Ocania is the most westerly of the Juragua Iron Co.'s mines. It lies on a steep hillside, at an elevation of from 860 to 1,085 ft.

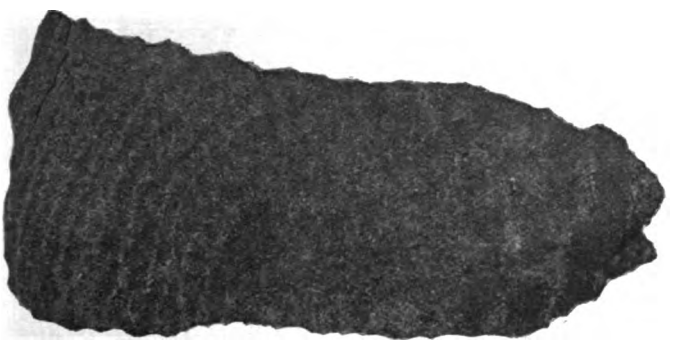

Fig. 20.—Specimen from Ocania Mine, Level 2. The Dark Material is Garnet. The Light Material is Wollastonite. The Structure is Attributed to Diffusion. Actual Size.

The wall rocks are coarse, diabase porphyry, volcanic agglomerates, and marble. The contact between the granitic and diabasic rocks lies on the south slope of the hill, at an elevation of about 700 ft. A quartz-bearing, andesitic dike is found in the mine itself. The ore forms a series of lenses that strike in a northwesterly direction, and have a steeply inclined dip to the southwest. These lenses are made up almost entirely of hematite and quartz, with little or no magnetite. There is considerable pyrite through the ore in various places, and, at the edge of the limestone, chalcopyrite and manganese oxide are found. Some fragments of diabase, with native copper in them, were observed near the limestone. Between the ore and the limestone are masses of garnet rock. Near the bottom of the orebody, as exposed on the southeast side of the mine, was

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found a rock (Fig. 20) which shows, in part, alternating layers of brown garnet, and a greenish mineral which the microscope shows to be wollastonite. As the accompanying photomicrograph (Fig. 21) of the contact between the garnet and wollastonite shows, the two bands have an interlocking structure, perpendicular to the boundary. The black crystals in the wollastonite band are magnetite. The garnet does not show crystal edges. Toward the center it is very dusty, with undeterminable inclusions. The wollastonite forms an aggregate of poorly developed, prismatic crystals. A somewhat similar structure, made up of magnetite and actinolite, is shown in a specimen from the Juragua Mines described by W. Lindgren. He says:

"The texture of this specimen strongly suggests diffusion banding, such as might form in a material, a limestone perhaps, freely penetrated by hot iron-bearing solution."

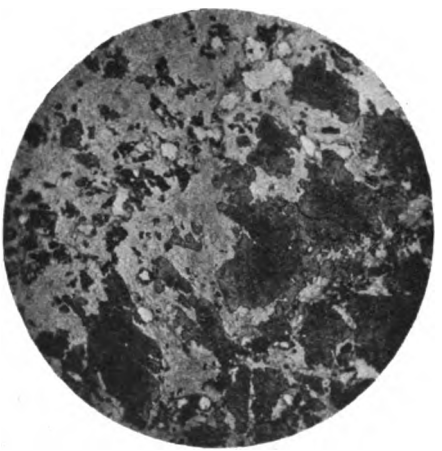

FIG. 21.—CONTACT BETWEEN GARNET AND WOLLASTONITE IN ROCK SHOWING DIFFUSION STRUCTURE. FROM OCANIA MINE. × 18. Magnetite—black. Garnet —dark. Wollastonite—grey. Holes in thin section—white.

In the case of the Ocania specimen the solutions were probably lime-bearing and lower in iron. Whether the original rock was limestone or diabase porphyry, it is not possible to tell. It is true of this mine that the mineralization effects have spread further into the diabasic rocks than into the marble. The clean white marble is separated from the ore by only a few feet of garnet rock, while the diabase is garnetized, near the ore, and epidotized for tens of feet from the ore. The ore itself is so complete an alteration of whatever wall rock it replaces that there is no *prima facie* evidence of what it originally was. The pyritization is later than the principal period of ore deposition, the pyrite forming in fractures and cavities in the ore. The chalcopyrite accompanies the pyrite.

Surface action, producing partial hydration of the ore and considerable infiltration of secondary calcite, obscures the evidence.

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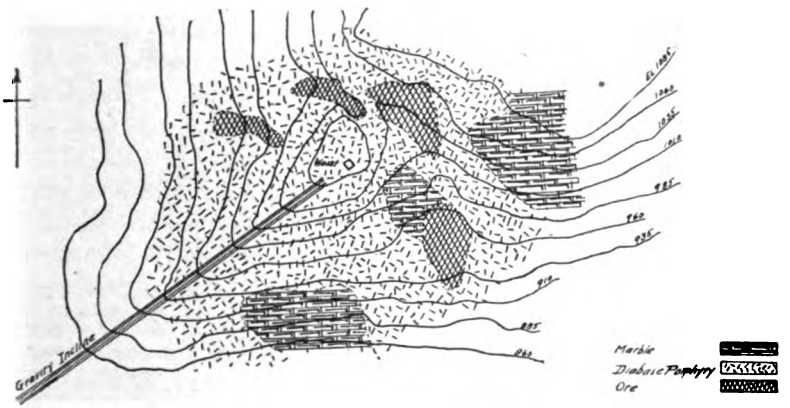
Northeast of the mine workings is the largest exposure of marble found in the district. It is cut by sills of an entirely epidotized, basic, igneous rock. It is separated from the orebodies by massive garnet rock, or highly garnetized and silicified diabasic rock. The map (Fig. 22) shows the relation of orebodies to wall rock. The commercial ore is sharply bounded, but the mineralization is not.

The striking features in this mine are the absence of magnetite and of highly acid wall rocks, and the clear evidence that the diabasic rocks are more diffusely mineralized than the marble. At the same time the resemblance of the blocks of ore to engulfed limestone blocks or to roof pendants, is noticeable.

If the Ocania Mine were the only one studied, it would be difficult for the writer to defend his hypothesis. There are no aplitic or pegmatitic



OCANIA MINE
FIG. 22.

rocks in evidence. Except for one dike of doubtful age, no quartz-bearing igneous rock is exposed in the mine. There is abundant limestone.

There are also some features that are not explained by any theory involving the diabase as the ore-bringing rock. The diffuse mineralization of the diabasic porphyry indicates a hydrothermal effect, later than the consolidation of the diabase. It is difficult to ascribe to the diabase porphyry a hematitizing effect on included limestone, or even a magnetitization free of chrome or titanium minerals, with a superimposed hematitization. The fact that the larger part of the marble has suffered no mineralization is also unexplained by any theory that makes the overwhelming of the limestone by the diabase the cause of mineralization.

On the other hand, if the solutions are supposed to come from the granite, the difficulties of explanation are not so great. A fissure in the diabase near the limestone, localized the ore and the mineralization in

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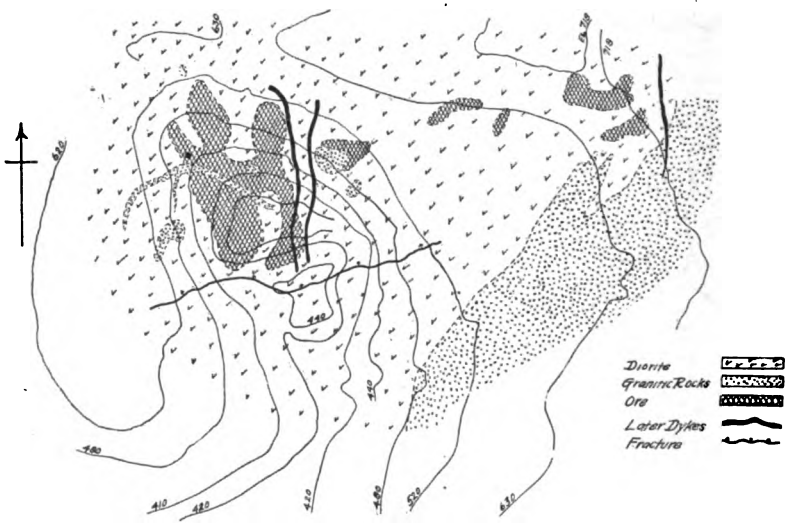
both formations. The distance of the ore exposed from the parent magma accounts for the preponderance of hematite over magnetite.

West Five Mine

This mine is the furthest west of the mines in the vicinity of Firmeza, and lies at an elevation of from 420 to 718 ft. There is a tunnel under the mine at a level of 299 ft.

The ore lies in flat or in steeply inclined lenses. It is almost entirely magnetite, the enclosing wall rock diorite, with considerable, more or less aplitic, granite through and near the ore. The diorite varies from



WEST '5' MINE

Fig. 23.

the even, granular type shown in Fig. 8 to a porphyritic type. No fragmental diabasic rocks occur, nor is there any trace of limestone.

The rocks from the aplitic dike, in the west side of the mine, were described under the granitic rocks. The connection between aplite and ore is shown on a small scale in the photograph (Fig. 17) of a small dike of aplite from the tunnel under the mine. The magnetite and epidote lying immediately next to the aplite merge gradually into the granular diorite. There can be no question of any rocks being involved other than the diorite and the aplite. Their inter-relationship has been discussed in considering the question of genesis. Considerable garnet is found in the mine workings, and its occurrence is similar to the magnetite and epidote. In one small exposure the position of the garnet, as a contact effect in the diorite, next to the granite, is clearly shown. This

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is on the north side of the bottom level. The epidotization and silicification are more widely diffused from the granite than are the magnetite and garnet. One specimen, from the lowest level, showed an intergrowth of apatite with magnetite, and garnet and epidote, cut by later veinlets of epidote (Fig. 14). This is the only case in which apatite amounted to more than a very minor accessory.

On the south side of the mine is an east-west fault steeply inclined to the north. It is obviously post-mineral, and appears not to be of very great throw.

Summing up the evidence from this mine we find: no limestone; much aplitic or porphyritic granite; almost no specularite; but much magnetite, garnet, epidote, quartz—all in diorite.

The ore deposits of this mine offer the clearest evidence that limestone could not have been the dominant cause determining the locus of their deposition. There is no limestone in the immediate vicinity. The transition from ore to diorite is gradual. The mineralogy shows none of the well-crystallized calcite that is so abundant in the ore deposits that were obviously formed in limestone.

The intimate relation between ore and aplite is also well shown here. The dike exposed in the west side of the workings has led other observers to believe the granitic apophyses later than the ore, but as shown under the discussion of the various theories, the same phenomena are interpreted by the writer to indicate the contemporaneous formation of the ore and the aplite.

All the phenomena can be explained on the hypothesis advanced. That is that the aplite, the magnetite, and the lime silicates are all the effect of the one mineralizing period.

Loma Alta Mine

The Loma Alta workings lie at the top of a hill, just east of West Five Mine. They are at an elevation of from 900 to 1,000 ft. As they have been practically abandoned for some time, weathering agencies have obscured much of the evidence. Cropping out on the east side and pitching under the workings at a flat angle, is a sheet-like mass of porphyritic, pegmatitic granite. In the southwest corner of the workings is recognizable diabase porphyry. What is left of the ore is earthy hematite lying on the granite with some kaolin and chlorite. All around the workings on the hill are rounded boulders of more or less pure magnetite. The association of the granite, diabase and ore in this mine is suggestive, but the evidence is too obscured to be conclusive. The earthy hematite undoubtedly owes its origin to the weathering of the primary, crystalline iron oxides. There is no reason to believe that the process did not involve the formation of limonite and its dehydration.

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West Three Mine

An exposure in this mine, which lies on the same hill, south of and below Loma Alta Mine, is worth describing. It is a mass of magnetite, in diabase, directly next to a vertical pegmatitic granite dike. The rocks are all fairly unaltered, and the contact relationship well shown. There is no reason to suspect that any limestone was involved.

West Four Mine

This mine is located northeast of Loma Alta, at an elevation of from 645 to 935 ft. At the north end of the mine, in the upper levels, is a mass of clear white marble, entirely unmineralized. The wall rock in the upper levels is diabase porphyry; in the lower levels it is diorite. The diabase and diorite are both cut by somewhat porphyritic and pegmatitic granite. The ore is magnetite, and hematite and magnetite, and lies in lenses, whose major axes vary from a vertical to a horizontal position, on top of and next to the granite. These lenses occur mostly in the diorite, partly in the diabase, but not, so far as exposed, in contact with the marble. The entire series, diabase, diorite, granite, ore, is cut by a large dioritic dike. The mineralization effects are no different from those in West Five Mine. The chief difference is that there is marble exposed in the West Four workings. So far as can be determined it has had no direct effect on the formation of the ore deposits.

West One Mine

The West One workings are located north of Firmeza along the railroad tract. Exposed for a long time and not much worked of late years, they offer a poor field for study. Altered diabase and diorite, highly mineralized, are the wall rocks. Neither limestones nor granite are in evidence. From one of the cuts that is being worked, come the well-developed pyrite crystals, of which one is shown in Fig. 13. This exposure shows the pyrite all through the magnetite in cavities and along minute fractures. It establishes the age of the pyritization as definitely post-magnetitization.

Fig. 24 is a photograph of a drawing made with a camera lucida, from a thin section of a rock from West One Mine. The specimen is dark and dense, and shows patches of magnetite. In thin section the rock proves to be a diabasic porphyry, partly replaced by magnetite. The replacement is apparently independent of the mineralogy of the replaced rock. The magnetite occurs in patches in the groundmass, or in the phenocrysts, or partly in each.

West One Mine offers no new evidence in regard to the genetic problem. It does offer, however, an excellent field for the study of the pyritization.

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East Mine

The East Mine itself lies east of Firmeza, at an elevation of from 440 to 700 ft., but it cannot be considered apart from the small cut just to the north at about 800 ft. elevation, known locally as the North Mine.

The large orebodies of this mine are made up of specularite and magnetite, with much epidote quartz and garnet. They lie in the lower levels of the mine around bosses of aplitic granite, in diorite and diabase porphyry. Fig. 18 is a photograph and Fig. 19 a microphotograph of a lime-iron garnet rim along a small aplite dike, in recognizable diabase porphyry. It shows the entire ability of the mineralizers that accompanied the aplite to garnetize the diabase.

Fig. 24.—Photograph of a Drawing Made with a Camera Lucida, from a Thin Section of a Specimen from West One Mine. It Shows a Diabase Porphyry, Partly Replaced by Magnetite (Solid Black). The Phenocryst is Plagioclase. The Lath-Shaped Crystals Are Plagioclase. The Groundmass is Granular Magnetite and Chlorite.

(In the upper half of the drawing the interstitial magnetite has been left out.)

Fig. 26 is a microphotograph of a thin section of a completely epidoized and silicified diabase. The specimen comes from within 2 ft. of the one shown in Fig. 15. The two show successive stages in the hydrothermal metamorphism.

In the upper levels, extending toward the North Mine workings, are several included sheets of completely marmorized limestone. The North Mine workings, now abandoned, show remnants of orebodies. This ore is different from any found in the mines so far described. Well-developed rosettes of specularite blades in garnet quartz matrix; well-crystallized epidote in abundance; coarse quartz; much recrystallized calcite; in short, all the components are found of a typical contact-metamorphic deposit in limestone.

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In the East Mine, as in the others, later andesitic and dioritic dikes are in evidence. A northwest-southeast fault, pitching to the northeast, has cut the ore and aplitic granite as well as the older rocks. It has brought the most basic of the granitic rocks, the quartz-bearing diorite, in contact with the ore. That the specimen described as a wall rock of the East Mine, by J. F. Kemp[39], came from this mass of quartz-bearing diorite seems probable. The rock in which the ore is found is of a much finer grain.

In the East Mine itself there is no evidence that limestone had any share in forming the orebodies. In the workings known as the North Mine, limestone is without doubt the host rock of the orebodies.

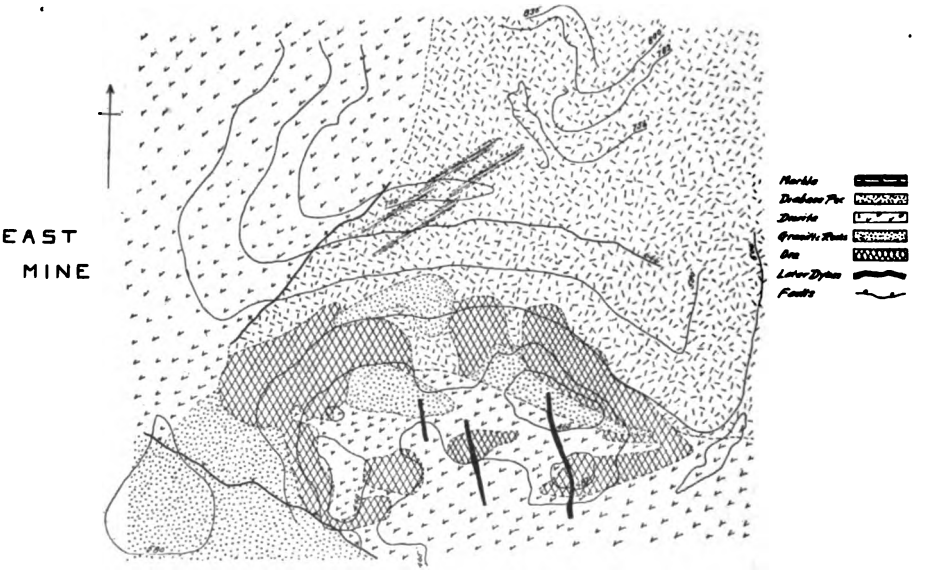

FIG. 25.

The ore in the main workings, with the associated granitic rocks, was formed at centers of intense mineralization. The ore deposits of the North Mine were formed at a greater distance from the centers of mineralization. The aplite dikes in the terrane between the two are the filling of the fissures through which the solutions traveled.

CHICHARRON (NORTH EAST) MINE

Located about ¾ mile northeast of the East Mine and now entirely idle, are the workings of the old Chicharron Mine. This mine is well north of the zone in which most of the mines lie, and in some respects differs from the others.

[39] *Op. cit.*, p. 15.

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The face of the mine is a cliff of diabase, over which the water of the Benevolencia River falls. This diabase is more or less mineralized, and

FIG. 26.—DIABASE PORPHYRY COMPLETELY REPLACED BY EPIDOTE AND QUARTZ. FROM EAST MINE. × 18. Quartz—light. Epidote—dark.

replaced by magnetite, wollastonite, and epidote. The brook bed follows along the bottom of the cliff, and with an overgrown talus pile, separates the cliff from a mass of marble. Cutting the marble is a dike of

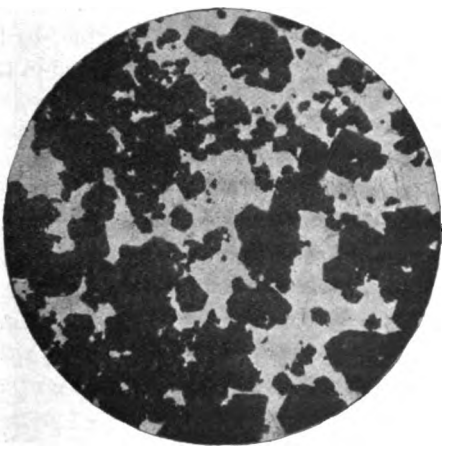

FIG. 27.—ORE FROM CHICHARRON MINE. × 18. Magnetite—black. Wollastonite—white.

magnetite, with interstitial wollastonite in small amounts (Fig. 27). A. C. Spencer describes this occurrence as follows:[40]

"At this place the magnetite seems to be an intrusive dike cutting across a mass of crystalline limestone into which it sends a short apophysis."

[40] *Op. cit.*, p. 80.

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The magnetite probably formed a dike, instead of a more diffuse form of deposit, because while the temperature was high, the pressure was not. As a result, the diffusive power of the mineralizers was not great, although they could form high-temperature minerals.

In the Chicharron Mine, also, the later dikes are in evidence.

Estancia Mines

To the east of East Mine, in the mineralized zone, lies Estancia Hill. All around the hill at from 700 to 800 ft. elevation lie mine workings. The ore in these workings is either magnetite and specularite replacing diabase, or it is the hematite-garnet-calcite combination that is eloquent of contact-metamorphic limestone, though no limestone is left. Around most of the hill, and all of the south side, is a railroad track at an elevation of about 500 to 550 ft., and it is nearly all cut in a porphyritic granite, in which micropegmatite predominates. In the valley north of Estancia Hill granite is exposed along the trail. The dump of an old tunnel run into Estancia Hill for 600 ft. at an elevation of 695 ft. is granite, and A. W. Gaumer, the Juragua Iron Co.'s chief engineer, informed the writer that all the tunnel was in the same rock. South of the railroad track runs the lower trail to Concordia, and it is all in diorite, or coarse diabase, which has been mapped as diorite.

All this evidence seems to indicate that a sheet of porphyritic granite intruded into diabasic rock with included marble, to form a sill. The orebodies at the upper contact of the granite formed in that rock to which the mineralizers had access.

Concordia Mine

This, the most easterly of the Juragua Iron Co.'s mines, offers no new evidence. It is the lowest of the mines, being at an elevation of from 300 to 450 ft. The rock and ore association is magnetite in diorite with granite near by.

The ore deposits are lenticular, and as a rule lie in an almost horizontal position. No limestone is in evidence.

VIII. GEOLOGIC HISTORY

The evidence from which the geologic history of the Firmeza district must be deduced is scanty. Almost all the field work was done in the immediate vicinity of the mines, and little search was made in the surrounding country for evidence which might give completeness to the geologic history. Enough material was found to make possible a sequential arrangement of the geologic events that produced the present

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topographic, petrologic, and mineralogic features of the district. These geologic events will be discussed in the order of their occurrence, as follows:

1. Sedimentation.
2. Igneous cycle (including ore deposition).
3. Uplift with tilting and erosion.
4. Deposition of coral limestone.
5. Emergence.
6. Submergence.

Sedimentation

The formation which appears to be the oldest in the district is the marble. As has been shown in the discussion of the rocks, this marble is Mesozoic, and probably Cretaceous in age. The constituents of the floor upon which the limestones now represented by marble were deposited, cannot be determined; nor is it possible to estimate the thickness of these early sediments, because of the lack of any continuous section. The limestone found at the highest elevation is a tuff-limestone. Because of its position it may be considered as the youngest of these early sediments, and because of the volcanic fragments embedded in it at the time of its deposition, it may be considered as representing the connecting link between the period of sedimentation, and the initiation of the igneous cycle.

Igneous Cycle

The igneous cycle began with a period of volcanic activity, to which the clastic igneous rocks of basic nature bear witness. These volcanic rocks form the southern slopes of the Sierra Maestra range, and the tops of some of the foothills. They contain remnants of interbedded limestones, and must have formed contemporaneously with the latter part of the period of sedimentation.

The next event was the invasion of these volcanic rocks and sediments by a magma of basic composition. Of this "parent" magma the present representatives are the diabasic and dioritic rocks. These rocks were left in their present position partly by the chilling of the magma, and to a lesser degree, by intrusion into the sediments and volcanic rocks.

The invasion of basic magma was followed by a long period of differentiation. During this period the liquid residue in the magma chamber became more acidic, and charged with mineralizers. This acidic liquid, with its burden of mineralizers, invaded the earlier, chilled, basic, igneous rocks in the form of a granitic batholith.

Further differentiation, resulting in a concentration of the mineralizers, ended in the injection of the mineralizers, with admixed rock material, into cooling cracks in the basic rocks above the batholith.

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There followed a separation of the highly heated mineralizers from their admixed rock material, which resulted in the formation of the ore deposits and the aplitic and pegmatitic apophyses.

It is uncertain to what part of the igneous cycle the marmorization of the limestone should be ascribed, because all the marble exposures are near or in igneous intrusive rocks, which range from diabase to granite. Any one of these may have been competent to produce the marmorization. It seems probable, however, that because it was the earliest, the invasion of the diabasic magma is responsible for the greater part of the marmorization.

What events were taking place at the surface during the igneous cycle is a question to which no answer has been found. The formation of cooling cracks, however, in the basic igneous rocks, seems to indicate that they were not very far from the surface at the time of ore deposition.

The last episode in the igneous cycle was the intrusion of basic dikes into fissures formed in all the igneous rocks and their associated ore deposits.

Uplift, Tilting, and Erosion

Because of the almost complete marmorization of the older limestones, their bedding is difficult to determine. But the pitch of the extrusive, and the few visible remnants of stratification in the marble, indicate a definite dip to the southeast. Evidence of uplift is found in the fact that limestone occurs at elevations up to 1,400 ft. above sea level.

As has been shown in the discussion of the areal geology, there is no conclusive evidence as to the exact time relation between the igneous cycle and the period of uplift and tilting, but because pressure is to a certain extent a function of depth, it seems probable that the ore deposits were all formed in an approximately horizontal plane. Their present position along an inclined plane would, in that case, be some measure of the differential elevation. Meager and inconclusive as the evidence is, it must be considered, and until evidence to the contrary is found, the uplift and tilting should be regarded as later than the igneous activity.

The coral limestones themselves show no evidence of tilting and this fact places the tilting as older than the coral limestones—that is pre-Pleistocene. Emergence has, however, continued.

Fragments of iron ore in the base of the later coastal limestones prove that this period of uplift was accompanied by erosion sufficient to cut into the ore deposits.

So far as the ore belt is concerned, the later record is one of continued erosion. Whatever else may have happened to that part of the district, no record remains except one of superficial alteration and erosion. The coral limestones and the valley topography show that dynamic forces were not entirely quiescent.

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Deposition of Coral Limestone

The deposition of the Pleistocene-Recent coral limestone began when the land was about 350 ft. lower than at present. Since then there has been oscillation with predominant emergence.

Emergence

The sea-cut cliffs in coral limestone, rising in terraces from the coast to an elevation of 350 ft., are evidence of the periodic emergence of the land since deposition of the limestone began. What the extent of the emergence was cannot be determined on the evidence in the district, because it has been masked by the most recent geologic event—submergence.

Submergence

A. C. Spencer,[41] judging from other parts of the island, places the amount of the submergence at from 40 to 70 ft. If the thickness of the fluviatile deposits in the east-west valley could be determined, it would give an accurate measure of the amount of submergence in this district.

For reasons that have been discussed in detail under the interpretation of the topography, the writer believes that this submergence represents a movement of the sea.

The geologic history of this district is a record of quiet activity. There is no regional metamorphism or profound fracturing, such as would be expected if vast diastrophic movements had taken place. The only metamorphism that has left a record was due either to igneous or to superficial causes. The only fracturing recorded is the result of the shrinkage of igneous rocks in cooling, and gives evidence of little or no local movement. Igneous forces, acting through long periods of time, dominated the rock formations. Slow uplift, alternating with periods of almost complete quiet, and accompanied by much erosion, formed the topographic features.

IX. ECONOMIC APPLICATION

Any geological investigation of more than a purely academic interest must answer some questions of a commercial nature. The present study of the ore deposits of the Firmeza district, by its inquiry into their origin and extent, is able to answer certain questions which are of economic interest. These questions refer to the continuation of the deposits in depth, a possible change in their composition if they do extend downward, and the favorable locus for the search for more ore.

[41] *Op. cit.*, p. 34.

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Continuation of the Orebodies in Depth

Since the mineralization came from below, from the granitic massif, there is no reason to believe that the ore will not extend at least to the granite. On the other hand, there is no reason to believe that the separation of the mineralizers from the granite was incomplete, so that no ore can be looked for in the granite. Where the ore lies on top of sheet-like granitic masses, like the Estancia and Loma Alta deposits, a definite limit in depth is in sight, but in workings where the massive granite is not in evidence, extensive development in depth seems probable.

Character of the Ore in Depth

The pyritization in the ores at Firmeza has made the owners of the ore deposits fear that the ores would change to pyrite in depth. However, the pyritization is later and independent of depth. There is no reason to believe that the ores found at greater depth will be any higher in sulphur than those now being mined in the lower levels of the larger mines. The lower sulphur content of the ores close to the surface is due to superficial oxidation of the pyrite. Below the level of surface oxidation the sulphur content may be expected to remain fairly constant.

The only change that seems probable is an increase in the proportion of magnetite to hematite, as the workings approach the deeper centers of mineralization.

Favorable Locus for Exploration Work

The problem of exploration work may be considered in two ways—positively and negatively. Both are of fundamental importance, although frequently the question of where to look for more ore is considered, while the equally important question of where not to look for ore is neglected. It is just as necessary to discover in which localities the search for ore will be fruitless and therefore any exploration work would be valueless, as it is to know where the search for ore will be profitable.

Obviously, rocks formed later than the ore, like the coastal limestone, cannot be expected to contain ore deposits. The granitic massif, from which the ore solutions are thought to have escaped, is also to be regarded as barren ground.

On the other hand, the granitic apophyses, aplites and pegmatitic granite are the best guides as to the location of orebodies. It is their upper surface which merits the most careful search.

Since the orebodies contain much magnetite, the magnetic survey is undoubtedly the best guide to their location. But a careful study of

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the formations is necessary to interpret such a survey, and to decide what areas of magnetic attraction are most valuable, and also what areas, in spite of a lack of this attraction, offer fields for development.

The economic application of geologic evidence will be successful only as the interpretation of the geologic phenomena is correct. At the same time the ultimate test of any geologic theory is its application in the field.

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Discussion

W. L. Cummings,* Bethlehem, Pa. (communication to the Secretary†)—In this discussion of Mr. Roesler's paper, I shall follow Kemp in using the term "granite" to refer to the acid rock called syenite by Kimball. Diorite, as I understand it, is the dark basic rock invariably closely associated with the orebodies. The types of rock of intermediate composition, mainly intrusives, are not under discussion.

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† Received Jan. 27, 1917.

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It is difficult for me to conceive that this granite is the ore-bearing agent, for the following reasons:

1. No isolated orebodies in the granite are known. It would seem that if this rock is the mineralizer, some small bodies, or at least patches and small masses of ore so situated would exist.

2. In 1909, the writer conducted explorations on the Pequena and Grande claims about 1 mile north of West Mine No. 5. This work was done after a very careful magnetic survey had shown a large and uniform area of attraction. As exploration progressed, it became evident that in this case the magnetic attraction was due to disseminated magnetite. Much ore of 40 per cent. Fe was found, but very little commercial ore was shown up. This ore was entirely in the diorite and no granite outcrops are near. The occurrence could easily have been described as "ore formation but very little ore." I cannot conceive how a remote granite intrusion could impregnate a diorite mountain with 40 per cent. of Fe, and prefer to believe that this 40 per cent. of iron was in its real home, rather than in a sort of dioritic "boarding house" after having been evicted by granitic parents.

3. The Ocaña mine, which has produced nearly 500,000 tons, shows no evidence that the granite is the mineralizer. This is admitted by Mr. Roesler on p. 115.

4. I believe it will not be disputed that all the iron-ore deposits extending along the south coast for 60 or 70 miles are of a similar origin. Evidence available at other points might properly be referred to in discussing the article under review. At the La Plata mines, about 60 miles west of Santiago, the writer has also made explorations. No granite is in evidence. At one point in the bed of a creek is a small body of magnetite about 10 ft. long, entirely surrounded by a hard fresh diorite. To me it is difficult to believe that a granite, which was not in evidence, was responsible for depositing this ore in the diorite. Had the latter been altered to a degree that would permit the entrance of ore-bearing solutions, the granite theory would seem more plausible.

In a general way I would comment on the lack of credit given by Mr. Roesler to the magnetic surveying which has been done at Juragua. One might infer, from his recommendation of magnetic surveying on pp. 126 and 127 that he was recommending something new. As a matter of fact, it is probable that no area in the United States had been mapped magnetically more thoroughly than Juragua at the time Mr. Roesler wrote his article. The first work was done in 1903 and the work was taken up again in more detail in 1906. This work, in connection with actual exploration, has continued ever since, as other work permitted. Finally, in 1915–16, N. M. Gibson was employed to go over the whole property in order to make sure that no area of attraction had been over-

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looked. The expression, "every foot of the ground has been gone over," is almost literally true in this case.

Magnetic surveying has also been done at Daiquiri.

In conclusion, it would seem that little of a practical nature has appeared in any of the four recent articles on Juragua and Daiquiri geology. If one studied the four articles carefully preparatory to planning exploration work, he would probably be badly confused in trying to reconcile statements and trying to find out just exactly what the different authors were really trying to prove.

In the meantime, the writer is of the opinion that about the only rule for exploration in the south coast of Cuba is, "stick to the diorite or 'piedra azul'" of the miners, and keep away from the granite. In exploring magnetic areas, be careful that the attractions are not caused by diorite impregnated with iron ore (iron-ore formation), or that they are not caused by areas of contact garnet rock, which, in places, is highly magnetic.

THE CHAIRMAN (WILLIAM KELLY, Vulcan, Mich.).—Some 8 or 9 years ago I was at Daiquiri, only a few miles east of Firmeza, where there are deposits of iron ore of the same general character. There is a great conical hill perhaps 800 or 1,000 ft. in height and the ore deposit appears to be somewhat in a canoe shape on one side of the hill. The mining has been done by quarrying the ore in benches. These are called levels and to go to the 10th level it was necessary to take mules and ride to the top of the hill, which was to me an upside down arrangement of numbering levels and a novel method of entering and viewing a mine.

A little copper ore can be seen in places, and not being a geologist I thought the deposit might possibly be a gossan, but none of the geologists who have written on the subject approve of this hypothesis and the great question seems to be what part, if any, the limestones have played in the origin of ore; whether these deposits are replacements of limestone and whether they are contact deposits between the limestone and the other rocks. Examinations have been made by many eminent geologists and the problem has been discussed vigorously on both sides.

The deposits are exceedingly interesting from a commercial standpoint and although they have been studied for a number of years the origin of the ore is still a difficult and somewhat controversial question.

J. T. SINGEWALD, JR., Baltimore, Md.—I have had the privilege of visiting these deposits and have been very much interested in this paper of Dr. Roesler's; having been on the ground, I can easily appreciate the difficulties under which he labored in attempting to work out the geology of that region, and he certainly deserves considerable credit for the way

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in which he has deciphered the geological relations of the Firmeza district.

There are several points in his paper I would like to discuss. First, in regard to the geological interpretations: His differs from previous interpretations in the relative age of the diorite and the granite, but you will note that Prof. Lindgren suggests the probability that those two rocks are very closely of the same age, and Prof. Kemp actually suggests, although he does not accept the idea, that the granite is probably later than the diorite, so that there seems to be no particular reason to doubt the evidence on this point and therefore to hesitate to accept Dr. Roesler's conclusion in that regard.

One of the most important features of his paper is the interpretation that he gives to the aplitic dikes. Prof. Lindgren and Prof. Kemp in their papers mention the occurrence of these dikes, but do not lay any particular stress on them, and in my own notes on the mines I have mentioned their occurrence, but neither Dr. Miller nor I at the time of our visit to the district attached any particular significance to these dikes, so in this respect Dr. Roesler's paper is a decided change in the geological interpretation.

I agree with Dr. Roesler in his interpretation of the age of the relations of the magnetite and the hematite; that is, that the hematite and the magnetite are both due to the primary mineralization, except in so far as some little hematite has developed as the result of later alteration.

The point of greatest interest, of course, in connection with this paper is the interpretation of the genesis of the deposits. Those of you who were present last year when these deposits were under discussion will remember the amusement that was created by the remark that everyone who had visited these deposits had come to a different conclusion as to their genesis, and now, as Dr. Roesler predicted at that time, he is bringing forward a still different interpretation. Although he has presented his evidence in a very able manner and though undoubtedly there is much to be said for his interpretation, I still feel inclined to adhere to the interpretation that Dr. Miller and I gave last year to these deposits.

The broad geological features of these deposits have been very succinctly stated by Prof. Kemp, in a single sentence:[1]

"They furnish a variety of deposits ranging from those of small size in streaks in limestone associated intimately and microscopically with quartz, garnet, and epidote; through larger and more extensive developments of the same with the practical extinction of the limestone; to the extreme of ore developed in great tabular masses in diorite, but still associated with the same quartz, garnet, and epidote, both in the large and in the microscopic way."

Looking at the deposits from that viewpoint, one hardly hesitates to

[1] *Trans.*, vol. **53**, p. 36 (1916),

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ascribe to them a contact-metamorphic origin, and the burden of proof would seem to lie upon him who would apply a special explanation to those of the deposits in which evidence of the original presence of limestone is lacking. Lindgren and Ross did not hesitate to ascribe such an origin to all of the deposits, but I think they made the mistake of not realizing to what extent the igneous rock itself had also undergone transformation. Prof. Kemp, on the other hand, found considerable difficulty in explaining such an ore mass as the large Lola Hill deposits at Daiquiri as due to the alteration of an included block of limestone, and consequently ascribed the whole mineralization in the case of that deposit and in the case of one or two others, to the alteration of the diorite. But, as we pointed out, there is a great deal of garnet and calcite at the southern end of that deposit, and, just beyond the mine workings, actually a limestone out-crop. The fact that this deposit is terminating at a comparatively shallow depth would indicate that originally some sort of rock was present where it is now localized, which was far more susceptible of replacement than the diorite underlying it, through which the mineralizers had to come.

These three theories, those of Lindgren and Kemp and the one advanced by Dr. Miller and myself, differ only in details and are very much alike otherwise. Several objections that Dr. Roesler advances against these theories are as follows:

"The conception of the orebodies as metamorphosed, engulfed limestone masses is difficult. The shape and position of the ore masses demand that the entombed blocks should have been comparatively thin and that they should have come to rest in every conceivable position." This is actually the shape of many of the limestone blocks, as is admirably shown in the Ocania mine, where there are plainly exposed several narrow slabs of limestone standing at a high angle within the diorite, and between two of these slabs and with the same shape and position is the Ocania orebody. The suggestion at once is that this orebody represents simply an alteration of one of those limestone blocks. The fact that this theory would require that the limestone masses should have "come to rest in every conceivable position" is just what one would expect if they are actually engulfed blocks of limestone in the diorite. Another objection he advances is that "this theory completely fails to account for the gradual transition from ore to diorite." He completely overlooks the endomorphic transformation that the diorite underwent at the same time the limestone was undergoing its alteration. A further objection is to placing "the burden of producing the orebodies upon the diorite" and that "Singewald and Miller regard the diorite as the source and as the cause of the mineralization." I think herein he slightly misinterprets what we really had in mind. The old theory that the mineralizers producing contact-metamorphic deposits were derived from the igneous

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rock immediately in contact with the orebody has long since been abandoned and it is generally agreed among geologists who have studied contact deposits that the mineralizers were drawn from the same parent magma from which the contact igneous rock itself came. In other words, we ascribe the source of the mineralization to the same parent magma postulated by Dr. Roesler. The divergence in our views is as to how and where the magmatic emissions deposited their metallic contact. Finally the statement is made: "One of the striking features of the larger Firmeza deposits is that the ores have almost obviously avoided the masses of marmorized limestone." Dr. Roesler describes 11 mines in the Firmeza district, and five of those are either directly or very closely associated with limestone. When we bear in mind that the engulfed blocks of limestone make up a very small proportion of the rock mass as compared in volume to the diorite, this seems rather to indicate that the orebodies as far as possible really sought out the limestone blocks. If you further accept our assumption that where the limestone is not present now, it is due to the fact that it has been completely altered, it would seem almost inevitable to conclude that there was a distinct relation between the limestone and the ore.

The absence of ilmenite in the Firmeza ore is used as an argument to connect the mineralization with the granitic phase of the original magma rather than with its earlier dioritic representative, and in support of this, Dr. Roesler quotes a statement from Vogt in which he says that the titaniferous ores are associated with basic rocks and the non-titaniferous with acidic rocks, but this statement applies to "Erzaussonderungen", that is, to magmatic segregations, and has no application to pneumatoly tic or hydrothermal deposits, and hence to the deposits in question. I have paid considerable attention to the titaniferous iron ores and recognize the almost universal application of Vogt's rule to the magmatic segregations. A notable exception, to it, however, is the Tofo mine of the Bethlehem Steel Co. in Chile, which, according to my interpretation, is a magmatic segregation of non-titaniferous magnetite in igneous rock which ranges from dioritic to more basic in composition. That the rule does not apply at all to deposits other than magmatic segregations is strikingly illustrated by the deposits of Cornwall, Pa., which are non-titaniferous but occur in limestone associated with diabase and presumably derived from the diabasic magma; and by the fact that the only known example of titaniferous magnetite of contact-metamorphic origin is one of no commercial importance at Cebolla Springs, Gunnison Co., Colo., which I described a few years ago merely because it was an exception to the statement by Beyschlag, Krusch and Vogt that contact-metamorphic magnetites are never titaniferous. The non-titaniferous character of the Cuban ores affords no argument, therefore, for either theory.

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In support of his own theory, Dr. Roesler cites the constant relationship between the orebodies and the aplite dikes and granite apophyses. In so far as the relationship exists, it is not surprising, you would even expect it in accordance with the other theories: that is, if the mineralizers and the aplitic dikes came from the same parent magma it would be only natural to suppose that the area of mineralization also coincided more or less with the area in which the aplitic dikes were intruded. But the relationship between the aplitic dikes and the orebodies does not seem to be intimate enough to make them competent vehicles for the mineralizing solutions, though I am not prepared to say that they had nothing to do with it at all. If you take an orebody, such as the Lola Hill deposits, in the Daiquiri district, the amount of aplitic material is not great, nor are the aplitic dikes connected in such a way with the orebodies as would make one think that they had been the channels used by the mineralizers in producing the orebody, and if they were, one would expect the ore to be coextensive with the dike itself, and the objection, that I pointed out in the case of Prof. Kemp's interpretation of this deposit applies here—that the orebody pinches out in depth. Roesler himself, in determining the economic conclusions that might be drawn from his interpretation, says: "Since the mineralization came from below, from the granitic massif, there is no reason to believe that the ore will not extend at least to the granite." The largest orebody of the region appears to terminate short of that depth.

To sum up, points of similarity in all the recent theories are that they ascribe the mineralizers to the same parent magma from which the inclosing diorite was derived and that the mineralization was pneumatolytic or nearly so. I differ from Dr. Roesler in not feeling it necessary to ascribe to the aplitic dikes the rôle of vehicle for the mineralizers. I also feel that the restricted localization of the orebodies makes it necessary to postulate special conditions at the points of localization In many instances, the relations of the ore to limestone afford unmistakable evidence of such special conditions. The assumption that the presence of limestone has determined the localization in all cases seems, therefore, reasonable.

BENJAMIN L. MILLER, So. Bethlehem, Pa.—Dr. Singewald has well expressed the ideas which we formed of the Firmeza district while in that region. I wish to call attention to only a few additional points.

On page 81 occurs the following statement: "The limestone cliffs, with their sea-cut terraces, have been interpreted as evidence of periodic movements of the island, but it seems to the writer that the widespread occurrence of a terrace about both Jamaica and Cuba at the same elevation above sea level indicates a movement of the sea surface, rather than of the land."

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Far larger areas than those mentioned would have to be investigated before one should conclude a general lowering of the sea surface. While there are many well-developed sea-cut terraces along the Atlantic Coast of the United States, it is doubtful whether these can be correlated with those existing in Cuba and Jamaica. It is certain that uplifts and depressions of large areas in which there was little warping have taken place in the West Indies, as pointed out by Dr. T. W. Vaughan, but the effects of such movements are much less extensive than terraces which might result from a general withdrawal of the ocean waters.

In regard to the origin of the orebodies, I feel, as do a number of other persons who have been in the region, that the exact origin of the orebodies is still somewhat in doubt. All of the facts necessary to reach a definite and positive conclusion have not yet been obtained. With the evidence at hand an investigator may place particular emphasis upon certain points and minimize other lines of evidence and so reach conclusions quite at variance with views formerly held. This is what has been done thus far. The region remains an interesting one for the geologist and no doubt diverse views regarding the processes which have taken place in the formation of the orebodies will be held for a long time, as there is little reason to believe that the necessary evidence for a final decision will be soon obtained.

I think Dr. Roesler has performed a very important piece of work. He has collected much valuable information, has outlined the problem in a commendable manner, and has furnished considerable evidence for the conclusions which he formulates. It is no fault of his that his views are not accepted in their entirety. In some particulars the speaker must dissent.

Opposed to the view which Dr. Roesler expresses concerning the fissures or fractured zones through which the ore solutions have ascended is the sharp delimitation of some of the orebodies in depth. If the heated solutions rose through such fissures, replacement of the country rock would seem to have taken place throughout a zone of greater depth and the lower limits would be much less sharply defined.

The shape of the orebodies seems to me to be much as one might expect if there had been engulfed masses of limestone that initiated the precipitation of the ore minerals. In many places the process continued even after the limestone had all been replaced and portions of the orebodies represent replacement of the diorite.

The fact that granite and aplite dikes have not been observed in other places in Cuba where similar orebodies have been observed, as mentioned by Mr. Cumings, also seems to throw doubt on the intimate relation between the orebodies and the aplite dikes which Dr. Roesler maintains.

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JOHN D. IRVING, New Haven, Conn.—I have not seen these deposits, but when Dr. Roesler prepared this paper I had the pleasure of going over it in very great detail with him and saw all his specimens and know some reasons why he arrived at his conclusions.

In the first place, these iron-ore deposits with their contact-metamorphic minerals, garnet and epidote, and with the magnetite associated with them, with the neighborhood of the limestones in the vicinity, at once suggest to anybody who takes them up that they belong to the ordinary category of contact-metamorphic deposits, one of the limestone masses that have been more or less completely altered as the case may be. In that conception there is nothing that does violence to any of our previous ideas in regard to contact-metamorphic minerals. Everything fits in beautifully.

I remember the first time that I had opportunity to learn anything about these particular deposits. A former student of mine, Dean Corsa, went down there and made a careful examination and wrote a little paper which was not published because it was never completed. I said, "The only thing I do not like about your paper is that it seems to me that these things must be contact-metamorphic deposits in limestone, and you do not believe it," so that my whole idea was in favor of that interpretation because it was the customary interpretation of the conditions which existed or are said to exist there.

Dr. Roesler brought his material back and I said to him, "The first thing you want to be careful about is to see that these things are not contact-metamorphic deposits in limestone masses, because that is what I believe they are." So that you can see that in supporting Dr. Roesler's point of view, I have been personally persuaded that the contact-metamorphic idea is not correct.

Now there are two questions, it seems to me, involved in this matter. The first is, is it possible for the solutions which arise from these granite pegmatite dikes which have been given off by them or for solutions in general in that region to have altered the dioritic rocks into contact-metamorphic minerals? And the other question is whether or not that had anything to do in forming these.

In regard to the first one, I think the most significant thing is a photograph of a small dike which Dr. Roesler has in his paper on page 112, Fig. 19, in which he shows a garnet band between aplitic granite and diabase porphyry. That was the thing that convinced me that these solutions were capable of producing contact-metamorphic minerals without the assistance of any limestone in the vicinity of the dike. The picture does not give as good an idea as the dike itself. At the edges of the dike is a beautiful layer of garnet which makes its way out in the dioritic rock to a short distance and is more or less intergrown with it and appears to be a replacement or alteration of it. I can see no conceivable

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way in which such a band, continuous over a considerable area of the exposure of that dike, can have been developed in any way except by alteration of the dioritic rock itself.

It is not an ordinary case of contact metamorphosis, because there is something added that is not in the diorite. So we must look upon it as a contact-metamorphic alteration from the dioritic rock. If that is true (I can see no escape from it) why is it not possible that that same diorite would have been able to produce larger masses of contact material without the necessary agency of limestone? Is it correct that we should think these limestone masses have necessarily been the determining feature in this occurrence just because we have in other places found them to be?

It seems to me the most significant feature is the arrangement that Dr. Roesler suggests in his letter, where we have larger dimensions in horizontal and in two vertical directions intersecting one another.

It seems as if there were a series of rocks, and that would give rise to this linear arrangement and this development in certain directions. Of course, it is possible that limestone masses can be engulfed in such a way that they can simulate conditions which would result from a certain distribution of fissures and therefore that seems to me to be a strong point.

L. C. GRATON, Cambridge, Mass.—Without attempting to discuss the facts or the arguments in this interesting paper, because they relate to a deposit of which I have no first-hand knowledge, I am nevertheless impelled to refer to one feature of the paper in its relation to two specific topics.

It is to be regretted, it seems to me, that this much-discussed question of the origin of these Cuban iron-ore deposits, and especially their relation to the limestone, appear not to have been settled by Dr. Roesler's paper. If it could have been established to the satisfaction of all that these iron ores, so similar to contact-metamorphic deposits, have, nevertheless, been formed directly and solely by igneous influences and without the aid or intermediation of limestone, then it would be easy and reasonable to believe that true contact-metamorphic deposits rich in iron have derived that metal from igneous emanations, rather than the view, which some have advanced, that the iron is simply a recrystallization of ferruginous impurities in the limestone beds.

My own feeling in reading Dr. Roesler's paper is that his failure in some directions to carry complete conviction is due to the brevity of his presentation. I feel that back of the conclusions he has here recorded he probably has a repetition and an accumulation of evidence which, in my opinion, it would have been better to present than to omit.

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The second point in which I was especially interested received similarly abridged treatment. I refer to the question which occupied so prominent a part in the discussion of these Cuban deposits at the Institute meeting a year ago;[2] namely, whether the hematite was derived from the magnetite by normal oxidation near the surface, or whether both iron oxides are products of the initial ore-forming processes. Prof. Irving tells me that he feels that Dr. Roesler has settled the question definitely, at any rate for these particular deposits, and I am glad to hear Dr. Singewald's opinion, likewise, that the hematite is in the main a primary constituent of the ore. But especially in view of the fact that no longer than a year ago Dr. Roesler was inclined to the belief that the hematite is a superficial oxidation product,[3] it is, I think unfortunate that he has not now presented more fully and convincingly the evidence that has caused his view to change.

It is to be hoped that Dr. Roesler may be prevailed upon to devote further attention to this particular problem, and by means of photomicrographs and extended discussion bring more light to bear upon this question which it seems to me is one of considerable importance.

C. P. BERKEY, New York, N. Y.—I think I ought to say one or two more words before allowing this matter to drop. I see quite clearly that Dr. Roesler has not completely silenced everybody, but it does seem to me, as the last speaker suggests, that Dr. Roesler has written too brief a paper to clear up everything that we may wish to know. Evidently he has not given all of his facts, although he is clearly satisfied and convinced in his own mind. He writes, for example, in a recent letter, as follows:

"How a bunch of entombed limestone blocks came to assume such a regular alignment as I have spoken of is not explainable, it seems to me. Besides, I have found all sorts of specimens of mineralized diabase with the retained diabasic structure; and, after living with these deposits for 6 months, I feel that I know that in 95 per cent. of the cases limestone had absolutely nothing to do with the matter."

That is a pretty strong statement from a man who has been on the ground 6 months and has been successfully guiding explorations. Some of the objections to Dr. Roesler's interpretation relate to the connection of the ores with aplites. One objection raised against his interpretation is that there is no aplite in sight at some of the ore pits. It does not seem to me that this is a very serious matter. He does not claim that aplite must be in immediate touch with these ores; it is only that there is a genetic association with these rocks as end-products from the same magma. The solutions or emanations which produced the ores seem to have been able to penetrate the surrounding rocks farther than the

[2] See discussion of paper by Lindgren and Ross, *Trans.*, vol. 53, 59–66 (1916).
[3] *Loc. cit.*, 66.

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associated matters now forming the aplitic dikes, which may be only the residual acid contents of the original intrusions.

I take it from what I have heard today that everybody admits that the mineralizing solutions have come up through weaknesses in the rock. I believe, also, everybody has admitted that the diorite is mineralized. If it is mineralized, and if some of the deposits are in that kind of rock, there is no necessity of attaching particular importance to limestone at all. This is very nearly in agreement with Prof. Kemp's point of view. He says there are two types of deposits: those associated with limestone, constituting contact-metamorphic deposits of rather common sort; and others that are mineralizations of diabase. Neither Dr. Roesler nor Prof. Kemp consider the wall-rock type at all essential.

It seems to me that the situation is fairly well defined, so far as Dr. Roesler's views are concerned. The guiding feature is the fractures and these are reasonably systematic. Physical conditions have controlled and guided the mineralization, rather than the chemical composition or nature of the rock. These ores have been deposited at points where their activity or carrying capacity has been reduced so that crystallization or deposition must begin; and mineralization has resulted wherever and whenever such conditions have been reached, no matter what the rock was. If diorites formed the walls, then diorites were affected and the deposit was made.

On the question of the hematite, he says there are two types—primary hematite intergrown with magnetite, and secondary hematite which is a simple superficial and earthy product. Where the hematite which he considers a purely secondary product is developed, the sulphides are destroyed. Elsewhere the sulphides still remain. These views look reasonable to me.

MAX ROESLER, Firmeza, Oriente, Cuba (communication to the Secretary†).—It is substantially admitted by all who have recently been in touch with these deposits that the orebodies lie in the fine-grained dark-green rocks, called diorite by most writers and diabasic by myself. The three points of disagreement concern the agency of included limestone blocks as causes of deposition, the agency of the granite as a source of the mineralization, and the primary or secondary nature of the hematite.

In regard to the first question there is a complete sequence of opinions. Professor Lindgren believes that the orebodies are localized in the limestone and does not admit replacement of the diorite. Professors Singewald and Miller admit the replacement of diorite (diabase), but only in the vicinity of limestone. Professor Kemp and I are in entire accord

† Received April 5, 1917.

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in considering the mineralization of the diorite (diabase) as entirely independent of the limestone.

To the evidence already submitted in regard to the mineralization of the diabase, I have very little to add. Duplication, of garnet and epidote rims in diabase next to aplitic dikes, and of cases of retained porphyritic structure in thoroughly mineralized diabase porphyry, has made me feel very sure of the replacement of the diabase. My work in the Daiquiri district has added to the number of occurrences in which, to my knowledge, no limestone is in evidence near the ore. It is, of course, not possible to prove that where there is now no evidence of limestone, there never was any. But since the prospect work based on the theory that the ore was localized along a system of fractures has in several cases succeeded in cutting extensions of the ore zone, I have faith in the latter theory. Furthermore, in the recognizable contact bodies in limestone, the mineralization is preponderantly in the form of specular hematite. In the cases where a coarse diabase porphyry has been replaced, and the porphyritic structure maintained, the mineralization is preponderantly magnetite. In the larger orebodies in which no limestone is in evidence, the mineralization is largely massive magnetite with an intergrowth of a minor and varying amount of hematite. It seems fair to believe that the geochemical processes that produced the larger bodies are more closely allied to those that produced the replacement of the diabase porphyry than to those that acted in the case of the limestone.

The question of the relation between ore and granite rests on two points: first, the relation of the granitic apophyses to the granite massif; second, the relation of the granitic apophyses to the ore.

The first point is a question of fact. As I have already said in the article under discussion (page 90):

> "The granitic apophyses, as now exposed, are rarely more than 150 ft. above the top of the granite massif, and their downward extension where exposed by erosion can be seen to merge into the granite."

Further study convinces me that in several places the granitic apophyses have reached a higher limit away from the granite, for the rest I stand by that statement.

The second point is a question of interpretation. Any interpretation that seeks to dissociate these apophyses from the ore must offer adequate explanation for the following facts.

In 13 of the 15 orebodies that are being mined or prospected in the Firmeza-Daiquiri region today, there is an intimate or at least extremely close association between granitic rocks and ore. In fact, a study of the Juraguá Iron Co's tonnage sheet shows that over 84 per cent. of the ore mined since 1908 comes from mines in which there is such an association. In deriving those figures, the Ocaña mine was considered as away

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from granite. Considering that mine as directly connected, the total figure would become over 97 per cent.

In all the orebodies that are being mined or prospected near Daiquiri, there is such an association. That is, if one kept away from granitic rocks in that area the mines would be shut down.

The granitic apophyses contain inclusions of magnetite. They are accompanied by garnet rims. They can be seen to change into epidote and quartz veins.

I can see no interpretation that will explain all the facts, except that these apophyses were a feature of the mineralization, and were contemporaneous with it as well as extending beyond it in duration. Dr. Berkey has already answered the suggestion that if they were associated with the ore they would have to be coextensive with it.

It seems to me, so far as the theory of ore genesis is concerned, that the evolution of highly siliceous emissions from a basic magma would be more than likely to be accompanied by the production of an acidic magma by differentiation. In this case the acidic magma has reached near to the point of deposition of the emissions and has been exposed by erosion. But the difference in viscosity between such emissions and aplitic material must be so great that the emissions might travel very far from their accompanying magma. So, even if there were no exposure of granite, the amount of siliceous material accompanying the ore would, in my opinion, justify the postulating of a magma of a more siliceous nature than the diabasic, as a hidden and immediate source for the mineralization.

The Ocaña mine has been cited as an example of an orebody whose genesis is different from the genesis that I worked out for the other mines in the Firmeza district. As a matter of fact, it is the poorest of all the active mines for study, because the ore lies along a hillside, close to the surface, and both it and the wall rocks have been subjected to extreme superficial alteration. However, I have found some further evidence in regard to the mineralization of this mine, in the course of my work during the last 7 months.

There is undoubtedly replacement of limestone by specular hematite, as evidenced by typical contact phenomena. There is equally definite replacement of the coarse diabasic porphyry, as evidenced by a friable mass of magnetite containing recognizable, kaolinized feldspars. These two types of mineralization are in close proximity in this mine. On the other hand, we have in most of the mines only one type, or at least one type vastly predominant. Do not these circumstances point to a dominating cause outside the nature of the rocks rather than to an interdependence? I feel that they do, and that the dominating cause was physical; *i.e.*, temperature and pressure.

In some of the lower and newer workings I have found an aplitic

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dike, and the opening up of some new ore farther down the hillside brings the bottom of known ore to within 75 ft. vertically of the outcrop of the contact of the diabase and a mass of quartz porphyry. The superficial alteration is too great in this mine to make it possible to prove the relationship. On the other hand, there is quartz andesite all through the mine, and some aplite. The independence of the mineralization and acidic rocks is certainly not shown.

Mr. Graton's suggestion as to the need of more work on the question of hematitization is in accord with my own ideas. I hope at some future date to have more to say on this subject.

Professor Miller's criticism of my statement in regard to the formation of the lower coral limestone terrace by sea movement is entirely justified. The statement, as quoted by him, is far more definite than I had any right to make it. It was meant to be more in the nature of a suggestion.

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Recent Geologic Developments on the Mesabi Iron Range, Minnesota

BY J. F. WOLFF, E. M.*, DULUTH, MINN.

(New York Meeting, February, 1917)

DURING the past 4 or 5 years, much has been added to the detailed geologic knowledge of the Mesabi Range. This has not been in the direction of discovery of any new fundamental facts, but of detailed study, subdivision and correlation of different parts of the whole formation and of individual orebodies. Prior to this time, mining engineers in the district were so engaged with the commercial interests of exploring and developing orebodies that close geologic study and subdivision were done in a few instances only. The demand for refinement of work in this direction has led the engineers of the Oliver Iron Mining Co. to do extensive structural subdivision and correlation in all exploration work during the past 4 years. Such work has become a commercial necessity rather than a scientific refinement, and at the present time is being extended to all parts of the Range.

In the summer of 1914, the author of this paper wrote a series of articles on the orebodies and special features of the Mesabi Range, which was published in the *Engineering and Mining Journal*, issues of July 17 to Aug. 7, 1915. Since that time studies of subdivision of the iron-formation have been extended considerably. The principal feature of this paper is the presentation of the subdivisions of the iron-formation. To this is added a discussion of the relations of the orebodies to the gentle folding and fracturing of the formation, and special features of the Range.

The outline will be as follows:

I. General geology.
II. Subdivision of Biwabik iron-formation.
III. Relation of orebodies to folding and fracturing of the iron-formation.
IV. Special features.

Acknowledgment is due to W. J. Olcott, President, and John Uno Sebenius, General Mining Engineer, of the Oliver Iron Mining Co., for permission to use the information presented in this paper.

* Mining Engineer, Oliver Iron Mining Co., Duluth, Minn.

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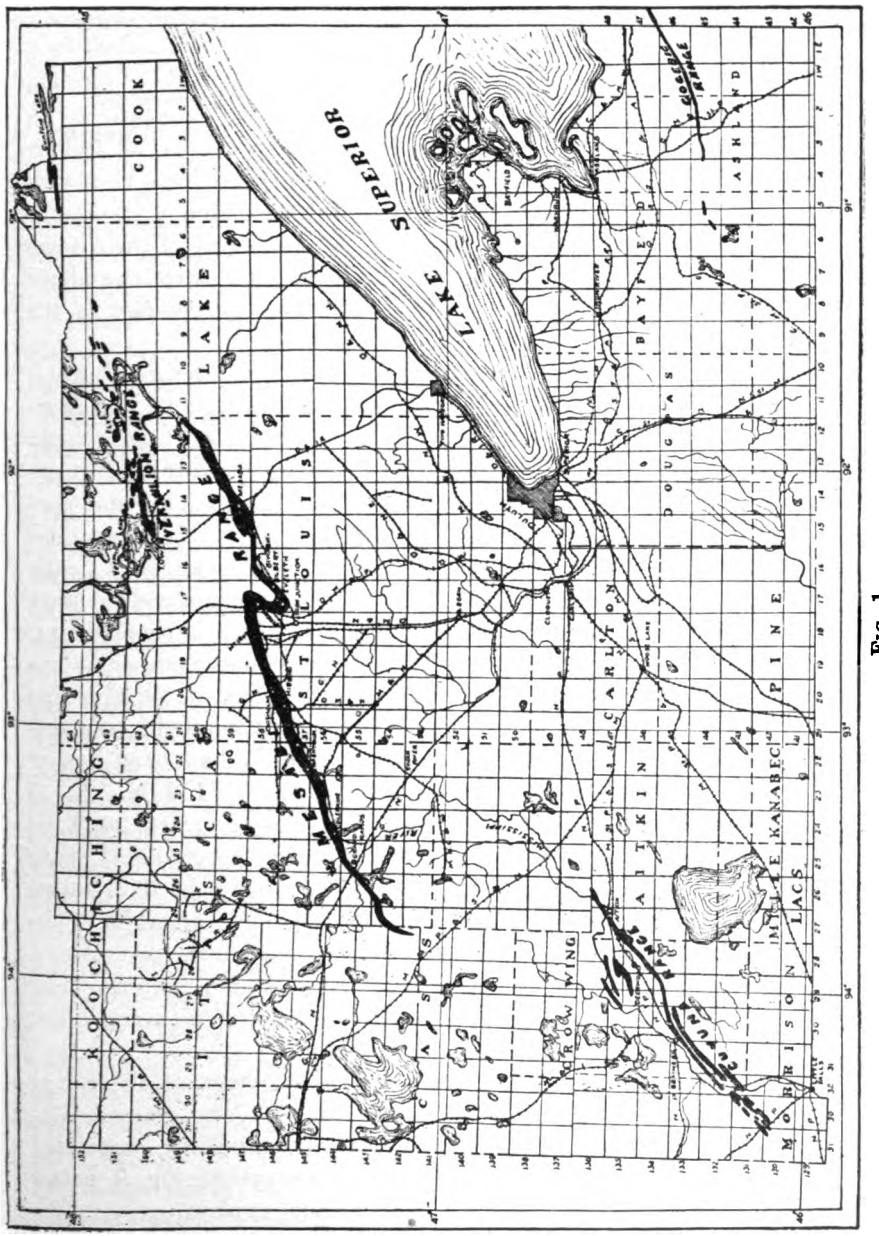

Fig. 1.

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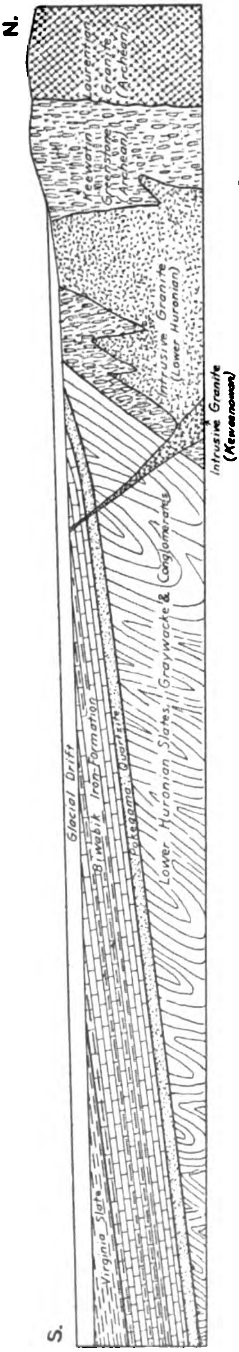

FIG. 2.—GENERALIZED CROSS-SECTION SHOWING RELATION OF IRON FORMATION TO ASSOCIATED ROCKS.

General Geology

Correlation

Although the general geology of the Mesabi Range has been described most exhaustively in *U. S. Geological Survey Monographs* Nos. 43 and 52, and elsewhere, a short synopsis may be a necessary preliminary to the detailed discussion of the Range presented in this paper, because many of the readers of this article may be unfamiliar with the above publications or with the general geology of the Mesabi district.

The topographic feature of the Mesabi Range is a line of fairly prominent and continuous hills ranging in elevation from 1,400 to 1,900 ft. above sea level, composed of a complex of granites, greenstones, green schists, slates, graywacke and conglomerate. Resting unconformably upon this basement complex, and sloping away at a gentle angle to the southeast, is a series of sedimentary rocks, the middle member of which is iron-bearing. The outcrop of this iron-bearing member is the geologic feature known as the Mesabi (or Missabe) Iron Range. Within this formation the iron ore-bodies are found. Its outcrop has been traced by explorations from Sec. 12, T. 142 N., R. 25 W., northeastward to Birch Lake, in Sec. 26, T. 61 N., R. 12 W., a distance along the strike of about 112 miles. Its width varies from 3/8 to 3 miles, due to variation in the dip and thickness. The sedimentary series above mentioned consists of a basal quartzite, named Pokegama, an intermediate iron-formation, named Biwabik, and a top black slate, named Virginia slate.

The location and general extent of the Range is shown on the Map, Fig. 1. The general relations of the iron-bearing member to associated rocks are shown by the cross-section, Fig. 2, better than further description could explain.

Instead of the complete correlation table as given by the U. S. Geological Survey, a simplified geologic column is herewith given, which is adequate for the engineer in the district.

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Quaternary System		
Pleistocene Series	Glacial drift, 0 to 300 ft.	
Unconformity.		
Cretaceous System	Conglomerates and shales, 0 to 50 ft.	
Unconformity		
Algonkian System	Duluth Gabbro	East end of range.
Keweenawan Series	Embarrass Granite	
Unconformity		
Upper Huronian Series (Sedimentary)	Virginia slate 0 ft. to great thickness. Biwabik Iron-formation, 475 to 775 ft. Pokegama Quartzite, 50 to 150 ft.	
Unconformity		
Algonkian and Archean Series	Basement complex of Slate-graywacke-conglomerate series, granites, greenstones, greenschists and porphyries.	

From a commercial standpoint the Upper Huronian series, Virginia Slate, Biwabik Iron-formation, Pokegama Quartzite are the important rocks of the district.

The characteristics of the different formations need not be discussed. This will be done for the iron-formation later on.

Orebodies

The Upper Huronian Quartzite-Iron-formation-Slate series is an ordinary (except for the iron) series of clastic sediments, deposited in fairly shallow water, contemporaneous with the middle member of which was deposited or precipitated out of solution an enormous quantity of iron, chiefly in the form of a ferrous silicate, $FeSiO_3$, occurring as coarse and minute green granules of the mineral greenalite, embedded in a matrix of chert. Iron carbonate, grünerite, amphibole, actinolite, and hematite occur in small quantities. Original magnetite in bands and finely disseminated grains occurs in much larger quantity than has been recognized heretofore, in all parts of the district. The bedding-planes of all three members are approximately parallel. Interbedded with the iron-formation in certain horizons are numerous slate layers, varying in thickness from a few inches to many feet. The iron-formation from Virginia slate to quartzite varies in thickness from 475 to 775 ft., the average being about 600 ft.

After this series of sediments had been laid down, earth movements raised them above water level, allowing erosive agencies to cut through the overlying slate into the underlying formations. These earth movements warped the formations and cracked the brittle iron-formation quite extensively, allowing surface waters to enter its upturned edges. Especially where such cracking was pronounced, the ground-waters entering the iron-formation, carrying carbon dioxide in solution, attacked the ferrous iron compounds and oxidized them. In such localities much of the ferrous silicate has been changed to a hematite-chert rock, the hematite occurring as bands and as disseminated particles in the chert, the rock still retaining its solidity. The whole iron-formation, whether

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thus altered or not, is a ferruginous chert with interbedded slate layers and locally is called "taconite." This term should apply strictly to the ferruginous chert and not to the slate layers, though in some horizons the slate bands and chert layers are so intimately interbedded as to make a distinction quite impossible.

Where the cracking has been most intense the circulation of ground-water has been most vigorous, the solvents in the ground-waters have leached out the silica and other minor constituents of the rocks and have oxidized and left in place the iron. Such residual material now constitutes the orebodies. They are surrounded on all sides by the rock walls of the iron-formation from which they are derived. Porespace was developed by this removal of silica and the settling or slumping in place of the layers of the orebodies is a characteristic feature. The typical orebody thus developed has a trough structure and an irregular trough shape. Orebodies vary in size from a few acres to several hundred acres, and from a few feet to several hundred feet in thickness. In many

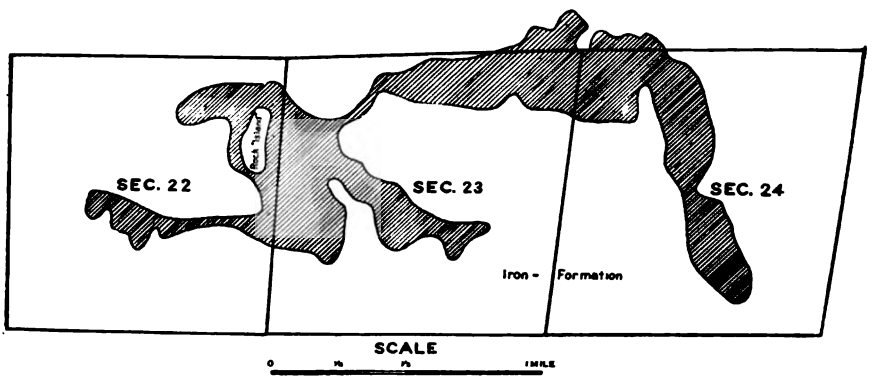

FIG. 3.—PLAT SHOWING SHAPE AND AREA OF TYPICAL OREBODIES.

places small troughs unite to form a large one. Fig. 3 shows the shape and area of a typical orebody. While the trough orebody is the typical one, there are two other types, namely, the flat-layered body and the fissure-type orebody. The former is either the remnant left by the erosion of a former trough-body or it is an ore layer continuing down the dip from a trough-body. Usually such a layer has a rock (slate) capping. The fissure-type orebody is an incompletely developed trough-body and is usually associated with a larger trough orebody. Fig. 4 shows the development of a trough orebody on the axis of an anticline, where cracking has been pronounced. All four stages here shown can be observed in the field. Notice the slumping of ore layers near the rock walls. The features of the three different types of orebodies were discussed at length in the *Engineering and Mining Journal*, July 17 to Aug. 7, 1915, and will not be repeated here.

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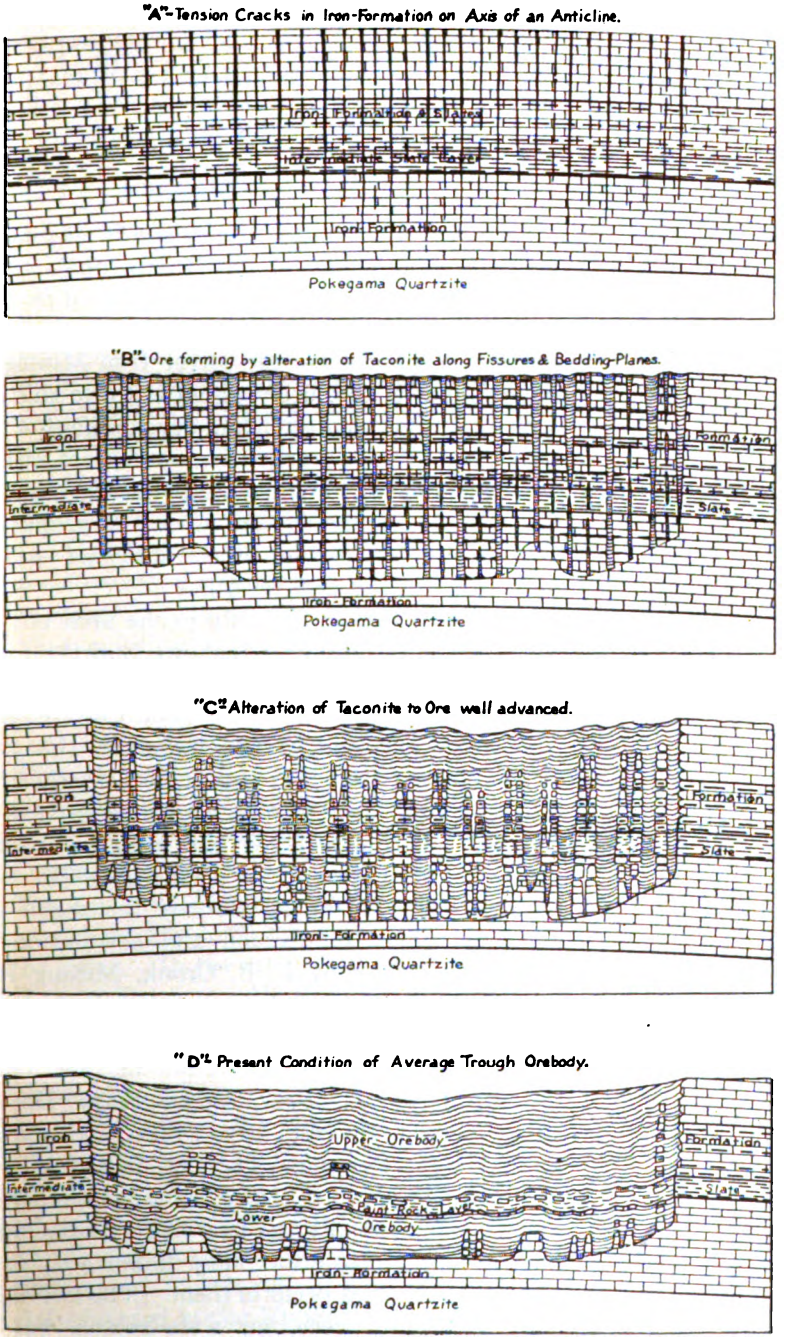

4.—Cross-Sections Showing Stages in Development of a Trough Orebody on the Axis of a Gentle Anticline.

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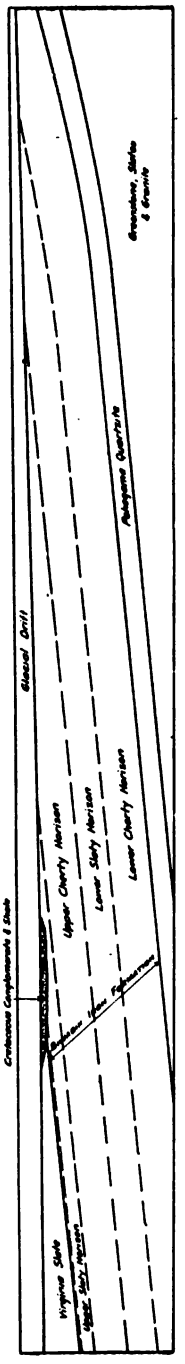

FIG. 5.—GENERALIZED CROSS-SECTION SHOWING SUBDIVISION OF THE BIWABIK IRON-FORMATION.

Covering the entire iron-formation, except at a few isolated spots, is a mantle of glacial drift, varying in thickness from a few feet to 300 ft.

SUBDIVISIONS OF THE IRON-FORMATION

Detailed Subdivision

The item of principal interest and most practical value to mining men in the Mesabi district, presented in this paper, is the subdivision of the iron-formation into its several horizons. As indicated above, the engineers of the Oliver Iron Mining Co. have found it absolutely essential to do this kind of work in the exploration and development of their orebodies. Undoubtedly others will meet with the same experience. The determination of the structure of an orebody depends upon the proper subdivision and correlation of the different ore and rock layers in the drill records.

Such correlation, not only in one area but over the length of the Range, has shown that the formation can be subdivided into four principal horizons, each of which has remarkably uniform characteristics from one end of the Range to the other. Each horizon can be subdivided further. Of course, the thickness of each horizon is not uniform because the thickness of the whole formation varies. There are local peculiarities in some places, but these do not destroy the major subdivisions. This subdivision and correlation has been done chiefly by F. B. Cronk, Mining Engineer for the Oliver Iron Mining Co., and the writer.

Fig. 5 shows the four subdivisions which can be made in any part of the Mesabi district. From the top down they are, Upper Slaty Horizon, Upper Cherty Horizon, Lower Slaty Horizon and Lower Cherty Horizon. They are named from the predominant physical characteristic of the rock in them. Commercially the two cherty horizons and Lower Slaty Horizon are most important, as the principal orebodies occur in them. The two cherty horizons contain very few slate seams, but the slaty horizons contain a considerable number of interbedded chert layers and a great amount of greenalite. The lower

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half of the Lower Slaty Horizon contains the most slate, the bottom 30 ft. or so being almost pure slate. But the upper half is slaty or banded in structure rather than in composition of the rock layers. The subdivisions are made by eye rather than on any chemical basis or microscopic examination.

The Upper Slaty Horizon is composed about half and half of slate layers and greenalite lenses. The upper part approaching the Virginia slate is the more slaty. It is separated from the Virginia slate by a layer 10 or more feet thick of a carbonate rock, probably an impure limestone.

Two cross-sections, Figs. 6 and 7, are presented to show in detail the four main subdivisions, the minor subdivisions of each, the relations of the orebodies to these horizons and the kind of ore derived from each.

Fig. 6 shows an east-west cross-section, looking north, through the Adams, Hull, Nelson, Leonidas orebodies just west of Eveleth. It is the best cross-section that has been or can be made from present explorations, so far as the author knows. The location of the section is shown on the accompanying map, Fig. 11, and is approximately at right angles to the strike. It is developed from drill-hole classifications, as can be seen, but practically the entire section of ore is being developed now by open-pit and underground workings. Because this cross-section is taken along the longitudinal axis of the ore trough, the trough structure does not show. In the area of drill holes 6 to 8 inclusive, a north-south trough is tributary to the east-west trough through which this cross-section is taken, and the trough structure is apparent. The gentle warping of the entire series can be noted. The principal warping is at right angles to the strike, however.

The relations between the different horizons and their derivative ores are so evident from the cross-section that a detailed description need not be given. In the area of drill holes 2 to 15 inclusive, the taconite in the Lower Slaty and Upper Cherty Horizons is so badly altered that classification is very difficult.

A few features may call for comment. The black slate indicated at the base of the Lower Slaty Horizon is the so-called "Intermediate" Slate, which makes the characteristic paint rock layer of the typical Mesabi orebody. The orebodies first developed on the Range were located in that part of the formation shown between drill holes 15 and 24; therefore, it is easily seen that the typical orebody had five layers—upper and lower blue ore layers, upper and lower yellow ore layers, and an intermediate paint rock layer.

Interbedded in the Upper Cherty Horizon and the top of the Lower Slaty Horizon are distinct conglomerates. At the base of the Upper Slaty Horizon there is also a fine interbedded conglomerate. The Upper Cherty conglomerate is continuous from the far eastern part of the Range

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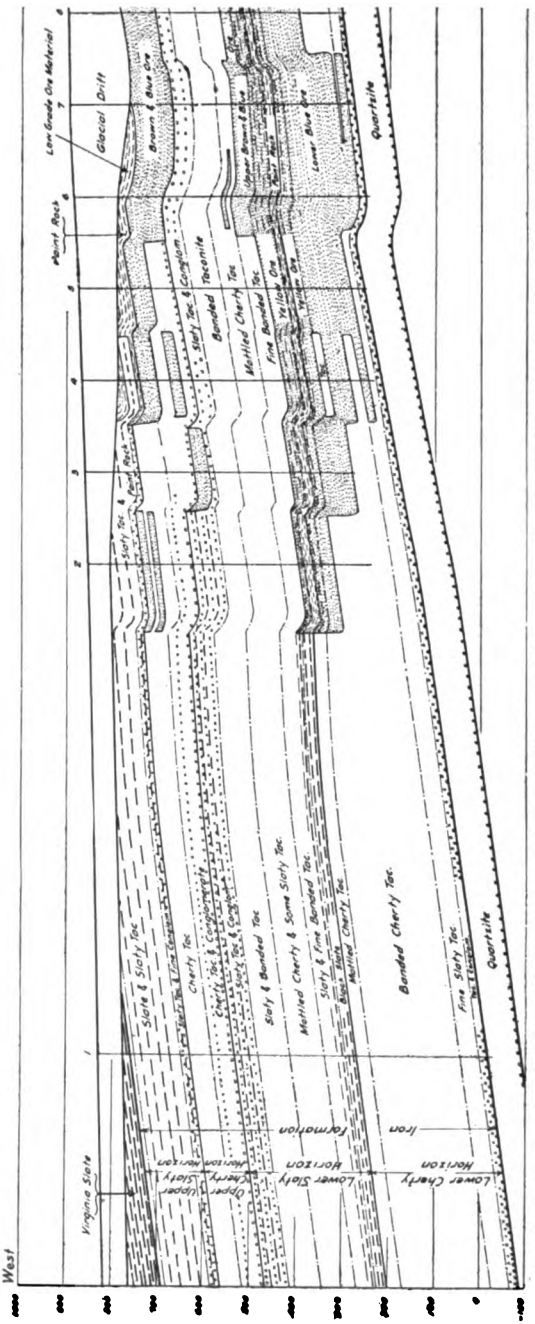

FIG. 6.—LONGITUDINAL CROSS-SECTION OF A LARGE TROUGH OREBODY SHOWING ORE DERIVED FROM ALL HORIZONS OF THE IRON FORMATION. (*Continued on two succeeding pages.*)

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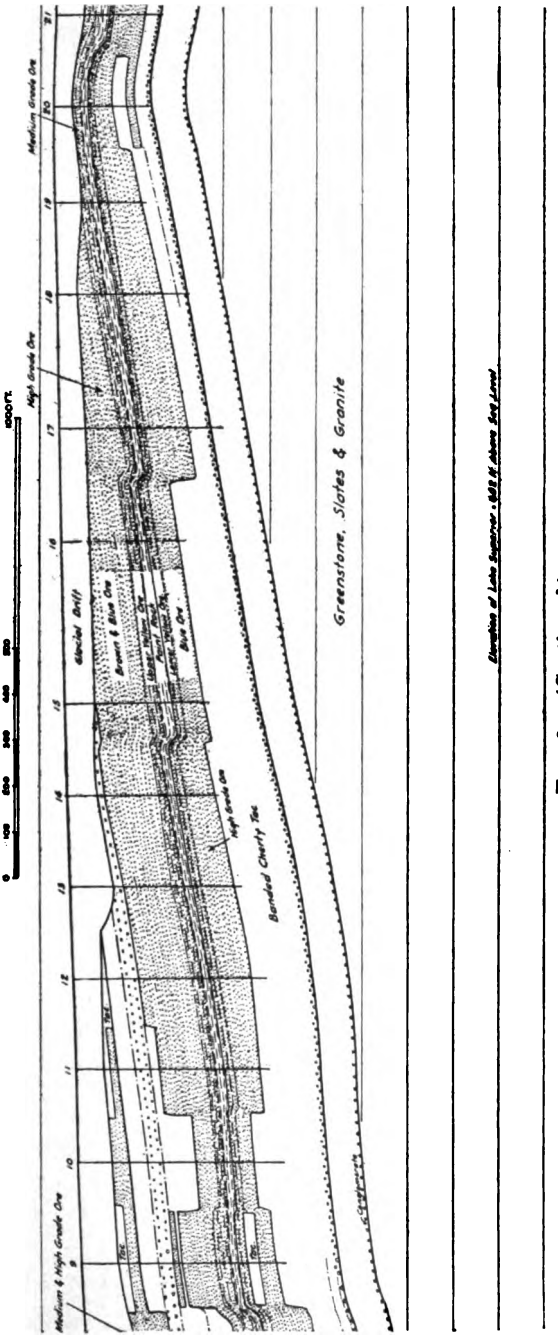

FIG. 6.—(*Continued.*)

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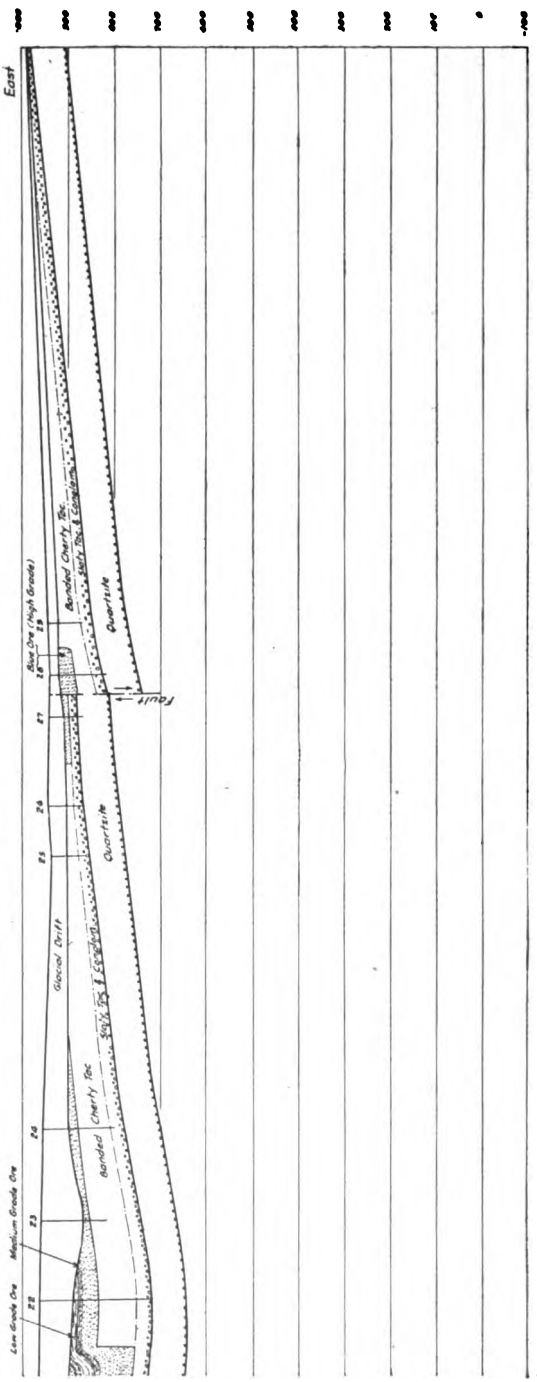

FIG. 6.—(*Continued.*) LONGITUDINAL CROSS-SECTION OF A LARGE TROUGH OREBODY SHOWING ORE DERIVED FROM ALL HORIZONS OF THE IRON FORMATION.

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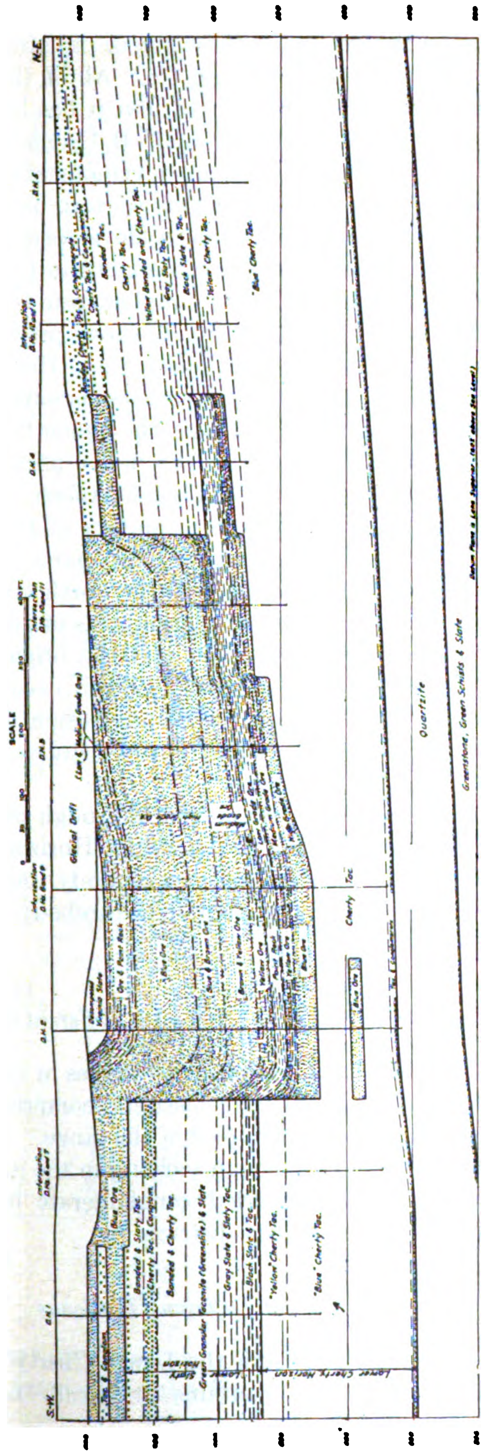

FIG. 7.—TRANSVERSE CROSS-SECTION OF A TROUGH OREBODY SHOWING ORE DERIVED FROM THE DIFFERENT HORIZONS AND TROUGH STRUCTURE OF OREBODY.

to the western part. It was recognized in drill cores only a few years ago by W. H. Crago, head of the exploration division of the Mining Engineering Department of the Oliver Iron Mining Co. All of the earlier drill cores have not been reëxamined for it as yet, but it has been correlated extensively in different parts of the Range by F. B. Cronk and the writer. The cross-section in Fig. 6 shows a maximum development of interbedded conglomerate. It is not as thick either on the western or eastern end of the range. On the eastern part it is distinctly developed in both Upper Cherty and upper part of Lower Slaty Horizons, but in the west central part of the Range only the Upper Cherty conglomerate has been recognized, and it is quite thin. This section (Fig. 6) shows practically a maximum thickness of iron-formation. The Lower Slaty Horizon is abnormally thick (260 ft.) whereas the average thickness is only about 150 ft. In the central and western part of the Range, the Upper Cherty Horizon is exceptionally thick, the slaty layers of the upper part of the Lower Slaty Horizon being replaced by cherty and banded taconite. Evidently, more muds were deposited with the iron-formation in the east central part of the district than in the central and western parts of the district. Perhaps the underlying rocks outcropping to the north are accountable for this. From Mt. Iron east to Aurora, large areas of greenstones and slates lie to the north of the iron-formation. If the original shore line of the sea in which the iron-formation was laid down occupied approximately its present position, the weathering and erosion of these rocks contributed muds and argillaceous sediments to the sea water contemporaneous with the deposition of iron.

Fig. 7 shows a cross-section across a typical trough orebody in the Virginia district. The Upper Cherty and Slaty Horizons have been eroded from the sides of the rockwalls of the orebody, but all horizons from the Virginia slate down are represented in the orebody. The typical trough structure is well shown.

Records and Subdivisions of Drill Holes in Different Districts

Fig. 8 shows records and subdivisions of drill holes in the Nashwauk, Hibbing, Virginia, McKinley and Aurora districts, comprising the territory from the west central to the eastern part of the range. These records all show the same major divisions, though varying in the minor subdivisions and dimensions. The interbedded conglomerate in the Upper Cherty Horizon is persistent in all the districts.

Ores Derived from the Different Horizons

The characteristic ore derived from the Lower Cherty Horizon is a coarse "blue" high-grade ore. It contains practically no paint rock

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seams. About 30 or 40 ft. of the top of this horizon is a brown or yellow mottled cherty taconite (originally a greenalite rock) containing some

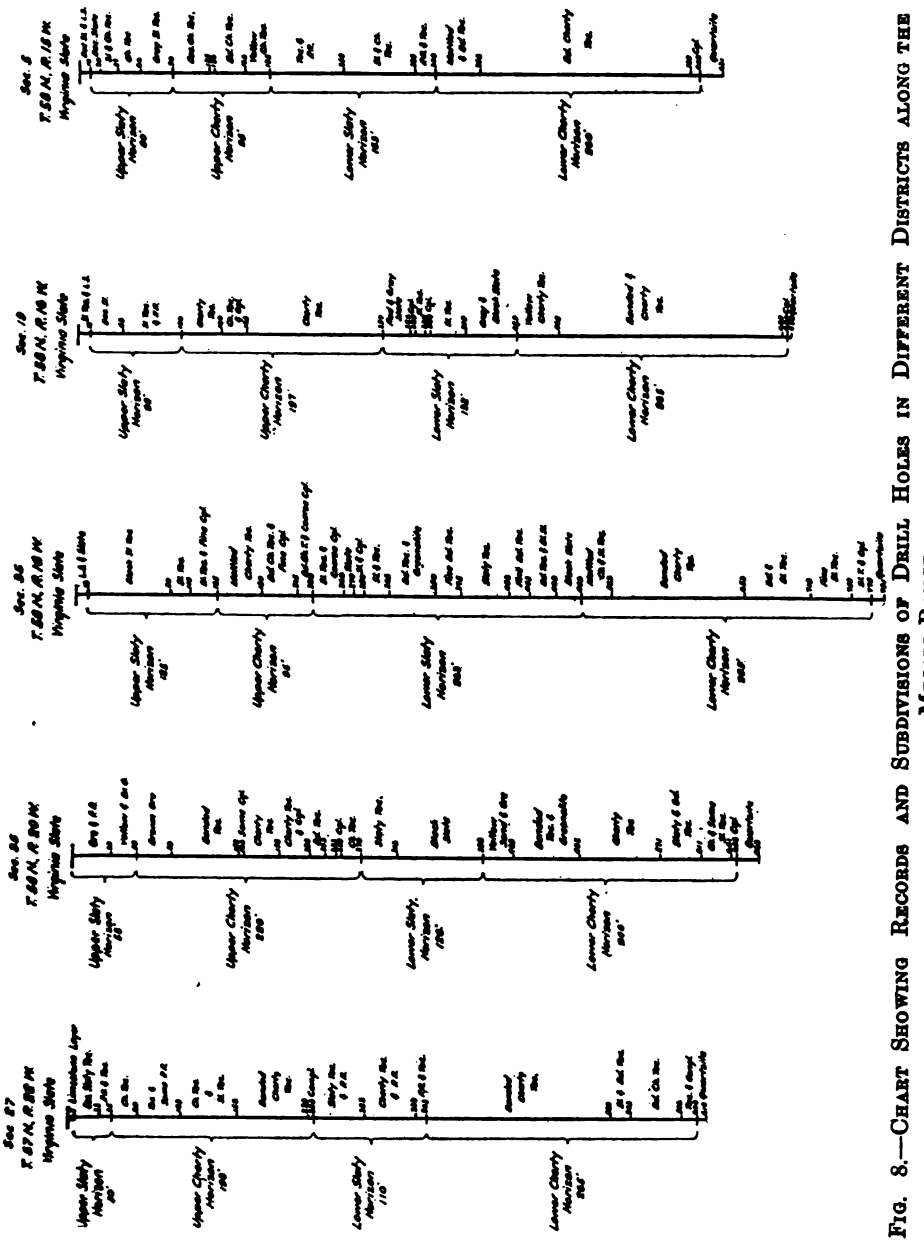

FIG. 8.—CHART SHOWING RECORDS AND SUBDIVISIONS OF DRILL HOLES IN DIFFERENT DISTRICTS ALONG THE MESABI RANGE.

slate seams in its upper measures, and this layer makes a yellow ocherous ore of medium grade. At the base of the Lower Cherty Horizon just

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above the basal conglomerate is a layer of fine slaty taconite (see Fig. 5) which makes a yellow ocherous ore. With the exception of these top and bottom layers, the Lower Cherty Horizon makes a "blue" high-grade ore. As used here, high-grade ore means ore averaging above 59 per cent. dry iron, medium-grade averaging 55 to 56 per cent. dry iron, and low-grade averaging about 50 per cent. dry iron.

The characteristic physical feature of ores from the Lower Slaty Horizon is their finely banded and slaty texture. As previously indicated, it is the black slate at the base which makes the paint rock layer so prominent in every typical orebody. This material is not a commercial ore. It is highly aluminous and contains 20 per cent. or more of moisture. The gray slate and greenalite and slate above this black slate (Fig. 7) make a medium-grade yellow and brown ore, the yellow ore being quite aluminous. The banded-cherty and banded-slaty taconite of the top of the Lower Slaty Horizon make a very fine-grained blue and brown ore of high grade. It was this kind of ore that was so objectionable to furnace men because of excessive fines, in the early days of the Mesabi Range exploitation.

The Upper Cherty Horizon makes a high-grade coarse blue ore, in all of the large well-concentrated bodies. It is indistinguishable in texture from the blue ore of the Lower Cherty Horizon.

In some orebodies toward the south side of the formation, such as the Morton Mine in the Hibbing district, the Duncan in the Chisholm district, the Leonidas in the Eveleth district and the Schley and Hobart in the Gilbert district, the ore in this horizon is somewhat sandy and cherty, due in part to incomplete concentration and in part to secondary silica deposited by ground waters.

The Upper Slaty Horizon generally makes a low-grade non-merchantable ore. The one known exception to this is shown in Fig. 7, in which orebody most of this ore probably will be merchantable. This orebody, however, is one of the most highly concentrated on the Range. The material from this Upper Slaty member in most orebodies is a soft plastic paint rock with decomposed chert and greenalite layers. It resembles very much the so-called "Intermediate" paint rock layer.

From the cross-section, Fig. 6, it is evident that all orebodies will not contain all of these horizons or layers. The unit of land subdivision is a 40-acre tract and many mines occupy one or a part of one such tract. If a mine is located near the quartzite outcrop, most of the upper horizons will be eroded away, and as the mine location approaches the Virginia slate outcrop more of the upper layers will be found in the orebody.

Relation of Orebodies to Folding and Fracturing of the Iron-Formation

In the *Engineering and Mining Journal* articles above referred to, the author stated that the data then at hand indicated that the orebodies

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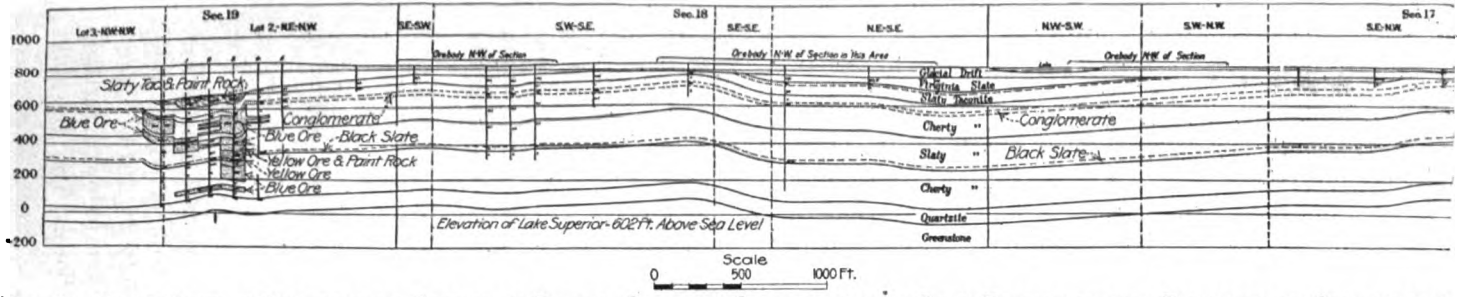

FIG. 9.—CROSS-SECTION PARALLEL TO STRIKE OF RANGE, SHOWING SUBDIVISIONS OF IRON FORMATION AND RELATION OF OREBODIES TO GENERAL STRUCTURE.

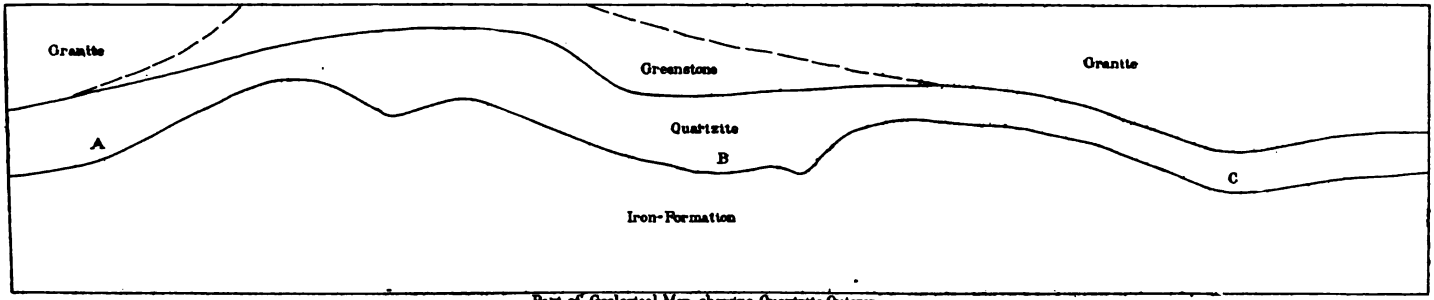

FIG. 10.—PART OF GEOLOGIC MAP AND CROSS-SECTION SHOWING RELATION OF OREBODIES TO GENERAL STRUCTURE OF ENTIRE IRON FORMATION IN HIBBING-CHISHOLM DISTRICT.

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have formed in places where gentle folding and warping of the iron-formation had fractured it considerably, allowing easy access and circulation of ground-waters. Evidence along this line has been assembled since the publication of those articles and in every case where the exploration data are complete enough it has been found that the orebodies occur where the whole formation has been warped. In the eastern part of the district (Virginia and eastward) the orebodies are on the crests of gentle anticlines. Fig. 9 shows a cross-section parallel to the strike of the formation in T. 58 N., R. 16 W., location of which is shown on the map in Fig. 11. This section is taken well to the south side of the formation and only one orebody reaches it. But the locations of other orebodies northwest of it are shown on the cross-section, and in each case where the exploration data are complete enough to show it, the orebody is located on the axis of an anticlinal flexure or combined anticline and syncline.

In the central part of the Range, great broad flexures rather than merely localized ones seem to have determined the locations of the orebodies. The formation was very generally cracked up and the broad structural basins directed the flow of underground waters. Fig. 10 shows a structural cross-section taken midway between Virginia slate and quartzite outcrops approximately parallel to the strike, through the Hibbing-Chisholm districts. A part of the quartzite outcrop to the northwest is also shown. Three prominent anticlines, *A*, *B* and *C*, with two intervening synclines, are shown. The cross-section is taken about a mile south of the quartzite outcrop. It has been published previously in the *Engineering and Mining Journal*, Aug. 7, 1915. It shows that the orebodies occur quite continuously over both crests of anticlines and troughs of synclines, indicating a very general fracturing of the formation, vigorous circulation of ground-water and consequent complete alteration and concentration of iron-formation. These major flexures can be determined only by such correlation and drawings as are shown in the two cross-sections, Figs. 9 and 10, but the minor flexures within the larger ones can often be observed in the field.

Special Features

Special features of the Mesabi Range may be of interest and deserving of inclusion in this paper.

Faults

The two principal faults known to date on the Mesabi Range are those known as the Biwabik and Alpena faults, both of which were described in the *Engineering and Mining Journal*, July 24, 1915. Mention is made of them here only because they have been followed further since that time. Fig. 11 shows the location of both. The Biwabik

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fault has been traced to the NW of SE, Sec. 5, 58–15, where it practically disappears. It is a hinge-type gravity fault, the south side of which has been depressed. The greatest throw is at its west end at the Biwabik Mine, Lot 4, Sec. 2 and Lot 1, Sec. 3, Tp. 58 N., R. 16 W., where the vertical displacement exceeds 200 ft. The underlying greenstone is faulted up against the ore, though the faulting probably occurred prior to the formation of the ore. Fig. 12 is a cross-section of the Alpena ore-body north of Virginia, showing the largest fault known on the Range. The location of the cross-section is shown on Fig. 11. As indicated, the strike of the fault is approximately north and south. It is a faulted thrust-fold, the probable development of which is shown by Fig. 13.

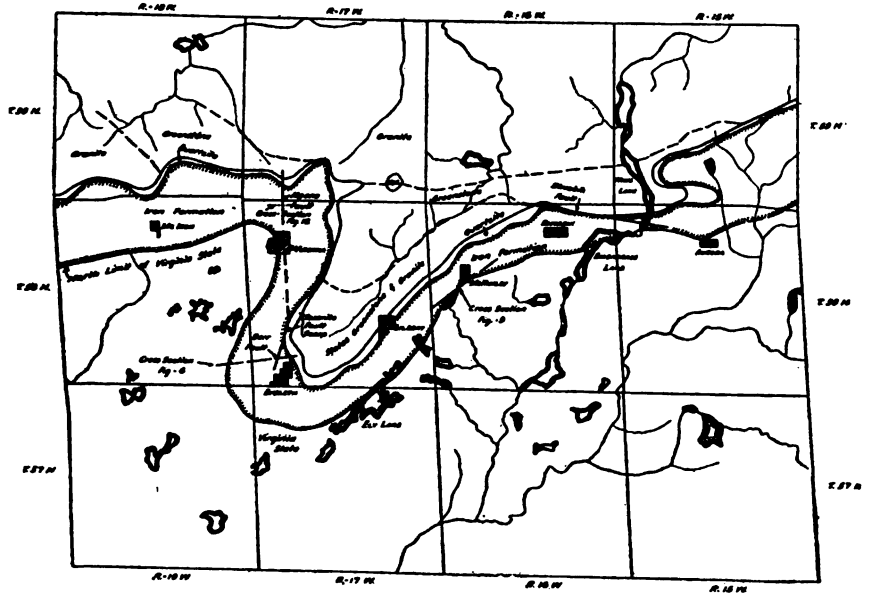

FIG. 11.—PLAT SHOWING AREA OF EAST CENTRAL MESABI RANGE.

The genesis of the fault is directly connected with the gentle uplift or crustal warping which produced the large Z-shaped bend in the Range known locally as the Virginia "horn." This was discussed also in the *Engineering and Mining Journal*, July 24, 1915. It is repeated here because since that series, a fault (shown on the east end of cross-section in Fig. 6) has been discovered by correlation of drill holes. The location of this fault, which may be called the Dorr fault from the property on which it is located, is shown on the map, Fig. 11, and is of the same type (reverse or thrust) as the Alpena fault. It is almost directly south of the Alpena. Between the two and about ¾ mile north of the Dorr fault is a taconite bluff, the east side of which is a very steep wall, undoubtedly a fault scarp. It is of the same type as the Dorr and Alpena faults.

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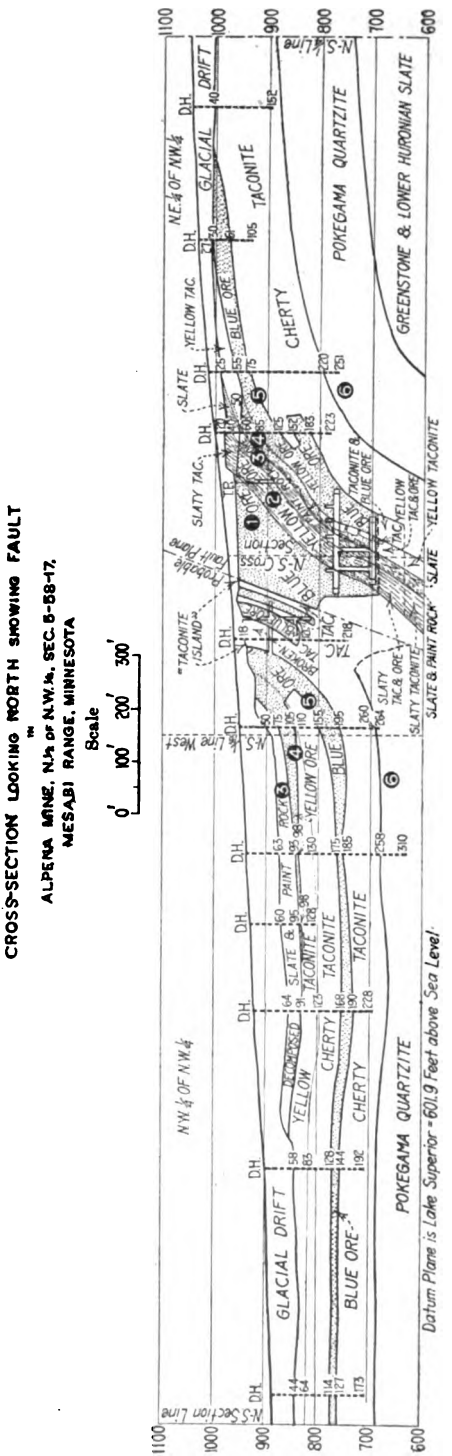

FIG. 12.—SHOWING ALPENA THRUST-FAULT.

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Although we have as yet no complete exploration data, it appears that the Alpena and Dorr faults and the intervening escarpment are one continuous fault produced by the crustal movements which caused the Virginia "horn." This probable connection is indicated on Fig. 11. However, it is possible that the Alpena fault is entirely separate from the Dorr fault.

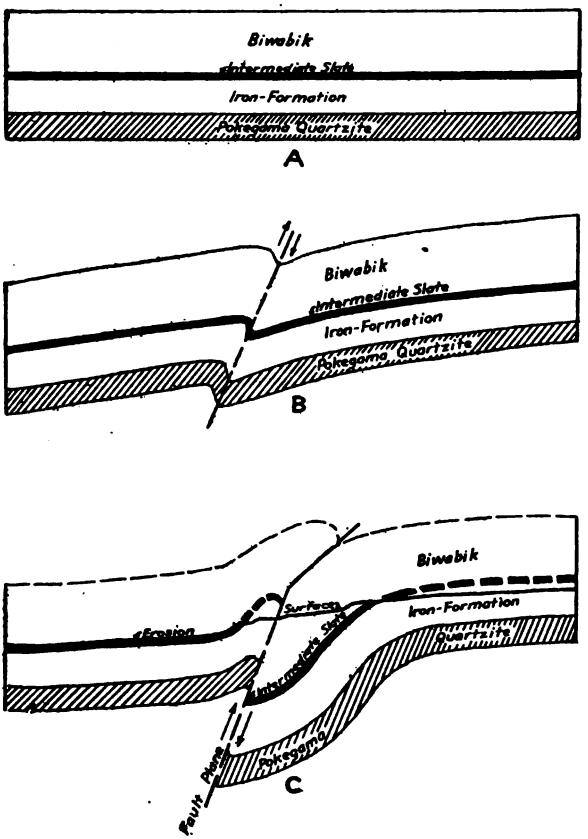

FIG. 13.—SHOWING PROBABLE DEVELOPMENT OF THE ALPENA THRUST-FAULT.

Thickness and Dip of the Iron Formation

The average thickness and dip of the whole iron-formation in different parts of the Range may be of interest. So far as they have been determined from the present very extensive explorations, they are shown in the accompanying table, tabulated by Ranges.

It will be noted that west of the Alpena fault the dips are quite low and uniform, while east of it they are higher and rather irregular. From this evidence and that of the Alpena-Dorr fault, it appears that the disturbance caused probably by the intrusion of the great mass of Duluth

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	Average Dip, Degrees	Thickness, Feet	
T. 56 N.—R. 24 W.	6	520	
T. 56 N.—R. 23 W.	3 to 8	475	(approx.)
T. 57 N.—R. 22 W.	5	615	
T. 58 N.—R. 21 W. T. 57 N.—R. 21 W.	5	590	
T. 58 N.—R. 20 W. T. 57 N.—R. 20 W.	4½	650	
T. 58 N.—R. 19 W.	3 to 7	660	
T. 58 N.—R. 18 W.	3	630	west of Alpena fault
T. 58 N.—R. 18 W. T. 58 N.—R. 17 W.	6 to 9	755	east of Alpena fault
T. 57 N.—R. 17 W.	10 to 15		
T. 58 N.—R. 16 W.	12	650	
T. 58 N.—R. 15 W.	25	530	(Secs. 5 and 6)

gabbro between Lake Superior and the eastern Mesabi range tilted the eastern part of the Range considerably; that the Alpena fault took up most of the crustal shortening in the Upper Huronian series due to the compression from the east; and that, because of the faulting, the sedimentary series west of it was relatively undisturbed.

The thickness is seen to be greatest in the vicinity of Eveleth and thinnest on the extreme eastern and western ends. The average of the figures given is about 620 ft.

Volumetric Shrinkage in Solid Taconite

Recent structural study has shown that the alteration which the original iron formation has suffered has produced a volumetric shrinkage even where the iron formation still retains its solid condition. However, in such solid taconite, hard layers of high-grade iron ore are interbanded with chert layers, the whole mass being firmly cemented together. Drill holes in fresh unaltered iron-formation alongside of holes in altered but still solid taconite show this fact to exist. Such a case is shown in comparing the depths of formation below the Lower Slaty Horizon in holes 1 and 2 with those in holes 4 and 5 in the orebody in Sec. 19, Fig. 9; also holes 1 and 2, Fig. 6.

This fact is a very important one to the mining engineer in the district in working out the structure of orebodies. The knowledge of it will aid him to establish more accurately the correct structure of certain ore layers. It explains the apparent lack of parallelism between different members of the iron-formation, which cross-sections often indicate unless this fact is known and applied.

Post-Algonkian Conglomerates on the Upper Huronian Series

In the *Engineering and Mining Journal*, July 17, 1915, the writer called attention to at least one conglomerate, and possibly two, on the top of the

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Biwabik iron-formation, older than the so-called Cretaceous conglomerate, beds and remnants of beds of which are found on most of the orebodies west of Eveleth. Since that time he has observed in two mines a conglomerate capping the orebody, containing large boulders which themselves were composed of the typical Cretaceous conglomerate. These conglomerates were overlain by layers of plastic shale and muds. Undoubtedly these are local lake-bed or stream-bed conglomerates and muds, intermediate in age between Cretaceous and Pleistocene.

Newly Discovered Fossil Remains in the Cretaceous (?) Shale

The conglomerate and shale found on top of many orebodies were correlated by the U. S. Geological Survey as Cretaceous from the fossil remains of bivalves and some teeth and vertebræ of the Mosasaur found in the shale. During the past year, in the Canisteo pit at Coleraine, one almost perfect fossil and part of others of Ammonites were found. A reproduction is shown in Fig. 14. As nearly as the writer and associates can correlate these specimens, they belong in the Jurassic and not in the Cretaceous. They show no such complicated sutures nor such ornamentation as is characteristic of the Cretaceous Ammonites. The Specimen *A*, Fig. 14, is about 15 in. in diameter and 3 in. thick. On Specimen *B*, which fits into the cast *C*, and *C* also, can be seen minute veins, but no evidence of complex sutures. It is possible, of course, that this Jurassic form lived over into the Cretaceous, but the numerous Saur remains the writer has seen could hardly have belonged to a reptile as large as the Mosasaur. They seem more fitted to a smaller creature, perhaps not over 10 ft. long, rather than to one 30 or 40 ft. long. May they not belong to a Jurassic saur, one less fully developed than the Cretaceous Mosasaur? Is it not possible, or even probable, that the conglomerates and shales on the Mesabi Range which have been called Cretaceous really are of Jurassic age and formerly were connected with the extensively developed Jurassic shales of Northwestern Minnesota? Northern Minnesota is such a well-developed peneplain that it is difficult to imagine a Jurassic ocean covering any part of it without reaching at least the foothills of the Giants or Mesabi Range.

Virginia Slate, Iron-Formation Contact

Because of possible value in connection with a revision of the correlation of the Huronian series in the Lake Superior district, the writer wishes to append to this paper the following record of observations of the relation of the Virginia slate to the underlying iron-formation, particularly because it is not in accord with statements as to this relation made in *Monograph* 52 of the *U. S. Geological Survey*. The latter publication states that at the top of the iron-formation and the base of the Virginia

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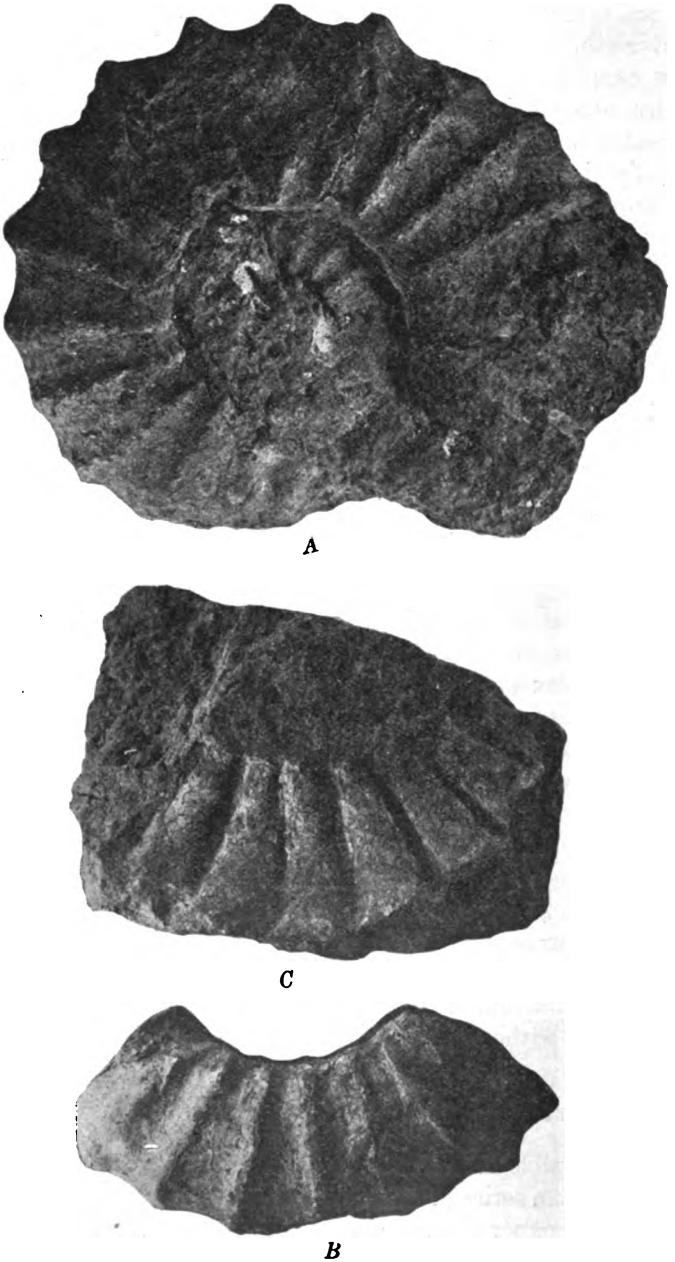

FIG. 14.—FOSSILS FOUND AT COLERAINE.

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slate, there is a horizon perhaps a few hundred feet thick which is one of gradation, in which layers of iron-formation and slate alternate, and that "the layers of slate are found well down in the iron-bearing formation, and layers of the iron-bearing formation are found well up in the slate" (page 174, *Monograph* 52, *U S. Geological Survey*).

An examination of cores from a great number of holes which penetrated through the Virginia slate into the underlying iron-formation has failed to substantiate this statement. Cores from two drill holes which penetrated between 500 and 600 ft. of Virginia slate and the entire underlying iron-formation to quartzite, and from scores of other holes, failed to show the presence of a single layer of greenalite in the true Virginia slate. Several thin layers of a carbonate rock (probably calcium carbonate) and a few crystals of iron carbonate were observed. In the iron-formation proper a very few bands of a carbonate rock were discovered. As shown on Fig. 5 of this paper, the upper horizon of the iron-formation is a slaty horizon in which layers of dark slate and greenalite alternate. At the top of this horizon and separating it from the true Virginia slate (which is a dense dark gray or black slate), is a layer several feet thick of calcium carbonate, amorphous or very finely crystallized. This layer is mentioned on page 171, *Monograph* 52, *U. S. Geological Survey*. Wherever drill holes have penetrated through the Virginia slate all along the Range, this carbonate has been found immediately beneath it. In a few holes, cores of which were examined very carefully, a small amount of conglomeratic material was found in the upper part of this carbonate layer.

Although the average thickness of the iron-formation in adjacent areas is quite uniform, as shown in the table of average dips and thicknesses, there are marked irregularities within short distances. Differences in total thickness of iron-formation of 20 to 50 ft. in drill holes ¼ mile apart are common. Two holes, 1¼ miles apart, show a difference in total thickness of 121 ft.; two holes 2½ miles apart show a difference of 184 ft. Not enough close subdivision and correlation work has yet been done to determine whether such differences are due to initial deposition or erosion from the top of the iron-formation. There is so much interbedded fine conglomerate in the upper horizons of the iron-formation that we know definitely that this part of the formation at least was deposited in very shallow water. It is not at all improbable, therefore, that prior to the deposition of the Virginia slate, the iron-formation may have been raised above water, and its upper surface somewhat eroded. The variation in thickness of the Upper Slaty Horizon gives support to this idea. No marked unconformity between the two formations can be established, however. They are conformable stratigraphically, as far as now known, but the significant fact of the absolute lack of any known greenalite (so far as revealed by many years of observation of thousands of feet

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of drill cores by the director of explorations of the Oliver Iron Mining Co.) in the Virginia slate and its marked prevalence immediately beneath the Virginia slate seems to call for such a pronounced change in conditions of deposition as to demand some time interval between the two. The universal prevalence of the calcium carbonate layer with some conglomerate between the two gives further support to this idea of a time interval.

These facts, minor though they may be, are presented here in the hope that they may be of some value to the geologists who are engaged in the revision of the correlation of the Huronian Series of the Lake Superior District.

Discussion

J. F. Wolff, Duluth, Minn. (communication to the Secretary*).—In raising the question ("Newly Discovered Fossil Remains in the Cretaceous Shale," p. 163) as to the possibility of the so-called Cretaceous remnants found on top of Mesabi Range orebodies really being Jurassic, the writer was acting under a wrong impression. In speaking of the matter with different geologists of note who have visited Northern Minnesota during the past year, he understood one of them to refer to Jurassic shales existing quite extensively in Northwestern Minnesota. This apparently was entirely a misunderstanding. In answer to an inquiry in regard to this matter, Prof. W. H. Twenhofel, paleontologist at the University of Wisconsin, said that the known Jurassic nearest to Northern Minnesota is in Western Dakota. From the reproductions of fossils shown on p. 164, Prof. Twenhofel tentatively correlated them as "middle-upper Cretaceous."

Carl Zapffe, Brainerd, Minn. (communication to the Secretary†).—Unless one has actually directed explorations for iron ore, it is doubtful whether the importance of Mr. Wolff's paper, the value of the information contained, and the large amount of detail work involved in what is set forth can be fully appreciated. It is indeed rare to get such a complete and masterful treatise on this subject, and Mr. Wolff is preëminently qualified to deal with it. The Oliver Mining Co. has a director of explorations of long experience, and he has ever maintained a competent staff to study in detail and correlate drilling exploration data, and the individuals have always been alert to recognize the value of and accept new suggestions and information given by others engaged in similar work. The application of these data in the construction of sections and interpretation of the Mesabi ore deposits by this company should be considered authoritative.

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* Received Jan. 15, 1917.
† Received Dec. 18, 1916.

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Mr. Wolff, a mining engineer, preaches a strong lesson—one of great interest to me as a geologist. In his opening paragraph he says (1) that during only the last 4 or 5 years has much been added to the detailed geologic knowledge of the district, and (2), that refinement in his work has become mandatory and a commercial necessity. It is noteworthy, then, that this district has now been a shipper for 25 seasons; that the Mesabi is well known to all men in the iron business; that the geology has generally been deemed too simple to afford a sufficient basis for consideration in the operation and development of a mine; that the geology has been well known for a long time (a comprehensive *U. S. Geologic Survey Monograph* on the geology of the Mesabi district was contributed by C. K. Leith in 1904, and in 1911 another monograph by Van Hise and Leith covered thoroughly the geology of the entire Lake Superior region, and in addition Leith has contributed much in special papers, and otherwise, on the geology and the ores of the Mesabi); and now, after these many years of activity, study, and publicity, the greatest and most influential operator in that district strongly indorses the fact that no matter how simple the geology is and how great the district is, geologic features, no matter how minute or inconspicuous, must be sought and taken into account and geologic principles must be applied in the work. Many have heard too often the old miner's classic remark that "the ore is where it is," but Mr. Wolff says that there is a reason for the ore being where it is but that the structural conditions governing are not readily discernible. Nothing additional to the subject can be advanced, and it is needless to comment further on the details presented other than to emphasize the remarkable regularity of the subdivisions of the iron-bearing formation throughout such great distances, the importance of this fact, and the value that could be derived from similar observations made in other iron-mining districts.

Mr. Wolff treads on less familiar ground when he discusses the Mesozoic fossils and questions the classification of the remnants of the Cretaceous formation. Even if other data were lacking, nothing that he says is a bar to correlating the fossils found as of Cretaceous age. Upper Cretaceous fossils are rather abundant in widely scattered places in the glacial drift in Minnesota, but never have I heard of Jurassic fossils in Minnesota, not even in the drift. Only a short time ago I had occasion to discuss this personally with Dr. F. W. Sardeson, formerly professor of paleontology in the University of Minnesota, who years ago identified such a collection of teeth and shells sent him from the Coleraine district. Dr. Sardeson said that he visited the place as recently as one year ago and that these fossils are undoubtedly Cretaceous fossils and clearly belonged near the Niobrara. Such a fossil as Mr. Wolff shows (his Fig. 14) Dr. Sardeson says would come from the Colorada group of the Upper Cretaceous rather than from the Jurassic, and that the coarse-

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ribbed, senile appearance which Mr. Wolff uses to argue for a Jurassic age, indicates Cretaceous, and quoted as examples other coarse-ribbed types of the Cretaceous such as Schloenbachia leonensis, Prionotropites woolgari, Mortoniceros shoshonense (Mick), and Prionotropites loevianus (White).

I do not know of any Jurassic deposits in the northwestern part of Minnesota, nor is the Jurassic considered generally to have been present. I know of nothing that would indicate that the interior Jurassic sea ever got farther east than the east flank of the Black Hills in South Dakota and the extreme southwest corner of North Dakota, which is a long way from the western end of the Mesabi Range. This interior sea was up to that time but slowly encroaching eastwardly and not until the later part of the Cretaceous period was this interior sea extensive, and there is no record that even with so extensive a sea were "extensively developed shales" laid down in northwestern Minnesota, as stated by Mr. Wolff.

Mr. Wolff concludes by presenting some facts which, as he says, may be of some value to geologists who are engaged in the revision of the correlation of the Huronian series. He is timely in his suggestion because recently a notable attempt has been made to change the correlation of the Huronian groups in the entire Lake Superior region, and there seems to be some foundation for it. So far as the Mesabi is concerned, it affected only the nomenclature of the Upper Huronian group, which was to be changed to Middle Huronian. But when it is a question of separating the Virginia slate from the Biwabik iron-bearing formation by an unconformity, if cognizance is taken of the fact that slate formations are built up under vastly different conditions, deep-sea deposits on the one hand and delta deposits on the other, and if the more recent theory of the deposition of the Mesabi iron-bearing formation is well understood, it seems that the conditions cited by Mr. Wolff need not call for a stratigraphic break to explain them.

EDWIN J. COLLINS, Duluth, Minn. (communication to the Secretary*). —This paper by Mr. Wolff is particularly valuable because it contains so completely the actual observations of the author extending over a period of many years.

As an engineer of the Oliver Iron Mining Co., Mr. Wolff has had access to all the mines of the Mesabi Range, on which that company operates about 40 properties. The Oliver company and the other operating companies on the Range are very liberal in giving each other information of mutual interest and permitting inspection of their mines.

The observations in his paper cover 110 or more operating properties and many others now inactive. The mines are located along about 80 miles of the present producing portion of the Range. As a result of his

* Received Jan. 20, 1917.

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keen observations in the mines and the study of the records of several hundred miles of drill holes, he has correlated the structural formation of all portions of the Range and shows that although the detail of the different orebodies varies, the structural features are the same.

The engineers of all Mesabi properties should work out the structure of their respective mines following the broad classification made by the author, so that ultimately there will be a uniform detailed structural record of all the orebodies of this great iron-ore district.

The information contained in Mr. Wolff's paper has been obtained by laborious observations and comparisons. It is of much economic value and anyone interested in mining on the Mesabi Range is indebted to him for publishing the information that has taken so long to acquire.

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The Geology of the Bawdwin* Mines, Burma, Asia

BY M. H. LOVEMAN, BAWDWIN, BURMA, ASIA

(New York Meeting, February, 1917)

THE orebody described below has been rediscovered and developed within the last 3 years. It has, however, been known and worked by the Chinese for hundreds of years. When assay values and size are considered, it contains what is probably one of the largest single bodies of zinc and lead sulphides yet found. The tonnages developed at other places, as at Broken Hill, are much greater but the average zinc and lead sulphide contents are considerably lower than at Bawdwin.

Bawdwin is situated in the semi-independent State of Tawng Peng, one of the numerous units which make up the Northern Shan States. These States are under British rule and are generally considered as a portion of Burma. They are governed as a separate administrative unit, however, under the Lieutenant Governor of Burma. Each State is under its own chief, or Sawbwa, who has considerable power over his own subjects.

Bawdwin is approximately in latitude N 23°6′, longitude E 97°20′, about 50 miles south of the nearest point on the Chinese border and 450 miles north of Rangoon (Fig. 1). The period at which mining was first begun at Bawdwin is unknown. The most reliable records have been obtained by deciphering local Chinese inscriptions and from these it is concluded that the work dates back to at least the beginning of the 15th century. The first undoubted reference to the mines by a European is by Symes in 1795. Crawford in 1827 estimated the output of silver from the mines at $600,000 anually. At about the middle of the 19th century the rebellion of the Chinese Mohammedans in the neighboring Chinese province of Yunnan so weakened the power of the Chinese that the incursions of hostile tribes (Kachins) made work extremely dangerous. The mines were finally abandoned by the Chinese about 1868. The closing down of the mines was probably not entirely due to outside causes but may have been influenced somewhat by the increasing difficulty of handling the water, as the workings gradually descended below the water level of the district. Several attempts were made by the Burmese kings, Mindon Min and Thebaw, to resume operations, but because of a

* Bawdwin—from Burmese: baw = silver and dwin = well or mine.

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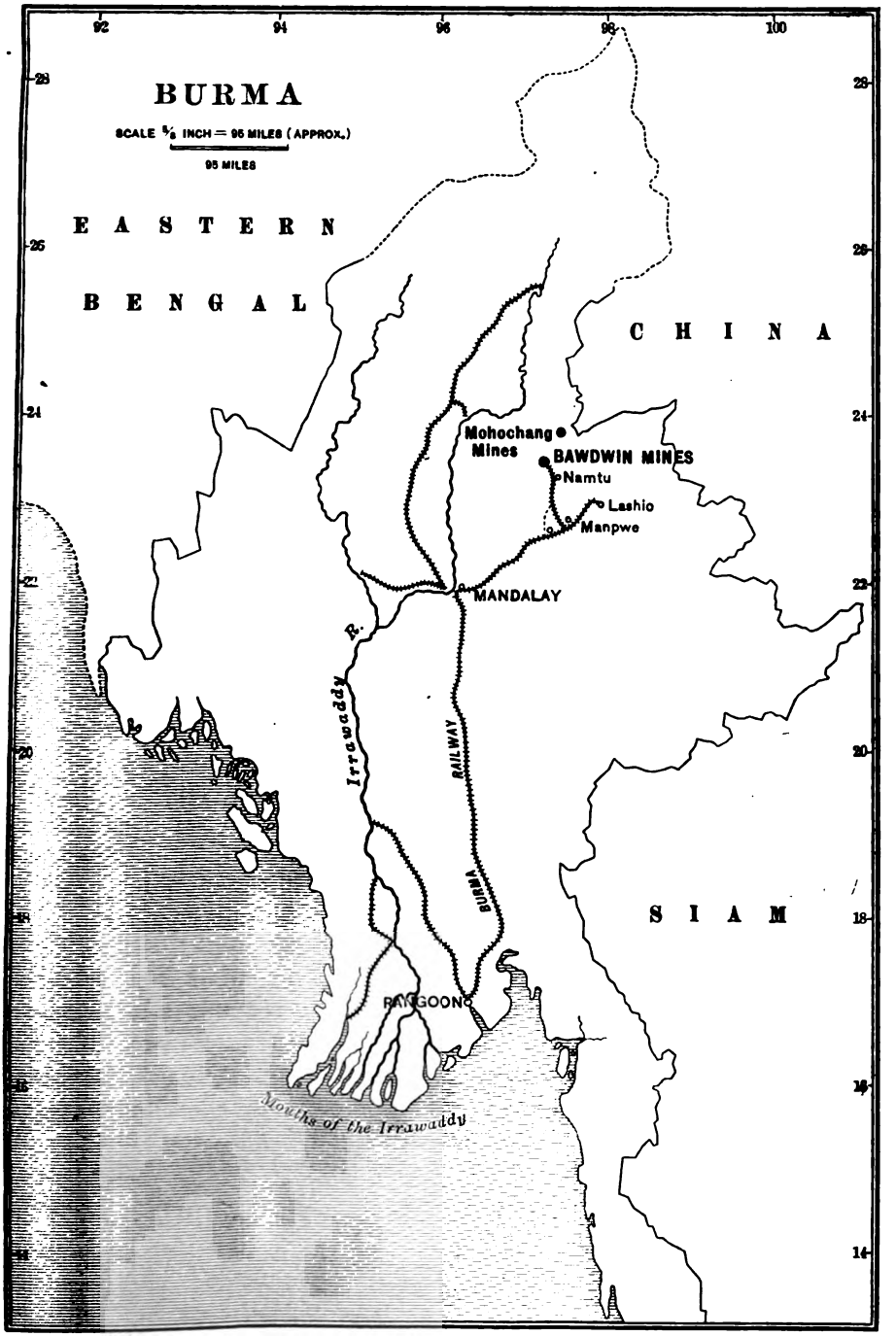

FIG. 1.—MAP OF BURMA SHOWING THE LOCATION OF BAWDWIN MINES.

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combination of diseases and lack of mining skill such efforts proved unsuccessful.

During the course of the long occupation by the Chinese, an immense amount of work was done and the amount of rock removed would certainly total well over 1,000,000 tons. The attention of Europeans was first attracted in 1901 by the extensive slag dumps which lined the valley for a considerable distance north and south of the mine workings. During the removal of the slag, prospecting work was started on the orebodies themselves. The multitude of Chinese workings proved a drawback rather than a help as they were the cause of impounding great bodies of standing water which made the reopening of the mine a dangerous undertaking. It is only within the last 3 years that this difficulty has been overcome and the orebodies have been developed.

Topography

The State of Tawng Peng is extremely rugged and covered with a fairly dense jungle over its entire area. The present drainage has now produced the maximum of relief and further erosion without elevation of the land will tend toward a reduction in the inequalities. The ruggedness is intensified by the folding and possibly to some extent by faulting. The only cleared spots are the paddy fields along some of the stream bottoms and occasional small hillside farms. The inhabitants, Shans, Palaungs and Kachins, live in small villages of from 10 or 20 people to a few hundred. There is very little commingling of the different races. The Shan settlements are in the river bottoms; the Palaungs and the Kachins generally live well up the hillsides or along the high ridges. All communication is by mule tracks and footpaths which generally run along the ridges and descend to the valleys only for short stretches. All travel is done either on foot or by mules. Small Shan ponies are occasionally used.

The seasons are divided into a rainy and a dry period. The rains extend from the end of May to the beginning of November; the dry season embraces the balance of the year. The amount of rain is not excessive. It averages approximately 65 in. The months just preceding the beginning of the rains are hot and sultry; but in January and February the temperature drops to a point where fires become almost a necessity.

General Geology

The rocks of Tawng Peng are entirely of the pre-Cambrian and Paleozoic eras. The youngest strata belong to the Carboniferous period. The western and central portions of the State are mainly composed of mica schists and of severely folded, unfossiliferous shales and quartzites. There are also some fairly extensive granite intrusions of an undetermined age. These rocks are followed unconformably by Ordovi-

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cian sediments and are themselves therefore either Cambrian or pre-Cambrian in age. This series of rocks formed an ancient land surface along the eastern shore of which Paleozoic sediments were laid down. The sequence from the unfossiliferous basement to the Ordovician series is not an invariable one. Silurian sandstones are often found directly upon the unfossiliferous series. Their presence in this relation is accounted for either by faulting or overlap. At a few points along the borders of the pre-Ordovician rocks are found small exposures of rhyolite, tuffs, breccias and flows. The most important of these rhyolite bodies is that at Bawdwin. Overlying the rhyolites at Bawdwin, and probably conformably, is a great thickness of unfossiliferous sandstones and shales. For the present their position must be placed after the pre-Ordovician shales and quartzites and before the conformably overlying fossiliferous Ordovician marls and sandstones.

Topography and Surface Geology of Bawdwin

The topography of the immediate vicinity of the mine is similar to the general description given above for the State of Tawng Peng; extremely rugged, narrow valleys with precipitous inclosing hills. The hills rise in instances to more than 2,000 ft. above the valley bottoms. One marked distinction, however, between the immediate vicinity of Bawdwin and the remainder of the surrounding country is the fact that the Bawdwin hills are absolutely devoid of the jungle covering so characteristic of all the rest of the country (Figs. 2 and 3). The hills are, however, covered by long grass, 3 to 6 ft. high, which is largely burnt off toward the end of the dry season and springs up again during the rains. The explanation of the absence of jungle is the obvious one of removal by the Chinese for fuel for smelting and domestic purposes. The demand for fuel was evidently so great that the hills were stripped clean of all trees and 50 years' desertion has been able to effect only slight reforestation. The greatest extension of the cleared area is to the northwest, evidently due to the fact that the carry from that direction to the site of smelting operations was a downhill one. The hills surrounding Bawdwin to the west, north and northeast are crowned by an elaborate network of earth fortifications. These fortifications were undoubtedly constructed by the Chinese for the protection of the mine, but at what period and against whom is not known.

The rocks at Bawdwin fall into two divisions: first, the volcanic rocks, rhyolite tuffs, breccias and flows; and, second, the overlying and underlying unfossiliferous sediments, consisting of sandstone, shale, and occasional conglomerate beds, the last only in the overlying sediments. No limestone has been found in these sediments.

On the west section of the 653-ft. level, the deepest point in the mine,

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sediments are present. These are presumably the underlying sediments, and appear to be unconformably overlain by the rhyolite tuff, although

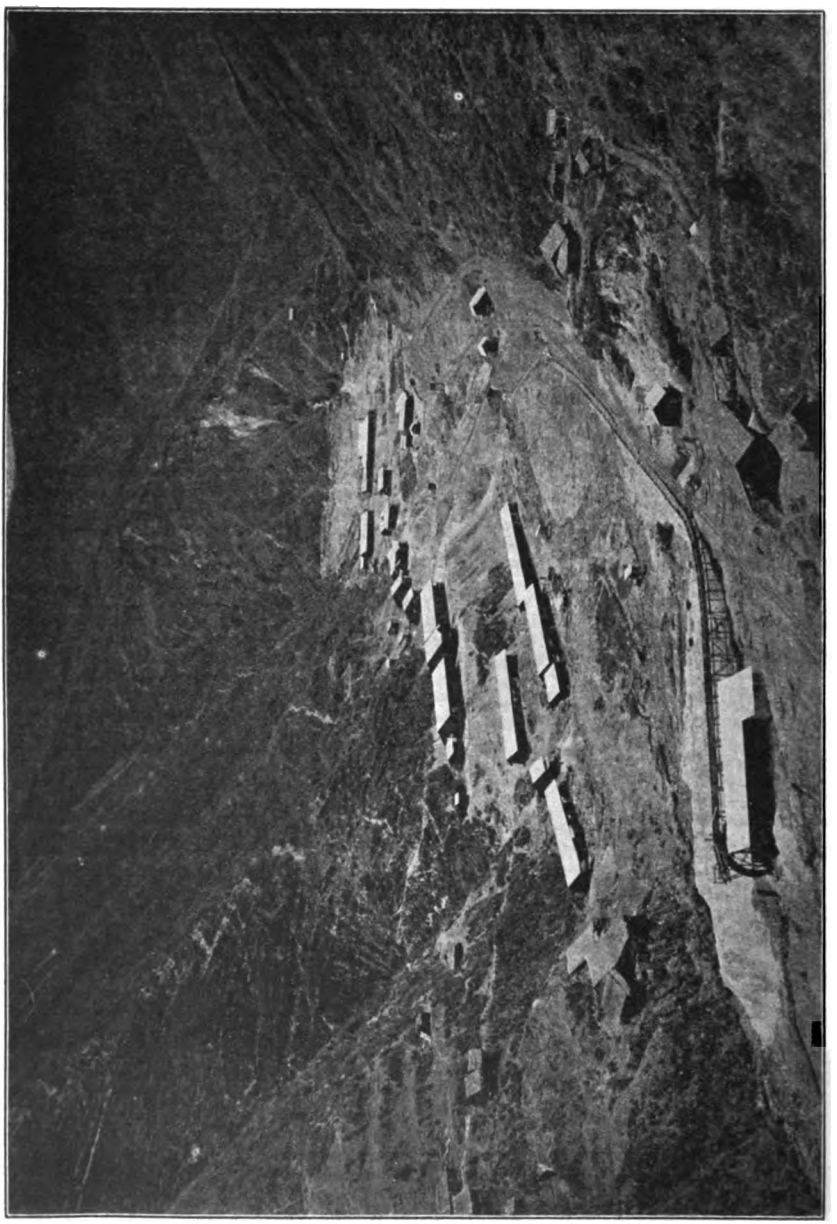

FIG. 2.—VIEW OF BAWDWIN SHOWING RUGGED CHARACTER OF THE TOPOGRAPHY.

this cannot be definitely stated, as the only point where the contact has been observed is a fault contact. The rocks are sandstones (practi-

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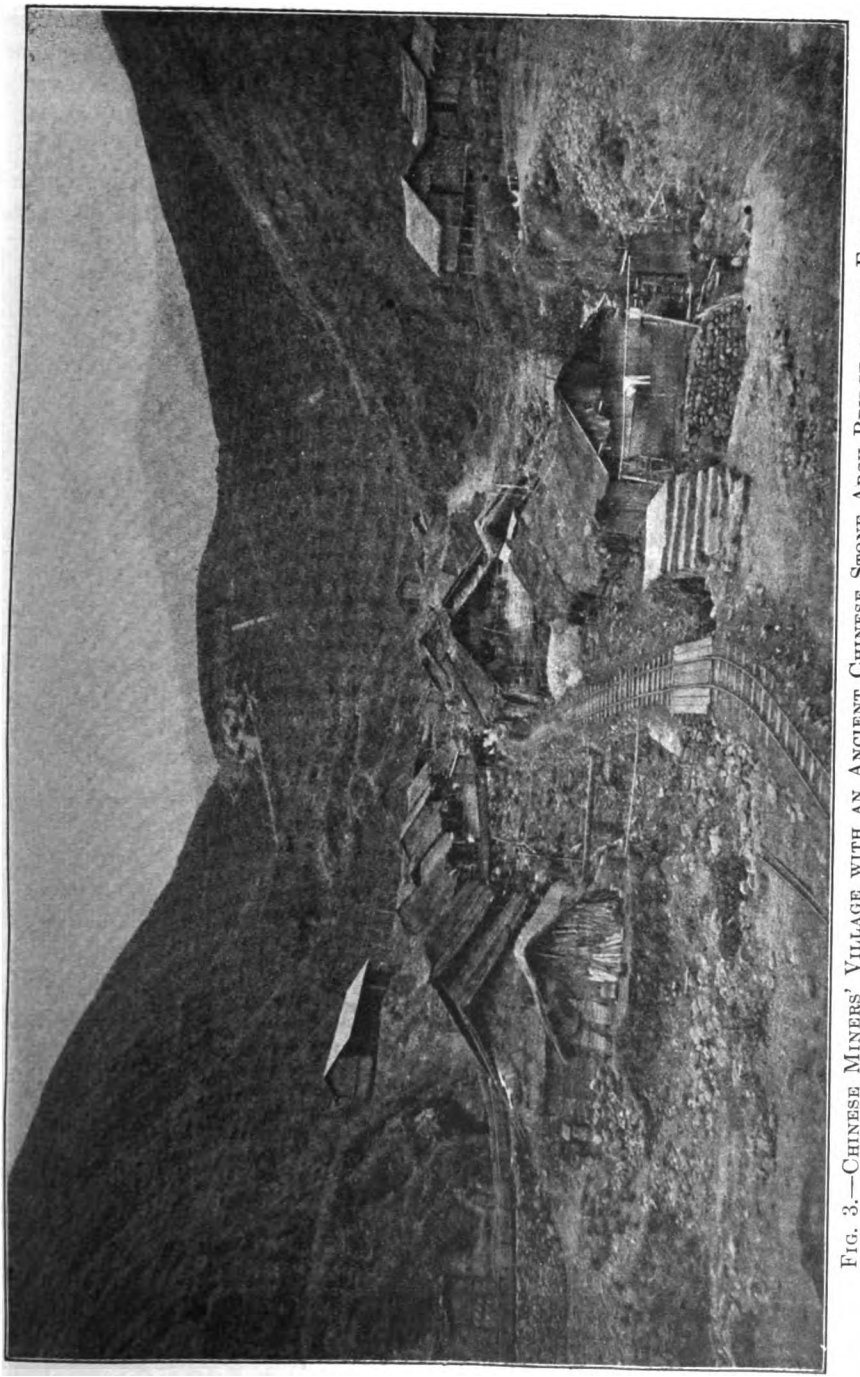

FIG. 3.—CHINESE MINERS' VILLAGE WITH AN ANCIENT CHINESE STONE ARCH BRIDGE IN THE FOREGROUND.

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cally quartzites) and shales. The sandstones are red to gray with small irregular quartz grains cemented together by sericite. The shales are light gray to purplish in color, compact with the original lamination largely obscured, although the bedding on a large scale is still observable in both the sandstone and shale.

The rhyolite in its various phases forms a rough band running northwest and southeast, but of irregular outline with numerous offshoots

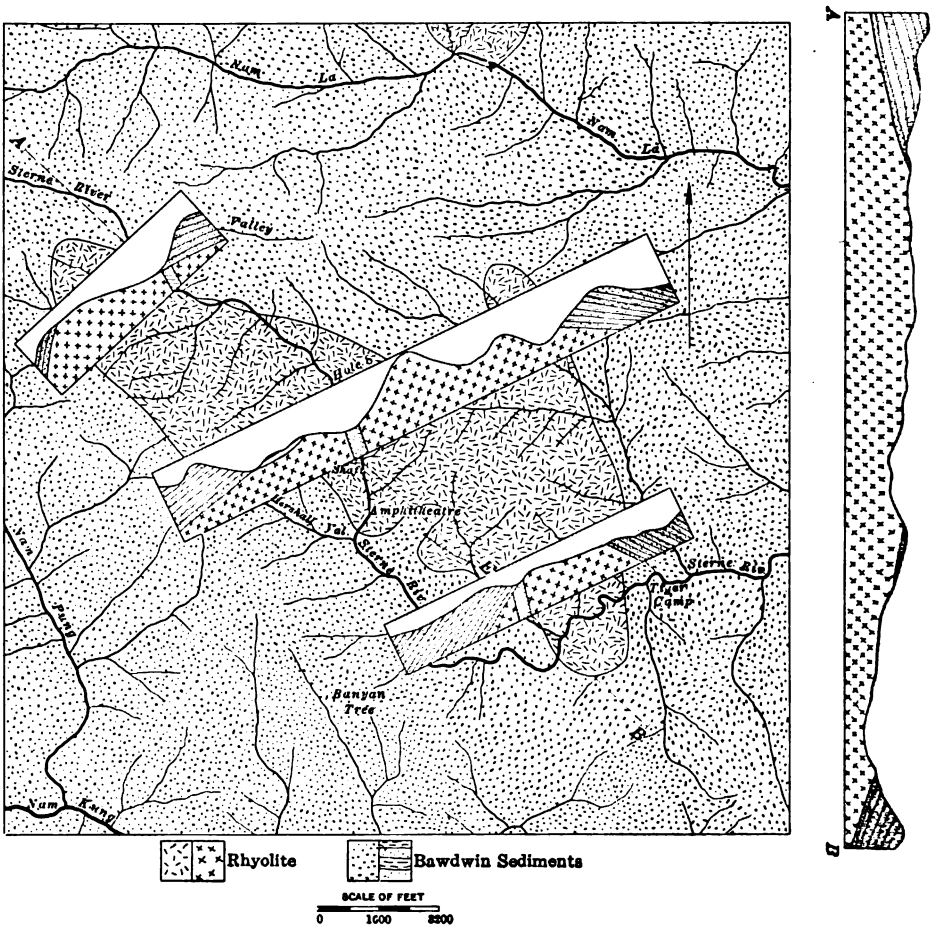

FIG. 4.—GEOLOGICAL SURFACE MAP WITH CROSS-SECTIONS. BURMA MINES, LTD.

which are sometimes directly connected with the main mass and sometimes separated in outcrop by intervening, overlying sediments. The exposure of rhyolite does not represent its actual shape or extent but simply shows that portion from which the overlying sediments have been eroded. The general direction of the rhyolite marks the axis of an anticline, from the crest and flanks of which the sediments have been removed, exposing

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the underlying eruptive. The anticline plunges both to the north and south with a resultant dome formation, having the main axis of the dome, however, running northwest-southeast (Fig. 4). This anticline is the principal one of a series of folds which parallel the rhyolite band on the west. The folding is further complicated by the great amount of faulting which accompanied it. Almost the entire exposure of rhyolite is a tuff with local areas where the included fragments attain sufficient size to justify the term rhyolite breccia for the rock at that point. Over a great portion of the area the pronouncement that the rhyolite is a tuff depends entirely on microscopic examination and even then the determination is not an absolute one. A rhyolite flow with all signs of flow structure obliterated by later changes, such as could easily have taken place in a rock of such great age as the one under discussion, would be indistinguishable from the tuff. The gradual gradation, however, from an undoubted coarse-grained tuff to the fine-grained type strengthens the belief in the tuff theory. Other observers have stated the presence of thin flows of rhyolite among the tuffs with flow structure plainly observable. The writer has up to the present been unable to confirm this.

The rhyolite away from the zone of mineralization is a fairly hard dense rock. Its color varies from light gray through pink to a slight purplish color. The groundmass is in excess as a rule but occasionally the phenocrysts of quartz and feldspar become so abundant as to comprise the major portion of the rock. The relative proportion of the quartz and feldspars vary considerably in different specimens, but as a rule the feldspars are in excess. The phenocrysts of both quartz and feldspar attain at times large dimensions, especially the feldspar, which reaches an inch or more, measured parallel to the prism. The feldspars are never found unaltered but are generally entirely changed to some secondary mineral, in most cases sericite. The change of the feldspars to sericite is characteristic of the rhyolite outside of the ore channel, while in the ore channel the change has been largely to kaolin. The feldspars are largely prismatic in form, occasionally tabular. Owing to the change to sericite, remarked above, only the outlines of the feldspars are left and these in a good many cases are largely obliterated. For the same reason the type of feldspar present cannot be definitely determined, but it is believed to be almost universally orthoclase. The feldspars occasionally alter partially to calcite with some quartz and sericite which may indicate the presence of some lime-bearing feldspars.

The quartz phenocrysts are idiomorphic to hypidiomorphic. Embayments due to the eating away of portions of the crystal by the liquid groundmass are common. The crystals are often cracked and broken and the parts separated from each other. There are occasional traces of what was possibly a ferromagnesian mineral, probably an amphibole.

The groundmass shows no glass but is entirely devitrified and ap-

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pears to consist of submicroscopic feldspars and quartz. Small amounts of sericite in the groundmass probably resulted from the alteration of the feldspars. Zircon and apatite are present in small amounts as well as occasional grains of tourmaline.

The following rock analysis is typical of the rhyolite away from the ore channel. The alkalies were obviously about 4 to 5 per cent.

SiO_2	Al_2O_3	Fe_2O_3	FeO	CaO	MgO	K_2O	Na_2O	Loss	Total
70.20	14.20	4.85	....	1.70	0.70	not determined		4.00	95.65

The variety of rhyolite just described is typical of the major portion of the exposure. There is, however, a small section north of the upper end of Hershall Valley in which the rock is a coarse breccia. Again, other portions, as that comprising the mass of Mt. Teddy, consist of a multitude of small rock fragments in a groundmass of unresolvable volcanic dust. The nature of the rock comprising the ore channel has been so entirely transformed from its original character by crushing, silicification and other alterations consequent on the mineralizing solutions that it must be discussed separately. It should be noted, however, that the southwestern portion of the rhyolite tuff forming the head of Hershall Valley to where it disappears under the sediments to the south of the Sterne River is a rock composed largely of small rock fragments, of broken crystals of quartz and feldspars and of great amounts of unresolvable volcanic dust, and even before the changes due to the faulting and mineralization was different in its character from the tuff characteristic of the rest of the rhyolite exposure.

The passage from the rhyolite tuffs to the overlying sandstone and shale is a gradual one and the exact boundary between the tuffs and sediments cannot be generally drawn. This is especially true along the southwest contact where the greatly altered rock of the mineralized zone abuts on the sediments. It appears that the basal members of the overlying sediments (coarse impure sandstones with large amounts of feldspars and rock fragments) were formed immediately after the deposition of the coarse tuffs and possibly before their consolidation. Thus the lower beds of sediments are simply the worked-over and somewhat sorted tuffs. Above these coarse impure sandstones, is a great thickness of well-bedded red and white sandstones, purplish and micaceous shales and occasional conglomerate bands. No fossils have been found in these beds. Conformably overlying these beds is a great thickness of Ordovician sandstones and marls with abundant characteristic Ordovician fossils. The geological sequence in the vicinity of the mine would thus appear to be as follows: A basement series of Cambrian or pre-Cambrian shales and quartzites followed by rhyolite tuffs and breccias. Conformably above the rhyolite is a great thickness of unfossiliferous sandstone and shale which is in turn conformably overlain by Ordovi-

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cian beds. The unconformity (as observed at other points in the Northern Shan States) between the Ordovician beds and the basal series is a very marked one, the lower beds having been severely folded and greatly eroded before the deposition of the Ordovician strata. The sequence from the rhyolite through the unfossiliferous sandstones and shales to the Ordovician beds is, however, a conformable one. It does not seem probable that the Bawdwin rocks correspond in time of formation to the unconformity between the basal beds and the Ordovician, especially as a portion of the interval was one of erosion and not of deposition. Consequently the Bawdwin rhyolites are most probably of early Ordovician or late Cambrian age while the basal beds should be assigned to early Cambrian or pre-Cambrian. The rhyolite probably lies unconformably on the basal beds.

Ore Zone

The orebodies occur in a wide zone of displacement (300 to 1,000 ft. across) in rhyolite tuff in which the faulting and crushing have been intense (Fig. 5). The zone has no well-defined boundaries but dies away gradually into hard undisturbed rock. As has been mentioned previously, the southwestern portion of the rhyolite area differs considerably in its lithological character from the remainder of the rhyolite. In this portion the tuff-like nature of the rock is much more pronounced, the constituents are considerably more numerous and variations in the type of rock are frequent. It is through this portion of the rhyolite that the zone of faulting passes, and, therefore, superposed on the original mixed character of the rock, are the great secondary changes due to the introduction of foreign materials and to the rearrangement of the minerals already present. The faulting extends through this type of tuff into the fine-grained tuff which comprises the major portion of the rhyolite area, but its effect there has been much less, due partially to the more homogeneous and compact nature of the rock and probably partially to a dying away of the severity of the faulting in that direction.

The rock of the ore channel varies from a fine-grained greatly silicified tuff consisting largely of introduced quartz to a rock largely composed of included fragments of other rocks, often dark blue or black angular fragments of shale. A type very characteristic of the neighborhood of the large orebodies is one consisting largely of a great number of large kaolinized feldspars set in a fine-grained groundmass. The alterations of the rock in the ore channel are so great that the original character is often almost entirely destroyed. The greatest single factor is the silicification which has penetrated all through the rock, replacing the groundmass, feldspars and included fragments. The feldspars are, however, generally altered to kaolin, the alteration being complete, the whole mass of the feldspars being changed. The change sometimes takes place

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to a mixture of calcite, quartz and sericite instead of to kaolin. Considerable sericite also forms in the groundmass and occasionally from included fragments. Large amounts of a light-green non-pleochroic alteration product, probably a form of chlorite, are found. At a few points in the ore channnel small portions which escaped the intense crush-

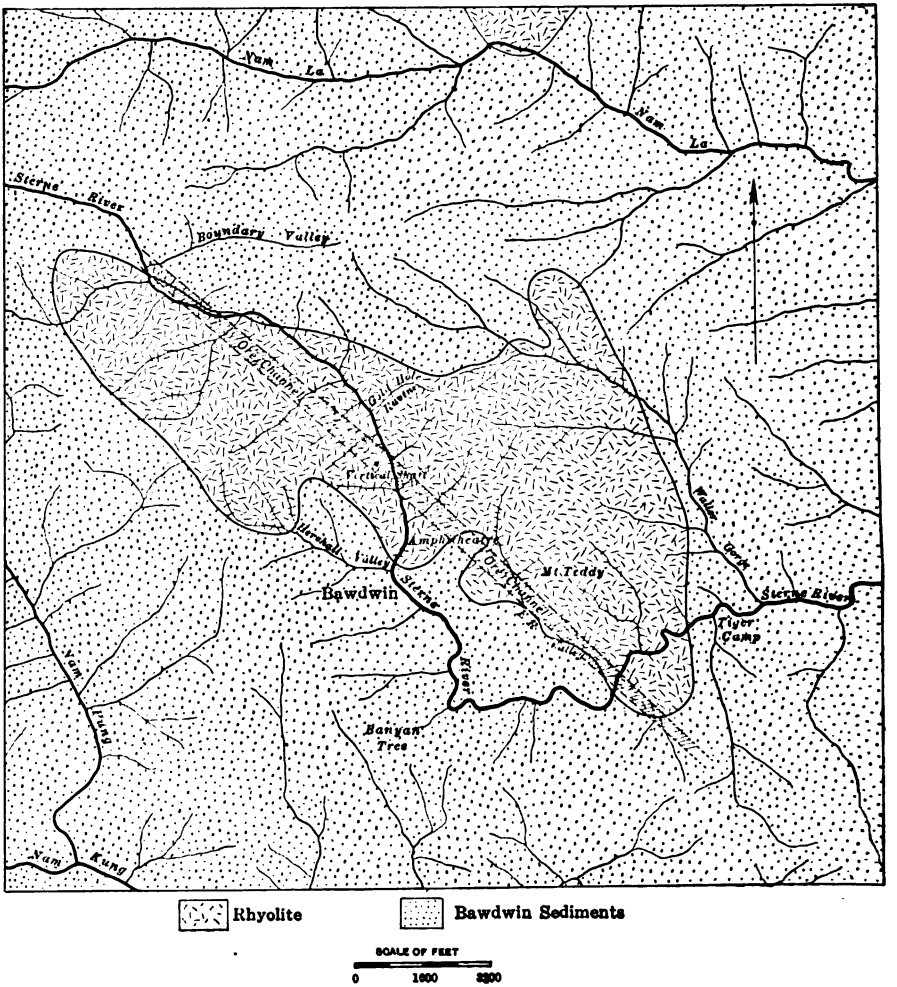

FIG. 5.—GEOLOGICAL SURFACE MAP. BURMA MINES, LTD.

ing and silicification and which retain the characteristics of the fine-grained tuff are preserved.

In a general way the following changes take place in the rock on approaching the ore channel. At a distance of 500 or 600 ft. the rock is a normal rhyolite containing considerable sericite and calcite due to ordinary secondary changes. On approaching the ore channel, the silica,

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increases and the alumina decreases. The amount of sericite in the groundmass increases and microscopic cubes of pyrite become numerous. Still closer to the ore channel, chlorite becomes abundant and the alteration of the feldspars takes place partly to kaolin instead of to sericite. Finally, in the ore channel itself, the rock is a mixture of quartz, sericite, chlorite and kaolin with some calcite.

The following are rock analyses from various points in the ore channel. Owing to the extremely variable character of the rock itself, a tuff with included fragments, it is unsafe to draw any conclusions by a comparison of these analyses with the one given as typical of the unaltered tuff.

	SiO_2	Al_2O_3	Fe_2O_3	FeO	CaO	MgO	(K_2O Na_2O)	Loss
(1)	77.20	8.28	0.71	1.09	1.07	0.48	2.55	
(2)	71.60	8.74	2.67	4.35	1.30	1.19	2.69	
(3)	80.40	10.25	0.46	1.28	1.40	0.37	2.91	
(4)	74.65	9.05	5.35	.....	3.05	1.70	determined	4.48
(5)	83.65	8.00	2.85	.....	1.45	0.45	determined	0.94

(1) Close to footwall of Chinaman Lode.
(2) Several hundred feet from hanging wall of Chinaman Lode.
(3) In Chinaman Lode—a block of country rock with large kaolinized feldspars and included in the lode.
(4) 300-ft. level—south of Shan Copper Lode.
(5) Close to surface on hanging-wall side of lode.

Lead, zinc and sulphur bring the totals of the above analyses up to approximately 100 per cent.

Orebodies

The metals of commercial importance found at Bawdwin are zinc as sphalerite (ZnS), lead as galena (PbS), copper as chalcopyrite ($CuFeS_2$) and silver whose manner of occurrence is not definitely known but which is probably present largely as argentite in submicroscopic mixture with the galena.

The ore occurs as solid masses of sulphides with practically no admixture of gangue; as veins; as interlacing seams forming small stockworks; and as an impregnation of the country rock. The gangue when present is quartz, calcite or country rock (rhyolite tuff). The principal orebody, known as the Chinaman Lode, is at present developed for about 1,200 ft. in length and varies in width from a few feet to over 100 ft., maintaining on some levels an average width of 50 ft. for over 1,000 ft. along the strike (Fig. 6). It is primarily a zinc-lead-silver orebody with small amounts of copper along the edges. Some of the diverging seams and faulted portions of the Chinaman Lode are, however, variable in their metal content, showing rapid alternations from zinc-lead to copper

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ore and *vice versa*. These changes take place both horizontally and vertically, sometimes as a gradual transition, sometimes marked by a fault plane. The largest single body of copper occurs on the 300-ft. level,

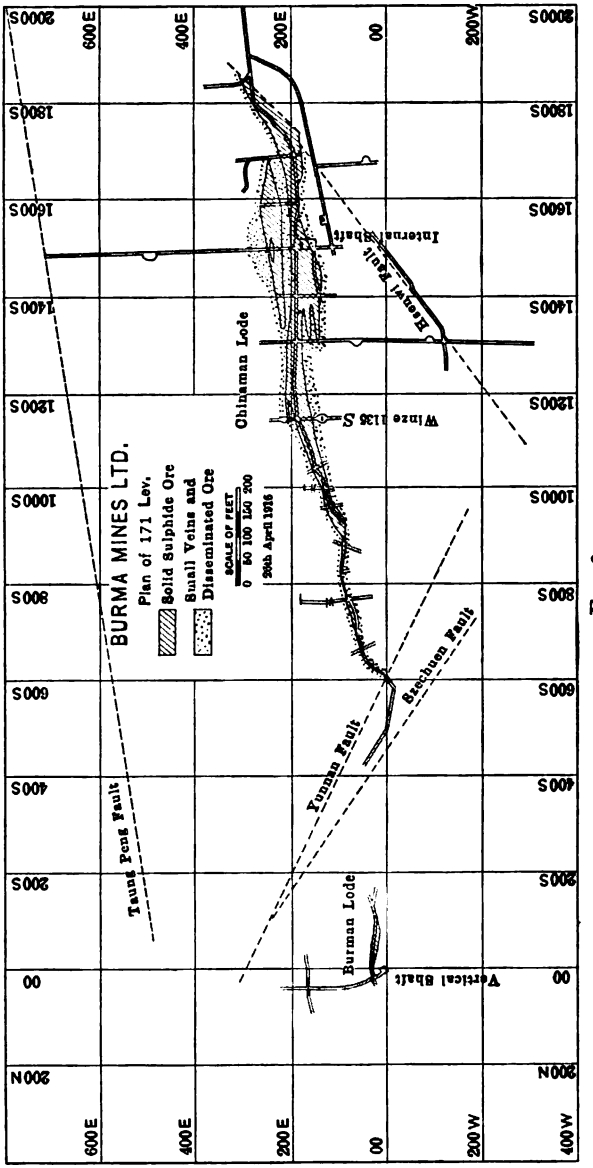

FIG. 6.

north of the Chinaman Lode. It is about 130 ft. long with an average width of 25 ft. running 14 per cent. copper, 7 oz. silver, and with only small percentages of zinc and lead on this level. It grades, however,

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about 75 ft. above into zinc-lead ore and does the same at 30 ft. below. This lode is probably the faulted northern extension of the Chinaman.

The orebodies thus far opened up occur entirely in the altered silicified rhyolite tuff. The mineralization extends to a slight degree into the overlying sediments. It is possible, however, that this mineralization in the overlying sediments belongs to a later period than that in the tuff and is more of the nature of a slight retaking into solution of the minerals in the tuff and redeposition in the sediments by ground-water circulation. The nature of the orebodies in the underlying sediments is as yet unknown. It is believed that they will probably be more restricted as to size and be more in the nature of fault deposits, with replacement of the country rock assuming a subordinate rôle, the opposite of the conditions in the tuff.

A cross-section through the Chinaman Lode shows a central core of solid zinc-lead ore, with the zinc generally but not invariably in excess of the lead. On both sides of this central core are alternating bands of solid ore and heavily mineralized tuff. These bands parallel the main body in strike and dip but are not persistent themselves, coalescing and pinching out and in reality forming a sort of stockwork. The bands are generally high in lead and comparatively low in zinc. A slight percentage of copper is generally found on their edges. From both sides of these bands the mineralization extends far out into the tuff, gradually merging into barren rock. Occasional seams and patches of ore are found at considerable distances. There is no sharp boundary between mineralized and unmineralized country rock as a general thing, although this condition is approximated in a few places by fault planes. Pyrite is found to a limited extent in the main orebody, but attains a much greater abundance along the edges where it is disseminated all through the rock as a multitude of very small (microscopic) cubes. The central core of solid ore in the Chinaman Lode attains at points a thickness of more than 80 ft., and on some horizons maintains an average width of 55 ft. for over 800 ft. in length and, as stated above, an average of 50 ft. for 200 ft. additional. In this core the gangue is generally very finely disseminated quartz grains. The extreme richness in metal content of the orebody is best shown by the fact that a block roughly 800 ft. long by 600 ft. deep by 30 ft. wide contains about 1,750,000 long tons with an average value of approximately: Ag, 30 oz.; Pb, 31 per cent.; and Zn, 29 per cent. A theoretical block of the same size of solid galena and sphalerite with equal amounts of Pb and Zn would contain approximately 2,300,000 long tons. Thus the block in the mine is over 75 per cent. solid lead and zinc sulphides. The accompanying cross-section (Fig. 7) through the orebody shows that this solid sulphide core has a high dip to the west in the upper levels and turns gradually over to a high dip to the east in the lower levels. A gradual thinning of the orebody takes place on approaching the underlying sediments. This is probably due to the greater effect of the fault-

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ing on a heterogeneous rock like the tuff than on the sandstone and shale. This wider zone of shattered rock in the overlying tuff and, in addition, the much more easily replaceable character of the rock, would result in a considerably wider area of mineralization.

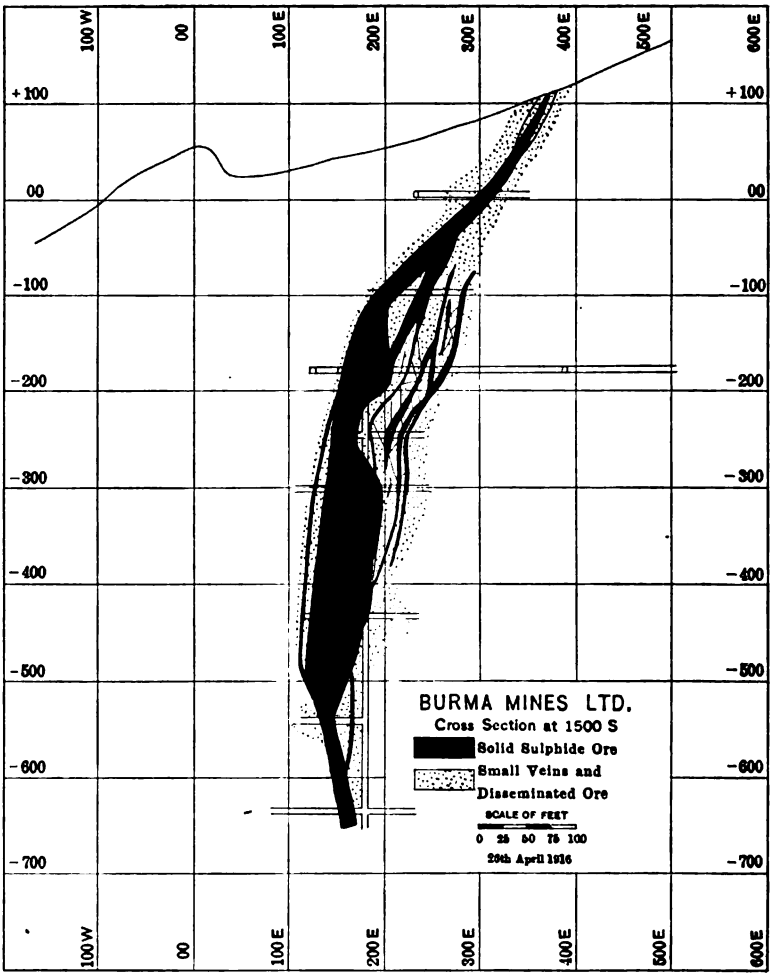

FIG. 7.

The original condition of the upper portion of the orebody is largely concealed by the work of the Chinese, but there appears to be a decided thinning of the orebody on approaching the surface. The outcrop, in fact, when considering the size of the orebody below, is scant, but this is probably partially accounted for by the oxidizing and subsequent leaching of the surface ores. At several points sphalerite and galena outcrop on the surface, but as a rule the surface ores have been largely

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leached and the outcrops are marked by a wide zone of soft decomposed rock colored by iron oxides, copper carbonates and, to a lesser extent, by oxidized lead ores. This gossan for considerable stretches runs 3 to 4 oz. in silver and about 5 per cent. in lead. It is quarried at one point for a siliceous flux for smelting purposes. A characteristic assay follows: Ag, 3 oz.; Pb, 5.8 per cent.; Zn, 0.7 per cent.; SiO_2, 58.6 per cent.; Fe, 17.7 per cent. The depth of the gossan is extremely variable but very rarely is more than 50 ft. At approximately this depth it is succeeded by a zone of secondary copper sulphides, principally chalcocite with some bornite. The chalcocite occurs largely as a replacement of sphalerite. The secondary copper ores cannot be considered as occurring as a well-marked zone over the whole orebody, but simply at a few favorable places, at some points stretching well up into the oxidized ores; at others, well down into the normal sulphides. This secondary copper is

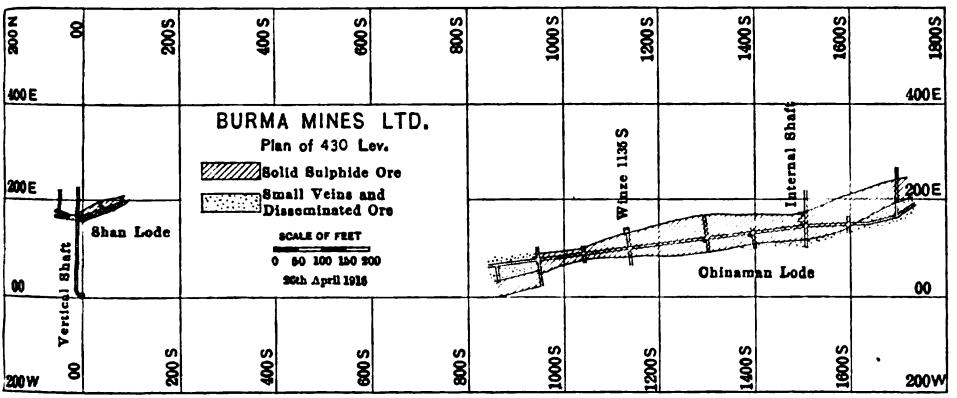

FIG. 8.

not present in large enough amounts to be commercially important. The points at which the chalcocite occurs are about 100 ft. above the present water level, but as erosion has been rapid it probably marks the water-table level at a not very distant period. The zinc and lead sulphides which, as remarked previously, at some points outcrop on the surface, extend down to the greatest depth yet reached, about 725 ft., with practically no change in their character or relationship to each other. The normal zinc-lead ore of the mine is an extremely intimate mixture of sphalerite and galena; the sphalerite, which is the older, being of a very fine granular structure, the grains varying from about 0.015 to 0.16 mm. in diameter. The galena forms around the separate grains of sphalerite, and as thin filaments running through the zinc. The ore grades off in both directions from this fine mixture, toward the zinc end to solid masses of soft, earthy sphalerite with occasional stretches of hard, dark sphalerite probably rather high in its iron content, and toward the lead end to masses of pure, coarsely cubical galena.

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The silver content rises and falls consistently with the lead and independently of the zinc, indicating that the silver is largely contained in the lead. The presence of copper even in small amounts destroys the silver-lead ratio, as the copper present in the small veins adjoining the main orebody carries a high silver content (Fig. 8). As a general statement, it can be said that 1 per cent. of lead carries 1 oz. of silver. Silver minerals have not been detected in any form but, as stated above, it seems most probable that the silver is present as argentite intimately associated with the galena.

The accompanying assays show the relations between the silver and the lead and also represent typical assays of high-lead, high-zinc, zinc-lead and lower-grade ores.

	Ag, Oz.	Pb, Per Cent.	Zn, Per Cent.
Silver-lead Ore	47.6	50.4	19.2
	54.4	50.0	24.1
High-zinc Ore	11.0	11.2	38.8
	15.5	13.6	40.7
Zinc-lead Ore	38.5	33.9	36.3
Second-grade Ore	11.8	22.5	7.0
	18.0	20.0	12.5

Faulting

The faulting along which the ore occurs was not concentrated in one fault plane, but occupies a zone reaching in places well over 1,000 ft. in width. Along this zone a multitude of faults of varying strike and dip—some normal, some reverse—were developed. The faulting is extremely complicated and of such varying strike and dip that a division into rigid systems cannot be made. The majority of the faults can be traced only for short distances and are either displaced by later faulting or die out. Roughly, however, two main fault systems can be determined, one with strikes about north and south and with high dips to either the east or west; and a second series with strikes varying from N35W to N75W and with high dips generally to the southwest. The displacement due to most of the faulting has been very slight, as is evidenced where faults have cut the orebodies. The displacement in these intersections is generally so small as to be barely noticeable. A few dominant faults, however, which can be traced for long distances, have caused severe dislocations. One such fault cuts off the Chinaman orebody on the southern end. So extended was the movement that the ore south of the fault has not yet been located. There are, in addition to the above faults, a great multitude of smaller ones of all strikes and dips but of subordinate importance. The faults as a rule are marked by varying thickness of soft gouge and rounded rock fragments. The gouge is often colored black by the minute sulphides contained in it. Faults when passing through the solid bodies of sulphides have produced smooth

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slickensided surfaces, often with a brilliant polish which may be modified by etching. This is especially true of fault planes in the high-grade galena ore.

The faults just described appear to be later than the mineralization, although it is possible that some of the faults that strike north and south along the edges of the orebody are pre-mineral and acted as bounding walls for the main ore deposition. It is also most probable that some of the small veins branching off from the main Chinaman orebody represent ore deposits along fault planes. That these smaller veins are contemporaneous with the Chinaman Lode is proved in some cases by direct observation of the points where they merge into it. In the large replacement orebody, the Chinaman, the practically complete replacement of country rock by sulphides has entirely obliterated the original character of the ground. This orebody represents, however, a replacement in a wide faulted and crushed zone rather than a replacement that has spread out into the walls from a central fault plane. Many of the faults that are later than the main orebodies contain ore due to drag and, in some cases, ore probably formed by solution from the original orebodies and by redeposition along the fault planes which have naturally served as water channels.

The plan of the 171-ft. level (Fig. 6) shows some of the more prominent faults. The large amphitheater, directly above the Chinaman Lode, is bounded to a great extent by faults, and partly owes its existence to land slips along these planes. Its precipitous sides have probably been slightly accentuated by Chinese work but the amphitheater taken as a whole is probably almost entirely due to natural causes.

Underground Waters

As is to be expected in a tropical region of fairly heavy rainfall, the ground-water level is close to the surface. The ore channel strikes diagonally across the trend of the ridges and the ground-water level was tapped by the company's drainage tunnels only a few feet below the valley bottoms (Fig. 9). There did not, however, appear to be a completely saturated zone below the water level. Diamond-drill holes at a greater depth than any of the workings occasionally go completely dry, and water pumped into them entirely disappears. This experience was met even in cases where the holes were cemented practically all the way down. The great number of old workings and the extremely shattered condition of the ground with numerous fault planes along which water circulates freely, tend to obscure the natural underground circulation. A heavy rainfall has an almost immediate effect on the amount of water in the upper levels, and a portion of the surface water probably drains down along faults to the lowest levels. Two analyses of water are given here-

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with: No. 1 from a shaft sunk in the Chinaman Lode; No. 2 from a drainage tunnel which at the time the sample was taken was about 6,500 ft. from the orebody and probably represents with fair accuracy the underground water of the region away from the influence of the orebody.

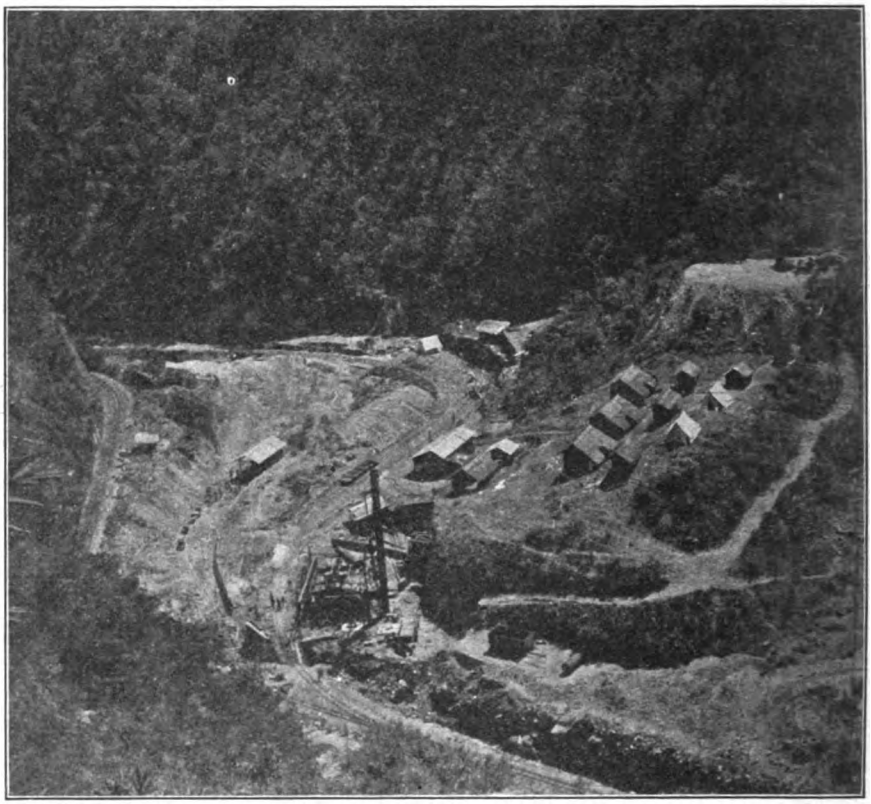

FIG. 9.—SETTLEMENT AT THE PORTAL OF THE TIGER TUNNEL. A DRAINAGE AND WORKING TUNNEL 7,000 FT. LONG.

Parts per 1,000,000

No. 1		No. 2	
SO_3	2,160	$CaCO_3$	138
CaO	227	$MgCO_3$	78
Fe_2O_3 and Al_2O_3	273	Na_2SO_4	19
SiO_2	35	Fe_2O_3 and Al_2O_3	13
MgO	227	SiO_2	136
Pb	30	Undetermined	9
ZnO	1,179		
NiO and CoO	250	Total	393
Organic and volatile matter	30		
Cl	Tr		
Undetermined	21		
Total	4,434		

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No. 1 is a strong sulphate water with an extremely high content of zinc. The large amount of nickel and cobalt in solution is remarkable inasmuch as the sulphides of these metals have not been definitely recognized in the orebody. The cobalt arsenate, erythrite, however, forms rapidly along some exposed faces. This sample was taken at a point about 400 ft. below the surface, the lowest point in the mine at the time, and it does not appear probable that the large amount of zinc is due to any extent to solution from exposed faces on the upper levels, because, if this were true, copper should also be present, as the descending water of the upper levels has a high copper content. The entire absence of copper is noteworthy and appears to indicate either that the body of underground water is virtually stagnant and its mineral content is derived from the immediate vicinity, or that the contained copper in the water above is redeposited before reaching the lower levels. A large number of the faults have impervious gouges and probably break the ground up into blocks, some saturated, some practically dry.

Minerals

The list of minerals found at Bawdwin is not extensive. The principal ones follow:

Quartz as phenocrysts in the rhyolite; as a large portion of the cryptocrystalline groundmass; as a secondary product from the alteration of other minerals; as a later introduction in veinlets; and as a product of general silicification. It appears in veins in massive form, and is hard and compact, but it may also yield well-formed pyramids in cavities. It may constitute ribbon quartz with lamellar structure. It may even be powdery. It can be detected as very fine grains, intimately associated with the sulphides.

Feldspar, probably largely orthoclase, occurs in crystals both large and small. Often only fragments of crystals appear, and both these and the well-bounded ones are generally badly decomposed. With quartz it forms the groundmass of most of the eruptive rock.

Calcite is abundant as a secondary mineral, especially as a replacement of other minerals, notably feldspar. It may be one of the gangue minerals and favors the copper ore in which relation it often composes the entire gangue.

A ferromagnesian mineral, probably an amphibole, has suffered such extreme alteration as to be almost entirely destroyed.

Barite appears in large masses as vein filling on the surface and as large weathered blocks. It has not been observed underground. Wherever seen, it is always in association with the sediments, never with the rhyolite.

Siderite is common as a gangue in some of the smaller veins but is of

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subordinate importance. Iron-stained calcite may sometimes be taken for it.

Apatite and *zircon* are fairly common as small crystals disseminated through the rock.

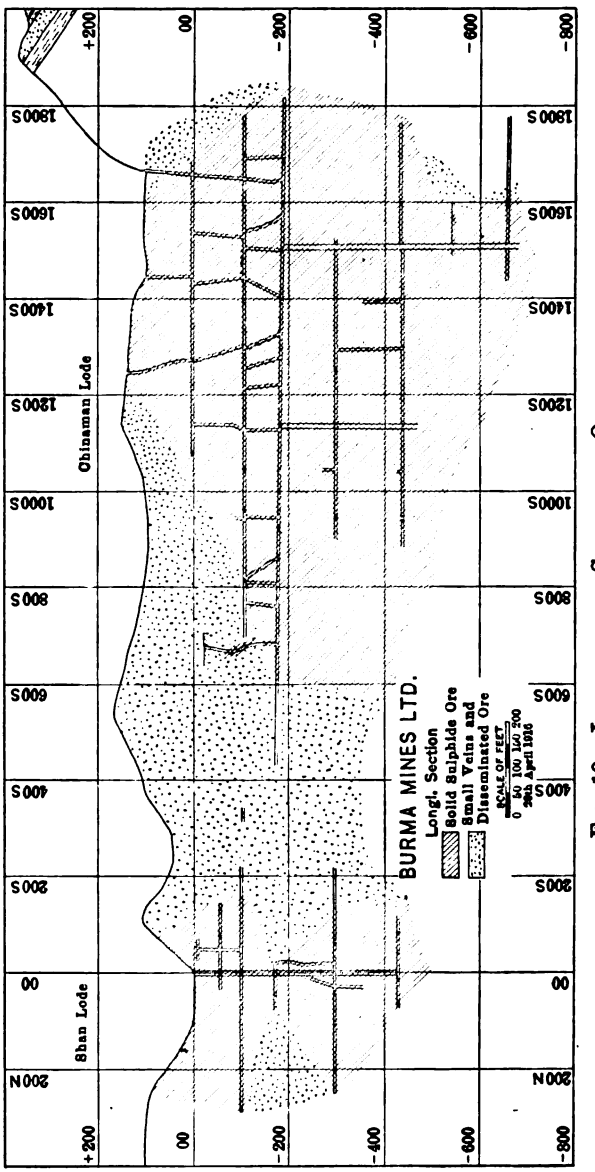

FIG. 10.—LONGITUDINAL SECTION OF OREBODY.

Tourmaline, occasional grains in tuff.

Biotite, occasional shreds, probably secondary.

Sericite is the predominant alteration product of the groundmass,

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and away from the ore zone also of the feldspars. It occurs as a mat of slender microscopic crystals.

Kaolin is the principal alteration product of the feldspars in the ore zone.

Chlorite. A light-green, non-pleochroic, alteration product is common in all types of the rock, and is probably a form of chlorite.

Galena occurs as coarse cubical galena in quartz stringers; as impregnations in the country rock; as replacements of silicates or quartz; and as a mixture in all degrees of intimacy with sphalerite and to a lesser degree with chalcopyrite.

Cerussite, as well-formed crystals in vugs in galena. It is present in considerable amounts as low as the 171-ft. level and to a lesser extent further down. It occurs also in the altered outcrops.

Anglesite appears in relations similar to those of cerussite.

Pyromorphite, as well-formed crystals in relations similar to those of the two preceding minerals.

Sphalerite occurs as large solid masses; as an impregnation of the country rock often finely disseminated; as replacements of other minerals, especially of the groundmass and feldspars of the rhyolite; as an intimate mixture with galena and occasionally with chalcopyrite, and as large masses of earthy sphalerite. The normal oxidized zinc minerals have not been observed, but goslarite forms rapidly and in large amounts along the walls of the drifts in the upper levels.

Chalcopyrite occurs in the smaller veins and along the edges of the Chinaman Lode and as the principal ore of the Shan Lode on the 300-ft. level (Fig. 10). Is often associated with galena and sphalerite but the relation is never so intimate as between the galena and sphalerite.

Malachite, azurite and native copper occur in small amounts in the oxidized zone.

Chalcocite appears as a secondary product above the water level; largely as a replacement of sphalerite, the replacement being generally not complete, so that a core of sphalerite is surrounded by chalcocite, usually of a soft earthy variety.

Bornite occurs in small amounts with the chalcocite.

Pyrite occurs in small amounts in the orebody as the last sulphide deposited. Great amounts of it lie along the edges of the orebodies, largely as a multitude of microscopic cubes disseminated all through the rock.

Hematite and *limonite* are found in the gossan above the orebodies.

Erythrite constitutes a pink incrustation along the drifts.

Nickel and cobalt sulphides are present but have not been identified.

Genesis

The orebodies at Bawdwin have been formed by the metasomatic replacement of the rhyolite tuff by sulphides deposited from hot solutions, which rose from below along an intensely crushed and sheared zone. From

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the data at hand it is not possible to make any definite assertion as to whether the mineralizing solutions are themselves a late phase of the rhyolite activity or not. Whatever evidence there is, however, appears to indicate that the rhyolite is not responsible for the metal-bearing solutions. About 25 miles northeast of Bawdwin, at Mohochaung, there are orebodies in sandstones and shales which are entirely similar to Bawdwin as regards the character of the ore itself. The beds in which they occur are probably roughly contemporaneous in age with the Bawdwin sediments. There is no rhyolite present at this point. Granite is, however, present about 3 miles to the west of Bawdwin and at roughly the same distance to the west of Mohochaung. The granite is a coarsely crystalline muscovite-biotite granite. It is a reasonable presumption to consider the granite mass as extending below the orebodies at both Mohochaung and Bawdwin, probably at a great depth below the surface, and it may be to the influence of the granite that the mineralizing solutions should be ascribed. This is, however, simply a hypothesis with no actual facts to support it.

The fault zone at Bawdwin passes through all the varying types of tuff, from the type composed largely of included fragments of other rocks to the fine-grained homogeneous rhyolite tuff. The effect of the kind of rock upon the ore deposition has been marked. The fine-grained homogeneous tuff is practically barren, the fine-grained highly silicified rock composed of fine rock fragments contains small bodies of ore along fault planes but with practically no replacement of the rock itself. With an increase in size of the fragments composing the rock there is an increase of replacement by metallic sulphides. The type composed of large numerous feldspar crystals set in a fine-grained groundmass is distinctly the most favorable rock and is, in fact, the rock in which all the large zinc-lead bodies are found.

The favorableness or unfavorableness of the various types of rocks toward ore deposition are due to several reasons.

1. The compactness or, inversely, the porosity of the rock.

2. The nature of the rock constituents as regards their ease of attack by solution and replacement.

3. Effect of the faulting on the rock—whether the rock gives way along a few prominent planes or whether the movement and consequent crushing is spread over a wide zone.

The coarse heterogeneous tuff, especially the one with large feldspars, was due to the nature of the rock itself which was not a compact one and consequently allowed an easy entrance of mineralizing solutions. The feldspars offered an easy first point of attack for the solutions and the rock itself, owing to its mixed character, suffered crushing and faulting over a wide area. All the above factors become progressively less active as one passes toward the fine-grained homogeneous type of tuff.

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The rhyolite tuff was greatly shattered and crushed along the zone of faulting, probably to a much greater extent than is now observable, as the rock has been reconsolidated by later silicification. The ascending solutions permeated all through the crushed mass, and along the zone of greatest shattering practically entirely replaced the original rock with sulphides. The feldspars which were probably the first point of attack were either replaced by sulphides or altered to sericite or kaolin. The alteration to kaolin may, however, be due to later descending solutions carrying sulphuric acid, although somewhat mitigating against this theory is the fact that the kaolin alteration persists several hundred feet below the water level. After the feldspars, the fine-grained groundmass was attacked, and finally the quartz. Deposition also took place in a subordinate degree in open spaces, as is evidenced by banding and rough comb structure and by drusy and botryoidal forms of sphalerite. Vugs and openings along fault planes are present in all parts of the mine, being, however, considerably more numerous in the upper levels where they are probably largely of recent origin. In the central core of solid ore none of the original country rock is left; where any gangue exists it is a very fine-grained quartz probably introduced with the sulphides. The sulphides were deposited in successive stages which, however, probably overlapped. The sphalerite is the oldest, followed by chalcopyrite and galena and finally by pyrite, the iron sulphide being to a great extent the last, although it was probably introduced in small amounts all through the period of the ore deposition. The origin of the chalcopyrite was contemporaneous with the introduction of considerable calcite, the latter mineral being a characteristic gangue of the copper ore. The relative ages of the sulphides as outlined above are shown not only in their relation to each other but also to some extent by their general location. The zinc forms the central core with the lead and copper along the edges of the zinc. This generalization is true only to a certain degree as the successive movements which opened up new channels for ore deposition along the edges of the already existing bodies also shattered these bodies. The mixture between the lead and zinc is, however, such an extremely intimate one that it is difficult to assume that it is formed solely by galena introduced into openings, however minute, in a shattered body of sphalerite. The relation of the two minerals to each other as shown in thin sections clearly indicates that they were not deposited simultaneously but that the lead is distinctly younger than the zinc. It would appear that the intimate relationship may be partly due to a replacement of the sphalerite by the galena. The galena having been introduced along minute cracks in the sphalerite gradually spread out from these cracks by replacement of the zinc, thus reproducing on a microscopic scale the method of formation of the orebody as a whole.

It does not appear that changes of any magnitude have occurred

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in the condition of the orebodies since their original formation. There is no distinct zone of secondary enrichment (barring the small amounts of chalcocite), a fact best indicated by the silver values which show no marked change from the upper part of the orebodies to the lowest levels, several hundred feet below the water level. There is a gradual increase of silver values on descending, but this is entirely due to a corresponding increase in lead. As has been stated previously, the lead-zinc ore maintains the same character from the surface to the lowest levels, varying somewhat in the proportion of one to the other but not due to any later rearrangement. As stated above, there is a gradual increase in silver and lead on descending. This is not, however, at the expense of the zinc, which also increases slightly, but is due to the almost entire disappearance of gangue minerals and of low-grade patches in the solid core.

The age of the orebody itself is extremely difficult of determination. The overlying sediments have been eroded from directly above the orebody and whatever patches of ore have been found in them at other points are of small size and may have been formed from already existing bodies in the tuff. This is not, however, certain and the barrenness of overlying sediments may be due to their unfavorable character as regards replacement by sulphides. If the overlying sediments are later than the ore deposition, it would mean that the orebodies are early Ordovician or late Cambrian. If the sediments, however, antedate the period of ore formation it becomes impossible to determine the age of the orebodies.

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Geology and Ore Deposits of Mohave County, Arizona*

BY FRANK C. SCHRADER,† WASHINGTON, D. C.

(New York Meeting, February, 1917)

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INTRODUCTION

THE present sketch is submitted by request in the hope that it may serve as a basis for geologic discussion of the mining camps in Mohave County, which is experiencing a marked revival of activities.

The region, commonly known as the Mohave district and Kingman district, lies in western Arizona in the southern part of Mohave County, bordering California and Nevada on the west (Fig. 1). Kingman, the principal town, is situated near the center of the area on the Atchison, Topeka & Sante Fe Transcontinental Railway.

This region is composed of naked desert ranges of mountains and broad detritus-filled valleys, the southern extension of the characteristic topography of the Great Basin. In altitude it varies from 500 ft. in the southwest to 8,300 ft. on Hualpai Peak southeast of Kingman.

The mountains trend north-northwest. They rise about 3,000 ft. above the valleys, are generally rugged and were formed mainly by erosion. They are composed in the main of a complex of pre-Cambrian granitoid rocks which underlies the area as a whole. Like the valleys, they average about 10 miles in width. Beginning on the east, they are the Grand Wash Cliffs, the Cerbat Range, the Black Mountains or River Range, and the Eldorado Range.

The upper or dominantly cliff half of the Grand Wash Cliffs, marking the edge of the Colorado Plateau, is composed of nearly horizontal sedimentary Paleozoic strata of the Grand Canyon section, and the lower half of the underlying pre-Cambrian complex.

The Cerbat Mountains situated in the central part of the area, and the Black Mountains situated between Detrital-Sacramento Valley on the east and Mohave Valley, the great trough of the Colorado River, on the west, are locally flanked or overlain by Tertiary volcanics (Fig. 2). The latter consist of five groups of mountains of which the most important is the Black Mesa group on the south.

The Eldorado Range, rising from the great trough of the Colorado on the west and containing Searchlight, Eldorado Canyon, and other camps, is topographically and geologically similar to the Black Mountains.

GEOLOGY OF THE DISTRICT[1]

The rock groups beginning with the oldest are the pre-Cambrian complex, Paleozoic sediments, pre-Tertiary intrusives, Tertiary volcanics, and Tertiary (?) and Quaternary sediments (Fig. 1). The first and third of the divisions named are the most important.

The pre-Cambrian complex consists of gray gneissoid granites, coarse,

[1] A fuller description of the rocks appears in *Bulletin* No 397, *U. S. Geological Survey* (1909).

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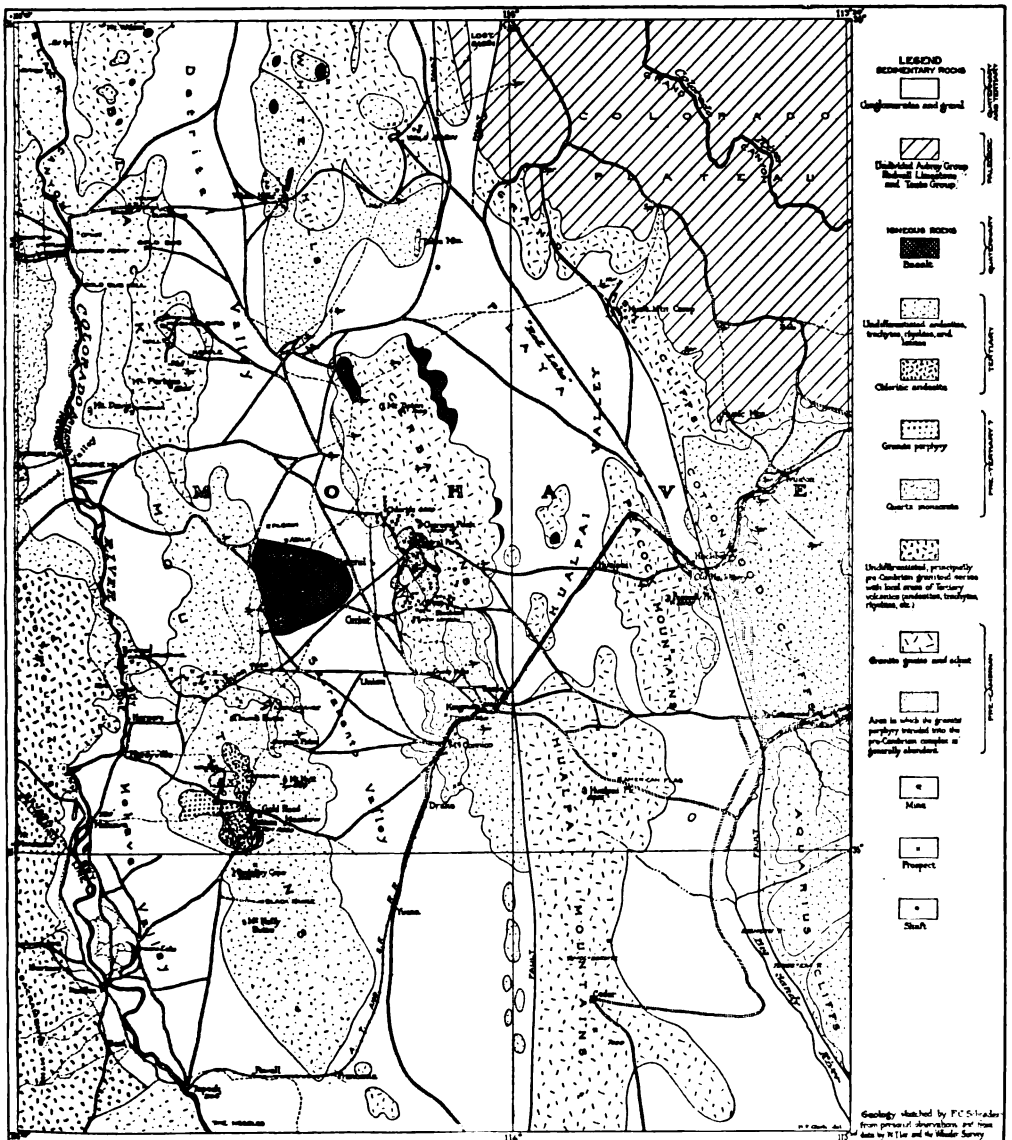
porphyritic granitoids and their related schists, all of igneous origin and with varying degrees of metamorphism. It is traversed by a schistose



GEOLOGIC RECONNAISSANCE MAP OF THE MOHAVE COUNTY MINING REGION, ARIZONA

FIG. 1.

structure trending N. 30° E., and contains nearly all the mineral deposits of the Cerbat Range and Grand Wash Cliffs.

The Tertiary (?) and Quaternary sediments consist of several placer

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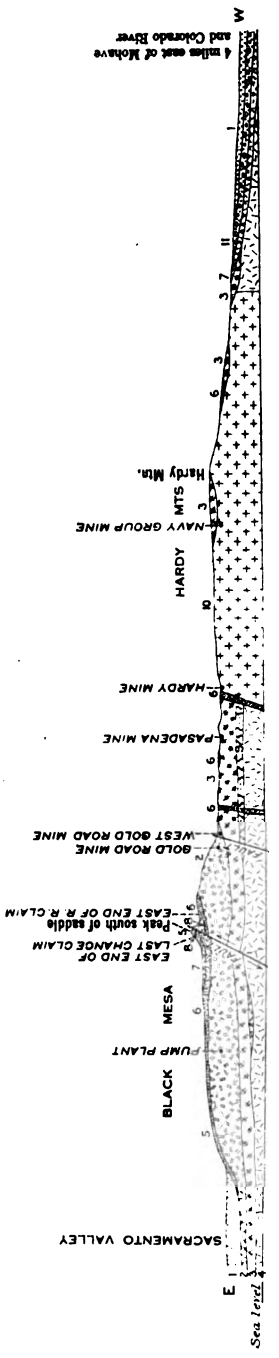

FIG. 2.—GENERALIZED SECTION ACROSS BLACK MOUNTAINS. 1, Sands and gravels; 2, undifferentiated volcanic rocks; 3, green chloritic andesite; 4, gneissoid granite; 5, basalt; 6, rhyolite; 7, young andesite; 8, rhyolite tuffs; 9, andesite and andesite tuffs; 10, granite porphyry and micropegmatite; 11, conglomerate.

Bulletin 397, U. S. Geological Survey.

gold-bearing detrital formations or "wash", locally 2,000 ft. in thickness, partially filling the intermontane valleys.

Locally intruding the pre-Cambrian rocks are pre-Tertiary igneous masses and dikes thought to be of late Jurassic or early Cretaceous age. They occur chiefly in the Cerbat Mountains and are connected with the genesis of the deposits. The most important are granite porphyry, a light gray medium-grained rock, and lamprophyric rocks, the latter occurring mainly as dark, complementary, narrow dikes accompanying the acidic intrusives.

The Tertiary volcanics consist mainly of andesites, trachytes, rhyolites, and latites, lying in broad superimposed sheets, flows and beds locally aggregating 3,000 ft. in thickness (Fig. 2). They are best developed in the Black Mountains, particularly in the southern part (Fig. 3). They contain most of the mineral deposits of the range and played an important part in their genesis.

ORE DEPOSITS OF THE DISTRICT

General Description

The discovery of mineral and the beginning of mining in the Mohave area date from the finding of ore at the Moss mine, 4 miles northwest of Gold Road in the early sixties. From 1904 to 1914[2] the production was nearly $16,000,000, of which $11,500,000 is in gold, nearly all derived from the Tom Reed and Gold Road mines. Besides gold and silver, zinc, lead, copper, tungsten, molybdenum, and bismuth are produced. The distribution of the districts or camps, about 30 in number, is shown in Fig. 4.

[2] *Mineral Resources, U. S. Geological Survey*, 1904–1914.

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The deposits are contained in two distinct groups of fissure veins. The first group consists of the veins of the Cerbat Range which occur chiefly in the pre-Cambrian rocks and are genetically connected with the Mesozoic intrusives, especially granite porphyry and lamprophyric rocks. They are quartz fissure veins in which the quartz carries principally silver but also gold and ores of the other aforenamed metals. They were deposited in depth by hot waters. Their deep-seated character and close association with the major geologic structures indicate continuity in depth. They seem likely to continue productive long after the gold deposits now attracting so much attention in the volcanic rocks of the Black Mountains shall have become exhausted. Oxidation extends to depths of about 300 ft. At present the sulphide ores are principally utilized, though the rich secondary oxidized silver ores furnished most of the early-day production.

The second group comprises the veins of the Black Mountains which occur chiefly in the Tertiary volcanic rocks and whose filling besides quartz includes calcite, adularia, and fluorite. They are deeply oxidized. The valuable constituent is almost wholly free gold.

The Cerbat Mountains Group

General Description.—The deposits of the Cerbat Mountains are mostly located at from 9 to 20 miles north of Kingman. Their production for the year 1915, according to the Chloride Mining Bureau, is $3,000,000. They occur in two sets of well-defined fissure veins, with steep dip-forming conjugate systems, one striking about N. 20° W., parallel with the dominant jointing, and the other N. 60° W. perpendicular to the schistosity of the rocks. Many of the veins have a length of nearly a mile. The structure is irregularly massive. Among the primary-ore minerals, the most important are pyrite, chalcopyrite, arsenopyrite, galena, and sphalerite; more rarely, molybdenite, gold-silver telluride and stibnite. The decrease in galena and increase in pyrite noted in the lower levels, suggests a gradual change in the primary filling. Silver and lead predominate in the Chloride, Mineral Park, and Stockton Hill districts; gold, zinc, and silver in the Cerbat district. The primary ore is leaner in gold and silver than the oxidized ore, and many mines which near the surface were silver mines, with increase in depth carried more lead, and at still greater depths have become cupriferous. The so-called "copper belt" of the area extends from Mineral Park northwestward toward Chloride for a distance of several miles. It contains the Pinkham and Midnight copper mines, and in the Mineral Park end of the belt a recently discovered "copper porphyry" deposit which is attracting attention.

The water level is found at a maximum depth of about 400 ft. In general, the ores above the water level are oxidized, but in many places

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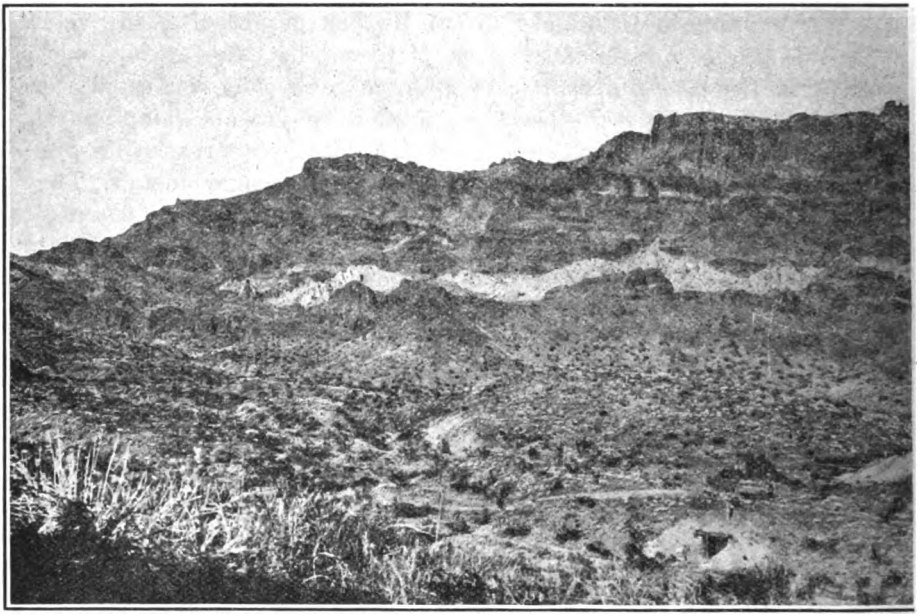

FIG. 3A.—TOM REED MINE AND VICINITY, LOOKING NORTHEAST (IN 1907). DARK

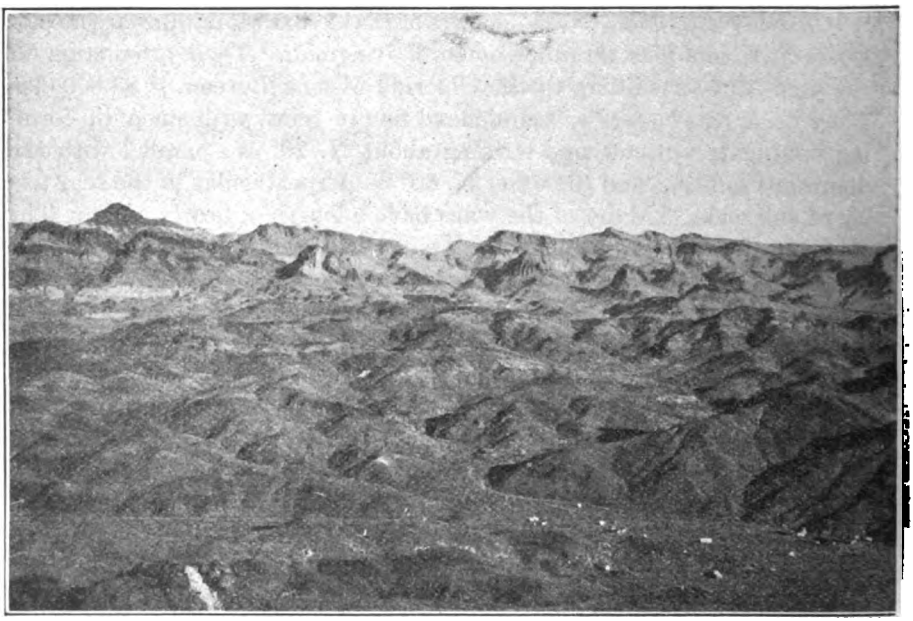

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FIG. 3B.—FOOTHILLS OF GREEN CHLORITIC ANDESITE, LOOKING EAST FROM LELAND RANGE, 4 MILES DIS-

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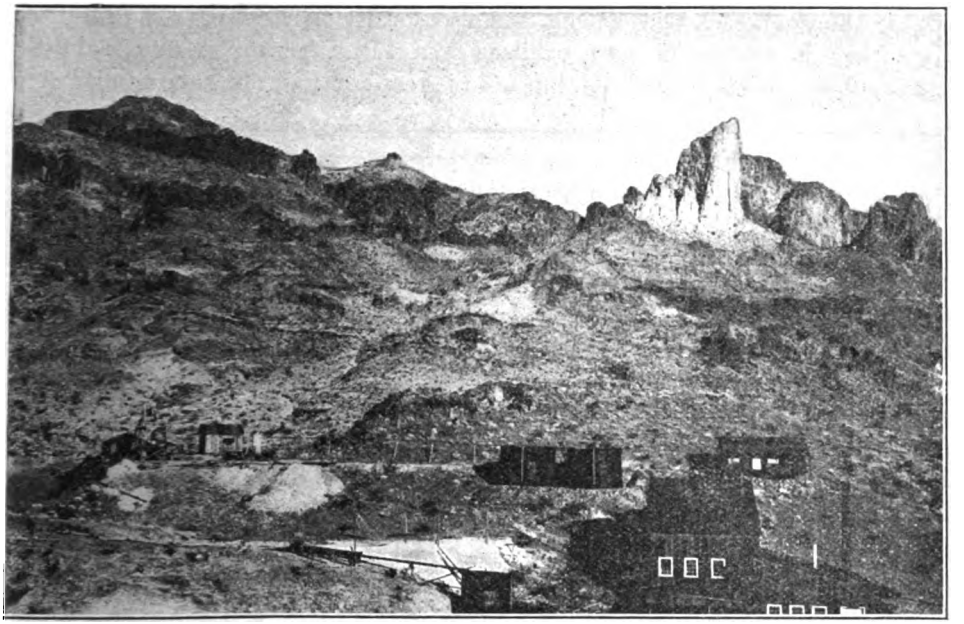
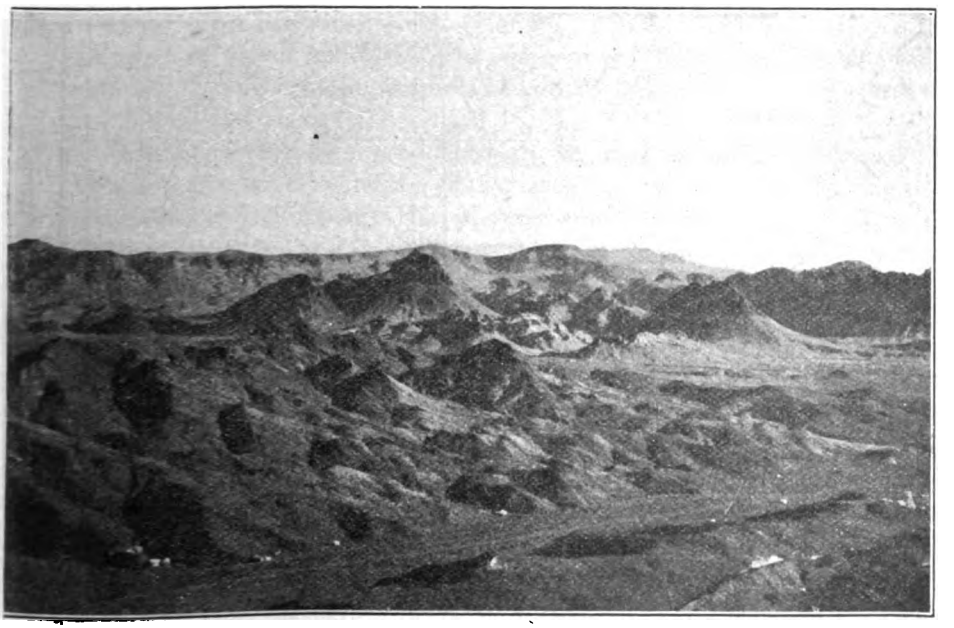


VOLCANIC ROCKS OF RANGE IN BACKGROUND, INTRUDED BY LIGHT-COLORED RHYOLITE.



MINE. MINES NEAR VIVIAN, AND GREEN CHLORITIC ANDESITE IN FOREGROUND; MAIN TANT, IN BACKGROUND.

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(*e.g.*, in the Tennessee vein) galena, and also, locally, pyrite, appears near the surface in association with oxidized ores. In a few mines oxidized ores are found below water level, but not to great depth. The secondary,

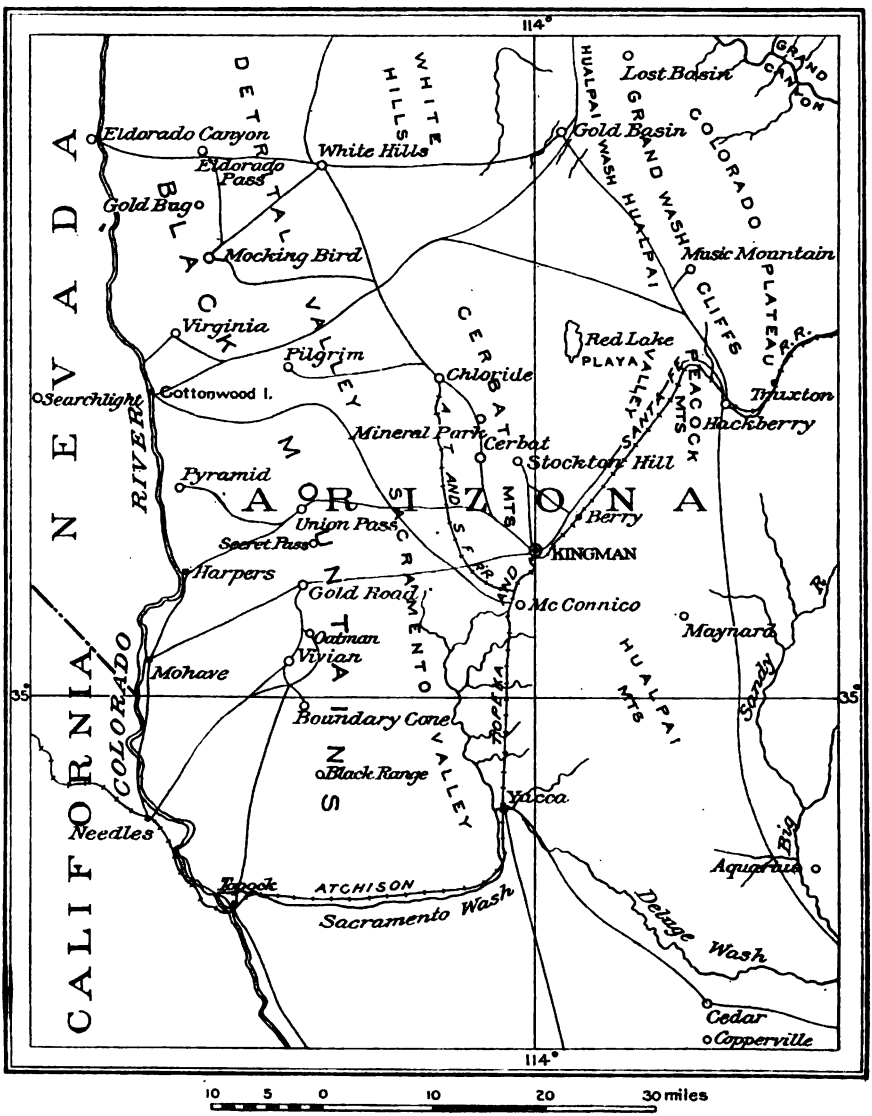

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FIG. 4.—MAP SHOWING MINING CAMPS IN THE MOHAVE COUNTY MINING REGION, ARIZONA.

or oxidized ores, consist chiefly of native silver, horn silver, and cerusite. Ruby silver and argentite are also present with oxidized ore, but do not occupy any well-defined zone between the oxidized and primary ores.

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Many of the ore shoots coincide with intersections or forkings of veins. Good examples were noted in the Pinkham, Elkhart, Rainbow, Pay Roll, and Tennessee mines.

Tennessee Mine.—In the Chloride district, a dozen or more mines are opened to depths of 200 to 1,000 ft. or more, and expose large quantities of good gold-silver and other ores. Among them the Tennessee mine, situated a mile east of Chloride and owned by the United States Smelting, Refining & Mining Co., has long been one of the greatest lead-zinc producing properties of the State. It is credited with a present monthly production of $150,000. It has good orebodies on the 400, 800, 900, 1,000, 1,200, and 1,400-ft. levels, the last-named being the present limit of development. During a considerable portion of the time in recent years it has shipped about 200 tons of ore daily, mostly to Needles. The present daily output is said to be about 300 tons, mostly from the 1,170, 700 and 500-ft. levels.

The mine is on the Tennessee vein, which is regarded as a part of the great lead-bearing "lode" on which the Schuylkill and Elkhart mines to the north are situated. The vein is 12 ft. or more in width, and is locally banded. It dips about 68°E. in pre-Cambrian gneiss, with granite and schist near by and a pegmatite footwall reported in the lower levels. The orebodies which occur as lenses in the vein average about 5 ft. in width. The ore consists mainly of galena and blende, but carries a fair amount of silver and some gold and copper. At present the zinc ore is shipped to the company's smelter at Bartlesville, Okla., and the lead ore to Midvale, Utah.

The mine has been productive almost from the surface down. From between the surface and the 400-ft. level, thousands of tons of rich galena ore have been shipped. Here the main ore shoot had a horizontal extent of about 250 ft., and in places was 15 ft. in width. On the 400-ft. level, an orebody 21 ft. in width with 5 in. of pure galena was mined for the distance of about 40 ft. From the fourth to the fifth level there was a predominance of blende, but from the fifth to the sixth level galena increased to the proportion found in the upper part of the mine.

The 500-ft. level contained good ore for a distance of 800 ft., and the raise from it showed 12½ ft. of almost pure galena. On the 600-ft. level, the vein contained about 10 ft. of good ore. Besides the aforedescribed deposits, large bodies of good zinc ore, some 12 ft. in width, on the 200-ft. and 500-ft. levels, have been left standing in the mine. According to recent reports there has just been opened up on the 1170- and 1400-ft. levels fine bodies of ore averaging about 25 per cent. each in lead and zinc. The body on the 1,170-ft. level has an average width of 8 ft. and a known horizontal extent of 250 ft.

Midnight Mine.—The Midnight mine, located about 2 miles south of Chloride, and adjoining the Pinkham mine, is said to have been recently

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purchased by Salt Lake parties for $250,000. It has produced considerable high-grade copper ore, which contained also important amounts of silver and gold. It is opened to the depth of 300 ft. and is said to have 25,000 tons of pay ore blocked out in the workings, including workable bodies of relatively pure zinc ore. On the 200-ft. level, where the lode is 40 ft. in width, the average zinc content is 15 per cent.

Mineral Park.—The copper porphyry deposit recently discovered near Mineral Park and owned by the Copperfield Copper Porphyry Co., occurs in "porphyry" which seems to be the intrusive granite porphyry aforedescribed, the abundant source of mineralization in this part of the field. The deposit is said to have a width of 1,000 ft. and a length of ½ mile. It contains seams and small bodies of chalcocite and native copper disseminated through the porphyry, which, throughout the greater portion of a 160-ft. crosscut tunnel, carries from 3 to 30 per cent. of copper, with a width of 6 ft., averaging 25 per cent. The deposit is reported to contain by estimate 100,000 tons of 5 per cent. ore. Ore removed in doing development work is reported being shipped to the Humboldt smelter.

Golconda Mine.—The deposits of the Golconda mine operated by the Union Basin Mining Co., in the Cerbat district, occur chiefly in the Golconda vein in the pre-Cambrian complex and seem to be associated with the Mesozoic intrusives. They have produced from essentially surface workings several hundred tons of rich ore containing chiefly gold, silver, and lead with some copper and zinc. The drift on the 300-ft. level is said to have been driven 200 ft. on a 4-ft. ore shoot that averaged about 50 per cent. of zinc, and more recently the mine is reported to be daily shipping to Bartlesville about 100 tons of high-grade zinc ore on which net returns of 9 c. per pound of zinc is realized. Some ore averaging about $12 to the ton is also treated in a 30-ton oil-flotation plant at the mine. The present monthly production is said to be about $250,000.

The mine is reported to have commercial ore on the 1,100-ft. level and a large amount of good ore in all other levels. The present production is derived mainly from the 800-ft. level. On the 900-ft. level, the ore shoot has a known extent of 850 ft. and a large tonnage of high-grade ore is being stoped. From this level a crosscut is being extended to the Tubb vein which parallels the Golconda vein 120 ft. distant on the west and has produced considerable lead-silver ore. On the 700-ft. level, the stopes are working in a 12-ft. shoot of excellent milling ore.

During the year 1915, the mine is reported to have paid two dividends of $85,000 each, which is about 20 per cent. on the issued capital, and having proved the continuity of the ore shoot in depth the company is now erecting a 200-ton oil-flotation plant for treatment of zinc ores. There is said to be $400,000 worth of zinc in the tailings on the dump and in the old stopes in the mine. The mill will be operated by electric power supplied by the Desert Power and Water Co. from its oil-burning

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plant at Kingman for about $12 per horsepower per month. The power line is also being extended to Chloride and the Tennessee mine. The introduction of electric power into the Cerbat Mountain districts seems likely to result in production from many mines now dormant but which, like the Tennessee and Golconda, are known to contain workable deposits. The prospect of cheaper power in the near future is said to be good.

Other Ore-bearing Districts.—Deep development is also being done by the Middle Golconda Co. on the adjoining Big Bethel and Silver claims, which are believed to contain the north extension of the veins of the Golconda mine. Here the main vein is 50 ft. wide and contains much good-grade zinc ore.

A few miles to the east, the Arizona-Butte Mines Co. is building a very complete mill to treat zinc-lead-gold-silver ores of the Banner and other Stockton Hill mines.

South of Kingman in the Yucca, Cedar Valley, and Aquarius Cliffs districts, respectively, plants are in operation producing concentrates of tungsten, molybdenum and bismuth ores.

Lost Basin District.—In concluding remarks on the eastern part of the region, attention is here called to certain copper deposits known for a decade or more in the Lost Basin district on the northeast. The occurrence of these deposits on the trend of the great northwest-southeast mineralization belt of Arizona which contains Bisbee, Ray, Globe, Prescott, Jerome, and other important districts, and under similar geologic conditions as productive deposits in most of those districts, seems to render them worthy of mention at this time.

With a width of about 9 miles, the Lost Basin district extends from the Colorado River at the mouth of the Grand Canyon southward in the Grand Wash Cliffs for the distance of about 20 miles, with Hualpai Wash roughly forming the western boundary. The topography is mostly rugged. The country rock consists of the pre-Cambrian granitic complex, which here is considerably schistose, and the overlying Paleozoic sediments of the Grand Canyon section (Aubrey group, Redwall limestone and Tonto group).

Important gold- and silver-bearing veins occurring mostly south of the middle part of the belt in the pre-Cambrian rocks have long been worked from time to time, and some are now producing. They strike about north. Their ores are fine in texture and are excellent cyaniding ores.

The copper deposits extend from the middle of the belt eastward nearly to the summit of the Grand Wash Cliffs and edge of the Colorado Plateau. They consist of copper-bearing quartz veins and lodes which trend northwest-southeast, some being exposed by erosion through a vertical range of several hundred feet or more. They occur chiefly in the granitic rocks, but some of them, notably on the east, are in the

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Paleozoic limestone and other sediments. The croppings are prominent and some are extensive. They consist principally of masses of brown and blackish iron, copper and manganese-stained quartz containing malachite and azurite. Some of the ore is reported to assay from 17 to 20 per cent. in copper and to contain also gold and silver.

The Black Mountains Group

The deposits of the Black Mountains are mostly on the western slope of the range. They occur in well-defined fissure veins, but differ in most respects very markedly from those of the Cerbat Range. They are found chiefly in the Tertiary volcanic rocks, and belong to the great group of deposits found in this class of rocks throughout the West.

Until recently the most favorable ore horizon was regarded as in the green chloritic andesite and the undifferentiated volcanics, with profitable though subordinate deposits occurring also in the upper rhyolitic series. Recent developments, however, seem to indicate that, in the Tom Reed-Gold Road district at least, the main ore zone probably extends to a deeper horizon, in the so-called older andesite or still lower rocks.

The veins in general trend northwest-southeast with steep northeast dip. They are fairly regular, but the walls are usually rough, broken and frequently full of stringers branching off from the vein. There is a general absence of fluccan or gouge. The gangue primarily was mainly calcite and dolomitic carbonates, but these minerals have largely been replaced by quartz and adularia, a variety of orthoclase free from sodium, semi-translucent and which is so intimately intercrystallized with the quartz that it is not recognizable to the eye. The gangue contains also many inclusions of brecciated altered country rock.

A striking feature of the gangue in many places, particularly in the Tom Reed-Gold Road district, is its characteristically laminated or platy, bladed and cellular structure, pseudomorphic after calcite, barite or other spar in which many contiguous or connecting plates are variously arranged. This material is aptly termed by the miner "fish-scale quartz," from the adjacent plates partly overlapping one another. The plates range from minute up to an inch in diameter and from the thickness of paper to $\frac{1}{10}$ in. in thickness. Much of the quartz intimately associated with the better-grade ore is of greenish or yellowish-green color and waxy luster, which has led to inquiry concerning the source of the color. The cause of the color is not definitely known, nor easy to determine. From preliminary tests it seems to be mainly silicates of iron, manganese, and perhaps other minerals, chlorite, actinolite, rhodonite, dioptase, etc., which may be an important source of the black iron and manganese oxides common as stain, small bodies and pockets in the croppings and more oxidized ores. It is noticeable that the greenish quartz

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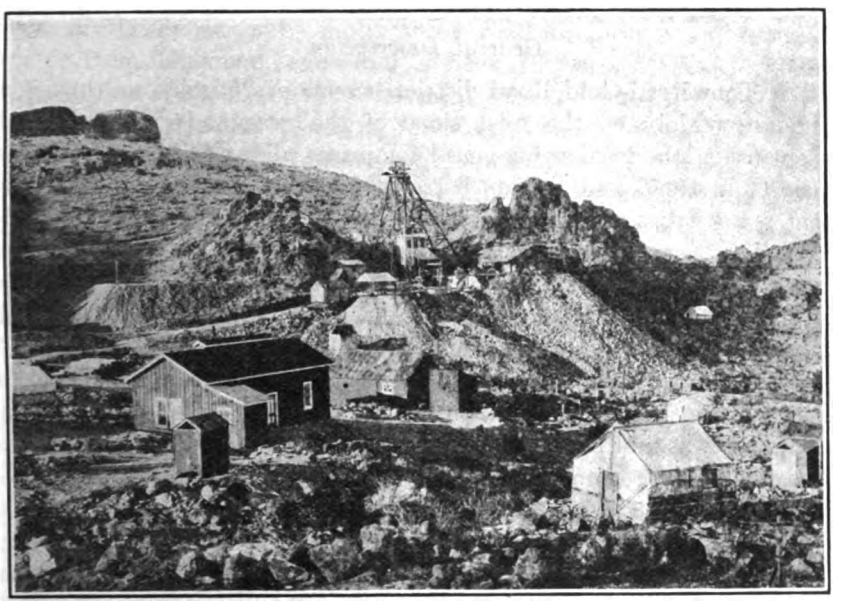
occurs more frequently in a crustified or banded form than does the uncolored gangue, which method of forming more readily favors the entering of various salts and minerals into its composition. In the Miller mine on the Hardy vein, 2 miles west of Gold Road, the greenish color of the quartz seems to be due largely to fluorite which is present in considerable quantity in the vein, much of it being replaced by quartz.

The deposits seem to have been formed near the surface by thermal solutions which circulated through the lavas at the close of igneous activity. They seem to belong to the late Tertiary epoch of metallization. They are oxidized to depths of 600 to 700 ft. and, as a rule, contain little or



Bulletin 397, U. S. Geological Survey.

FIG. 5.—GOLD ROAD MINE, MAIN SHAFT LOOKING SOUTH.

Silicified lode and vein wall croppings on both sides. Edges of heavy flows of volcanic rocks in left background.

no sulphides. Gold is almost exclusively the valuable constituent, usually no base metal being present. The gold as a rule is free, but occurs in very minute particles and is best recovered by the cyanide process. Gold telluride is reported from a few mines.

There is no gossan nor iron hat in the outcrops of the veins. In general, the veins weather in relief only where the filling consists chiefly of quartz or a mass of cemented silicified rock. There the croppings form prominent reefs. Likewise, the vein walls are frequently strongly silicified and hardened with the result that they too weather in forms rising to heights of 20 ft. or more above the surface and extending for considerable distances as seen at the Gold Road mine, Fig. 5. This hardened wall

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rock, or so-called "ledge matter," is sometimes netted by stringers of quartz branching off from the vein. It denotes arresting or damming back of copious mineral-bearing solutions that circulated at the locality, and generally indicates workable deposits in the adjacent underlying portion of the vein as described later under the Gold Road mine. Many of the deposits, as exemplified by the Tyro, the Gold Road, and other veins, carry relatively unimportant values near the surface.

Of the ten or more districts in the range the most important is the Tom Reed-Gold Road district.

Tom Reed-Gold Road District

General Description

The Tom Reed-Gold Road district lies about 25 miles southwest of Kingman, mainly on the west slope of the range. In keeping with present usage, the term as here used comprises what was formerly known as the Gold Road and Vivian districts, and the area is approximately co-extensive with the southern part of the San Francisco district of early days. The district has a north-south length of about 10 miles and a width of 6 miles. The principal camps and centers of activity are Oatman, the settlement of the Tom Reed and neighboring mines, situated in the west slope of the range 27 miles from Kingman, and Gold Road, 2 miles north of Oatman. For more than a year Oatman continued to be the center of attraction in the Southwest.

Mineral was first discovered in the district, as aforedescribed for the Mohave County region, in the early sixties at the Moss mine 4 miles northwest of Gold Road. The mine soon produced $240,000, in gold from rich surface ore.

Production in the district has continued more or less steadily since the discovery of the Gold Road mine in 1902. Recently discoveries in the Tom Reed mine and vicinity have been attracting attention to the district, with the result that the value of the plants and machinery now installed at the various mines is said to aggregate nearly $2,000,000. Some 50 odd plants are in operation. The greater portion of them have been installed since the first of the year 1915, during which time nearly 200 companies have been organized to operate in the district, of which 150 are fully equipped and most of the others are receiving machinery. Thirty or more properties hitherto dormant have become active, and the population, which is gathered from all the mining camps in the West, has increased from 600 to more than 7,000 and is gradually increasing. Oatman, which is said to recall Goldfields' boom, is described as a well-equipped, substantial town, a cleanly, orderly, model camp, where living is such as one expects to find only in large towns. It is electrically lighted, has three news-

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papers, schools, churches, a stock exchange, and well-stocked stores and business houses of all kinds. It is rapidly becoming the outfitting center for a large territory containing many new districts for a distance of nearly 100 miles north and south along the range. Wildcatting is checked by the laws of Arizona.

Owing to the demand for building space, a new camp, Old Trails, has grown up in the broad wash south of Oatman and there are a dozen other surrounding town sites and additions. Mail, passenger, and express service is by automobile chiefly from Kingman. A similar service exists between Oatman and Needles, 20 miles distant on the southwest, in which the Colorado is crossed by boat. Freight is delivered in the district at a cost of $11.50 per ton by motor truck, road locomotive, and mountain tractor from Kingman, and from Topock, formerly Mellen, located 25 miles distant at the east end of the railroad bridge crossing the Colorado. Early construction of a branch railroad from Topock to Oatman, which is quite feasible, is reported under consideration.

Good water for domestic use is pumped from neighboring springs found mostly in the porous rhyolite tuff or water rock. Seemingly, ample water for milling purposes is being found in deep mining. It also is palatable, being suitable for domestic and all other purposes. Should an additional water supply be required, it can be pumped from the Colorado River 14 miles distant, preferably from wells sunk in the gravel beds on the bank of the river. For this purpose, it is said, a company is being organized.

Development

With a capitalization of more than $53,000,000, operations are being actively prosecuted by 125 separately organized mining corporations and the activity seems to be warranted by substantial results of nearly all deep development. More than half of the companies now in the field are sinking or prepared for deep mining. Ten have good milling ore opened up and four have large producing mines of proven merit. Mining operations are steadily increasing in volume and area; more than 2,200 miners are actually employed in the district and more than $25,000 per day is being expended for wages and equipment.

The approved method of prospecting is sinking to depths of 300 to 500 ft. and then crosscutting and drifting. Practically no surface work is carried on. Gas-engine hoists, compressors, and Jackhamer drills are the usual equipment. Usually also much lateral development must be done before pay ore in large quantities is found and the mine proved. The automobile, a prominent feature in the present activity, has taken the place of the burro in prospecting.

The cost of mining and milling is about $6 per ton, of which $1.25 is for power. The power at the larger mines is electric power supplied

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by the oil-burning plant at Kingman. At the Gold Road mine, treating 200 tons of ore daily, the best record obtained for mining and milling is reported to be slightly less than $3 per ton. At the Tom Reed mine, however, where 20 stamps are used, the cost is about $6. There is said to be no profit in treating $5 ore in the district on a small scale. Both the Gold Road and Tom Reed mines treat their ore by the cyanide process, and have installed the counter-current decantation system.

From what has been said of the Tyro and Gold Road veins, and from the large number of other widely distributed profitable orebodies being found at depth and the cost of mining and milling, this is not a camp for the small operator but seems rather to offer encouraging possibilities for capital to engage in deep mining. The district has received the approval of many eminent mining engineers, a number of whom have become investors there and are now directors in some of the larger companies.

Topography

The district lies mainly in the Black Mesa Mountains, which, with an average elevation of 4,000 ft., extend from Gold Road 20 miles southward to the end of the range east of Needles. Their rugged forms are due chiefly to deep dissection of a huge volcanic plateau known as Black Mesa.

The district ranges in elevation from 2,000 ft. on the west and about 3,000 ft. on the east to 4,500 ft. at the top of the range. The range portion, which is about 4 miles in width, is marked by deep canyons, steep slopes, and peaks. In a horizontal distance of about 1½ miles, the surface declines from the elevation of 4,500 ft. at the crest to 2,500 ft. on Silver Creek just below Gold Road. The edges of the harder lava beds present steplike cliffs (Fig. 3 *A*).

The principal outliers are the Hardy Mountains, a group of hills situated about 3 miles west of Gold Road. They are about 3 miles in diameter and rise about 600 ft. above the surrounding country. Two miles to the north is a smaller group, the Moss Hills, while Leland Mountain at Vivian represents similar features on the southwest.

Geology

The Tertiary volcanic rocks prevail, particularly in the eastern or range portion of the district. They practically constitute the range, dip gently eastward toward its axis and are in places covered by younger rhyolite, andesite and basalt. In the southern part the green chloritic andesite is dominant, while on the west occur also local areas of the pre-Cambrian gneiss, younger granite porphyry and micropegmatite, greenstone agglomerate, and overlying sheets of supposed Tertiary conglomerate and younger gravel and lava flows. Locally intervening between the pre-Cambrian and the overlying volcanics are occasional remnantal patches of tilted and metamorphosed Paleozoic limestone and shale be-

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longing to the Grand Canyon Section. These sedimentary rocks are not as yet known to have any bearing on the deposits or mining other than to indicate to the miner where encountered the general lower limits of the volcanics.

Recent mine developments have disclosed the geology of the ore-bearing volcanics to be more complicated and seemingly of more importance to the district from a gold-producing standpoint than was at first supposed.

In the vicinity of Vivian, and extending from there toward Oatman, occurs the older or basal andesite, which is light gray, calcitic, 300 ft. in thickness, and rests mainly on the pre-Cambrian complex and Paleozoic sediments. The older andesite, however, is not known to be of wide extent in the district, a fact seemingly overlooked by Bancroft and others. It is seemingly absent from Secret Pass where the next higher rock, the green chloritic andesite, rests directly upon the pre-Cambrian granite, and from the Hardy Mountains where the green chloritic andesite similarly rests upon the Mesozoic granite porphyry or micropegmatite.[3] It is not known to be present at the Gold Road mine, and according to Sperr[4] the rock underlying the green chloritic andesite in the deep workings of the Tom Reed mine does not correspond to the older andesite described at Vivian. The older andesite is unconformably succeeded by another series of flows, the green chloritic andesite which contains an important part of the mineral deposits in the Tom Reed-Gold Road district (Figs. 1 and 3*B*). The flows aggregate a known thickness of 800 ft. The rock consists mainly of a greenish, fine-grained groundmass containing abundant whitish feldspar phenocrysts. It is very chloritic and calcitic. It is intruded by black latite and younger lavas.

The intrusive character of the green chloritic andesite or rocks grouped with it is well shown at the head of the wash, just west of the Leland mine, where dikes from 2 to 20 ft. in width, given off from the main mass, extend ⅛ mile or more westward into the older andesite. A black, fresh-looking specimen of it collected by the writer from the Leland mine proved by microscopic study and chemical analyses to be latite, and it contains chlorite in abundance throughout.[5]

The intrusive nature of the green chloritic andesite and the association of ore deposits with its intrusive phases in various parts of the district are also abundantly corroborated by later work of Sperr, Probert, Bancroft, and other engineers. Probert[6] believes it to be both intrusive and

[3] *Bulletin No.* 397, *U. S. Geological Survey*, p. 35, and Fig. 2 (1909).

[4] J. D. Sperr: The Tom Reed-Gold Road Mining District, Arizona, *Engineering and Mining Journal*, vol. 101, No. 1, pp. 1–5 (Jan. 1, 1916).

[5] *Bulletin No.* 397, *U. S. Geological Survey*, pp. 36–37 (1909).

[6] Frank H. Probert: Oatman, Arizona—A Prohibition Camp, *Mining and Scientific Press*, vol. 112, No. 1, pp. 17–20 (Jan. 1, 1916).

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extrusive, that dikes and sills of it occur in the older andesite and that mineralization is dependent upon this association.

Bancroft[7] writes that in the vicinity of the mines which he examined in localities rather widely scattered in the district, he found evidence of the intrusive nature of this formation, and that the orebodies are largely formed within the intrusive.

More recently, according to Smith,[8] the bottom as well as the collar of the Tom Reed shaft at 1,075 ft. in depth was in the green chloritic andesite which in the bottom of the shaft was ore-bearing, and he suggests that the rock may here be intrusive. The supposition of the rock being here intrusive, probably as a neck, would help to account for the unusual thickness of the formation at this point, which seems to be local, since elsewhere in the Tom Reed mine and in the neighboring United Eastern, Pioneer and other properties the workings, according to Schader,[9] passed through the green chloritic andesite and into the older underlying andesite at shallower depths and have workable ore in the lower rock.

Therefore, according to the observations of six or more investigators, the green chloritic andesite (formation) contains rocks which vary considerably mineralogically from the normal andesite, rocks with which the ore deposits in general seem to be associated and which are known to be intrusive into the older andesite. The most important of these rocks seems, to the present writer, to be the dark latite occurring at the Leland mine and elsewhere. It seems to intrude not only the older andesite but also the green chloritic andesite as sheets, necks and dikes, and to be intimately connected genetically with the ore deposits. More recently, Sperr,[10] whose observations in the district have been extensive, regards all the commercial ore as occurring in the andesites intimately associated with latites. The intrusive nature of the rocks associated with the ore deposits obviously favors continuity of the deposits in depth.

The deposition of the green chloritic andesite was followed by a period of great fissuring and faulting accompanied and followed by eruption of the next higher group, the undifferentiated volcanic rocks 2,000 ft. in thickness, containing the Gold Road and other important veins, and by intrusions of younger rocks, especially latite and rhyolite in the form of dikes, necks, and rounded plug or stocklike masses, and seemingly the formation of many of the larger fissure veins. The undifferentiated volcanics are succeeded by a series of younger light-colored tuffaceous

[7] Howland Bancroft: Geology of Gold Road District, *Mining and Scientific Press*, vol. 3, No. 1, p. 21 (July, 3, 1915).

[8] Howard D. Smith: The Oatman District, Arizona, *Mining and Scientific Press*, vol. 3, No. 5, p. 172–175 (July 31, 1915).

[9] Carl F. Schader: Personal letter, Feb. 6, 1915.

[10] J. D. Sperr: "Conversational Geology" at Oatman, *Engineering and Mining Journal*, vol. 101, No. 26, p. 1119 (June 24, 1916).

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rhyolites locally 1,000 ft. in thickness and known as the "water rock," which is succeeded by dark reddish andesite which in turn is followed by black olivine basalt, the youngest of the effusive rocks, which remains as a capping over a large part of the Black Mesa Mountains.

With the extensive development recently done in the district, the rocks merit detailed study with reference to their sequence and bearing on the genesis of mineralization. Such a diagnosis seems certain to prove of great economic value in preventing useless expenditure of money in some directions and leading to profitable development in others.

Ore Deposits

General Description.—The deposits, which are numerous, are chiefly gold-bearing fissure veins or lodes of the character already described for the Black Mountains. The veins vary from 5 to 70 ft. in width and from a few hundred feet to several miles in length. In general they are strong and persistent. They strike northwest with steep dip to the northeast. They are almost devoid of metallic sulphides, the gold being free. They occur chiefly in the lower part of the undifferentiated volcanic series, the green chloritic andesite, the granite porphyry and micropegmatite, other underlying rocks and also along certain contacts, where latite and rhyolite are generally the intrusives. Some of the deposits are very rich, but the large bodies of low-grade ore constitute the main resource. Ore having a metallic content of $10 or less is considered low-grade.

The older andesite, from the ill behavior and feathering out of certain vein deposits on entering it from the green chloritic andesite, was originally regarded by the writer as unfavorable for mineral, or essentially barren, particularly in the Vivian district. Owing to its tufaceous brecciated and fragmental nature it is almost devoid of lava-cooling shrinkage cracks and fissures, which elsewhere form favorable repositories for ore deposition. According to Palmer[11] "the occurrence of any ore shoots in the earlier (older) andesite is yet to be demonstrated."

Also, E. W. Brooks limits the area of commercial mineralization in this part of the field to the green chloritic or "younger andesite." Later developments, however, it is gratifying to note, in the Oatman and Vivian camps, report workable ore deposits in the older andesite also. It is hoped that with development similar reports may be received from several mines near Vivian which, though well-equipped for operations nearly a decade ago, have remained inactive. That the writer has never doubted that major veins probably occur in and below this formation is evidenced by the following statement: "The veins cut through the great mass of Tertiary volcanic rocks which characterize the range and un-

[11] Leroy A. Palmer: The Oatman District, Arizona, *Mining and Scientific Press*, vol. 113, No. 6, p. 195 (Aug. 5, 1916).

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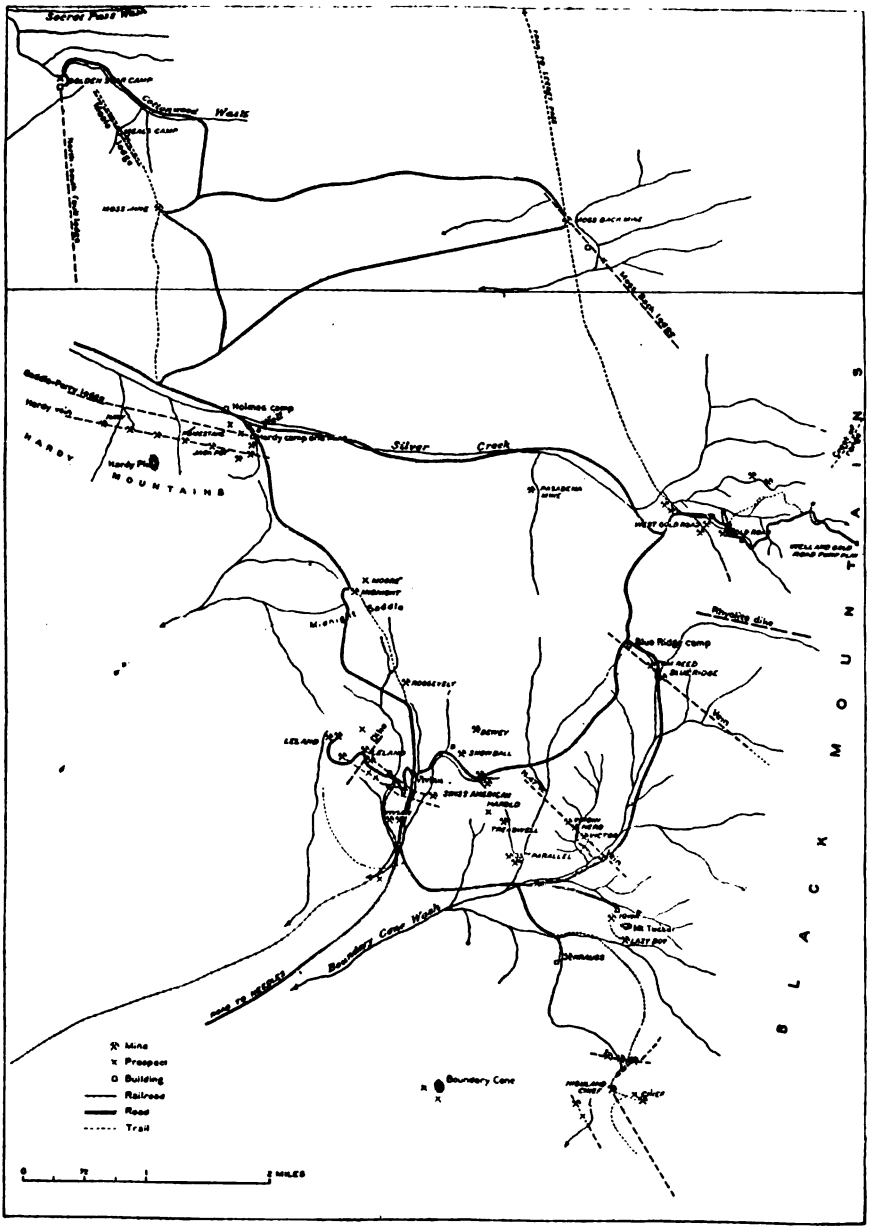

Bulletin 397, U. S. Geological Survey.

FIG. 6.—MINES AND VEINS IN THE TOM REED-GOLD ROAD DISTRICT.

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doubtedly continue in depth into the underlying pre-Cambrian granitic rocks."[12]

According to Palmer,[13] "some ore of value has recently been found in the pre-Cambrian."

Since the deposits are confined to the vein filling and do not as a rule form metasomatic replacements in the wall rock, as at Cripple Creek and other camps, the selective preference which any bounding wall rock by reason of its more favorable physical or chemical properties for replacement may exert in favor of ore deposition seems to be practically nil. Accordingly, there is no apparent reason, other conditions being equal, why the deposits should not be equally developed in any one of several formations through which the fissure vein with like strength may extend.

The deposits consist of two types—those in which the gangue is chiefly quartz and adularia and those in which it is chiefly calcite. The source of the quartz and adularia is referred to the siliceous magmas and that of the calcite to basic or andesitic magmas with possible contributions derived from underlying limestones. The former carry the best values, occur mostly in the undifferentiated volcanic rocks and in granite porphyry and have a general northwest-southeast trend. The latter seem to occur mainly in the green chloritic andesite and trend more nearly north-south. Among the most important of the former type are the Gold Road and Tom Reed veins; among the latter, the Pasadena, Mossback and Meals veins (Fig. 6). In some cases the veins are associated with boldly cropping silicified dikes of which the deposits in certain instances may be in part replacement.

According to Platts,[14] the most productive veins, such as those in the Tom Reed, United Eastern, and Big Jim mines, are in a complicated series of fissures, part of which strike about N. 45° W., and others N. 60° W., producing with each other a conjugated system with numerous intersections near which many large orebodies are found.

Zones.—Surficially, the veins seem to mostly fall into four main zones[15] which, named in order from north to south, are the Gold Road, Tom Reed, Vivian, and Black Range zones. The Tom Reed zone is the best developed and contains the most interesting discoveries.

There seem also to be two or more horizons or vertical ore "zones." The largest and richest orebodies seem in general to lie in a "zone" of enriched oxides between the 300-ft. and 500-ft. levels. Below this zone

[12] *Bulletin No.* 397, *U. S. Geological Survey*, p. 48 (1909).

[13] Leroy A. Palmer: *Op. cit.*, p. 195.

[14] J. B. Platts: Geology of Oatman, *Mining and Scientific Press*, vol. 112, No. 23, p. 814 (June 3, 1916).

[15] Leroy A. Palmer: The Oatman District, Arizona, *Engineering and Mining Journal*, vol. 101, No. 21, p. 895 (May 20, 1916).

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the ores decrease in value, but continue to be of workable grade beyond the deepest point yet penetrated by any working. The richness of this zone as suggested by Smith[16] is probably due to secondary enrichment, by contributions leached from shallower depths, in support of which the presence of vugs and manganese oxide in the upper part of the veins is cited. This view is also seemingly corroborated by the tendency of the zone to parallel the contour of the surface. For instance, its occurrence at about the same depth in the Gold Road mine as in the Oatman camp, though at correspondingly greater elevations and higher geologic horizons. The gold was probably precipitated in large part along with the manganese oxide.

If the thickness of 600 or 800 ft. assigned to the green chloritic andesite be correct, this ore zone in the Oatman camp or, more generally speaking, in the triangular area of several square miles comprised between the Tom Reed, Pioneer, and Pasadena mines, should lie mainly in this formation.

There seems to be also present, notably in the Oatman camp and vicinity, a shallow or surface ore zone of leached oxides to which pay ores found at or near the surface are generally confined. It extends to depths of about 150 ft., between which and the zone of enriched oxides, or 300-ft. level, lies a 150-ft. intermediate zone of leached or relatively barren ground, although the valuable ore shoots, according to Sperr,[17] almost without exception come at least within 100 ft. of the surface.

These two zones have probably suffered about the same amount of leaching, the upper zone certainly not less than the intermediate or barren zone. The upper zone seemingly owes its greater ore content to the more siliceous, and consequently resistant, character of the ore which accordingly better withstands the process of leaching.

Differing from the view of enrichment by leaching and redeposition in the main zone is that of Platts[18] which holds that the ore is essentially a primary deposit formed by heat ascending solutions, that from the nature of the gangue it is evident that acid solutions could not exist, and that, except for the oxidation of the pyrite, there is no evidence of the action of surface water on the ore.

It seems quite possible, as suggested by one writer, that the groundwater table in the district may be in part dependent upon the neighboring Colorado River. If this view be correct, physiographic study will probably be able to correlate certain horizon features of the vertical section as leaching, etc., with relatively prolonged pauses in the historical downcutting of the river. It does not, however, seem safe to assume that the water table at Oatman coincides with the level of the Colorado River,

[16] *Op. cit.*, p. 173.

[17] J. D. Sperr: "Conversational Geology" at Oatman, Ariz., *Engineering and Mining Journal*, vol. 101, No. 26, p. 1119 (June 24, 1916).

[18] J. B. Platts: *Op. cit.*

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which is 2,000 ft. lower than Oatman, and that therefore the ores if they persist downward will continue to be oxidized and of the same milling character to that depth as advocated by Palmer.[19] Owing to the greater elevation of the gathering zone on the east, which probably extends to the Hualpai Mountains, or longitude of Kingman, the ground-water table is not a level surface but gradually rises from the Colorado River, eastward, and at Oatman it probably stands several hundred feet or more above the level of the river.

Structure of Ore Shoots.—The ore occurs chiefly as a series of more or less tabular or lenticular ore shoots and pay streaks dipping and plunging variously within the vein, with which they exhibit a greater or less degree of parallelism. The shoots vary from 1 ft. wide to the width of the vein. They usually carry gold for their full width. They range up to nearly 1,000 ft. in length and depth, and there is a general similarity or repetition of the shoots in the same vein. They seem to have been formed by thermo-aqueous processes that followed igneous activity. In general, the quartz and values favor the hanging wall, which of the two walls is generally the best defined, and contains stringers branching off obliquely from the vein, while the spar or calcite favors the footwall. The gold is mostly associated with the quartz-adularia gangue and not rarely where sulphides have existed, it, according to Platts,[20] occurs in hematite (which is pseudomorphic after pyrite) in the quartz.

According to Palmer,[21] the first indications of the vein encountered in sinking are small stringers of quartz and calcite scattered through the andesite, usually accompanied by slight pyritization in the vein-wall andesite which yields a little free gold in the pan, while in the ore shoots the vein matter shows pronounced hematite and manganese stains. It is said that the problem in development is not so much the finding of veins as the discovery of ore shoots in the veins, that nothing sufficiently tangible has yet been found to use as the basis for a theory to guide the operator in the search for ore.

Though no rigid rule can be laid down to guide the operator in search for ore, nevertheless, from the apparently well-established facts that the metallic values have been largely imported by the replacement quartz-adularia solutions and that more gold is found where the replacement of calcite is most nearly complete, in formulating plans of exploration much benefit in most cases should be derived from a correlative study of the criteria indicating the probable courses followed by these solutions, namely, quartzose vein croppings, silicified wall rock, the quartz pseudomorphic structures, etc., which have been described. It was the quartz

[19] L. A. Palmer: The Oatman District, Arizona, *Mining and Scientific Press*, vol. 113, No. 6, p. 196 (Aug. 5, 1916).

[20] J. B. Platts: *Op. cit.*

[21] L. A. Palmer: *Idem.*, vol. 101, No. 21, p. 896.

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adularia or siliceous waxy-appearing character of the deposits seen in the Tom Reed mine and the recognition of their marked similarity to the then-producing deposits of the Gold Road mine that apparently led to the resumption of operations in the Tom Reed mine following the writer's visit in 1907.

Gold Road Mine

Some of the principal mines in which the deposits have been worked are the Gold Road, Tom Reed, Leland, Pioneer, Victor-Virgin, Midnight, and United Eastern. Their general distribution is shown in Fig. 6. The most important producers are the Gold Road and Tom Reed mines.

General Description.—The Gold Road mine, owned by the United States Smelting, Refining and Mining Co., is situated at Gold Road, on the western rugged slope of the range about 1 mile below the crest, at an elevation of about 2,900 ft., mainly on the western part of the Gold Road vein. From its croppings, the Gold Road vein has long been known, but the discovery of values which resulted in the opening of the mine was made about 1902. The mine soon began to be worked systematically and paid dividends of 5 per cent. on the capitalization of $500,000. Several years ago it was acquired by the present owner for $1,500,000.

The mine is opened to a depth of 1,000 ft. or more, mainly by shafts and drifts, and ore has been mined in quantities for a distance of 4,000 ft. on the vein.

Most of the work done up to 1907 was comprised in the ground extending about 200 ft. from the main or Gold Road shaft (then known as the Gold Road mine), in either direction on the vein, and to the depth of 500 ft. Since then, however, extensive developments have been made, especially to the east on the Billy Bryan and adjoining ground.

The gross production of the property is estimated at more than $6,000,000. To the end of 1907 the production was about $2,250,000, most of which was made during 1905 and 1906. Since 1909, the output has averaged about $800,000 annually, which figure in some years was much exceeded. The present monthly production is nearly $80,000 in gold bullion.

Geology.—The country surrounding the mine is occupied by the undifferentiated volcanic rocks consisting mainly of heavy sheets of volcanic flows (Fig. 5). The series comprises andesite, trachyte, latite, rhyolite, and dacite, which aggregate nearly 2,000 ft. in thickness and are difficult to separate. The series extends from a point about 1,000 ft. west of the main shaft eastward nearly to the crest of the range. On the west it gives way to the underlying green chloritic andesite, which in the West Gold Road mine, situated near the contact, has been penetrated to a depth of 455 ft. On the east it is overlain by the upper rhyolite. The

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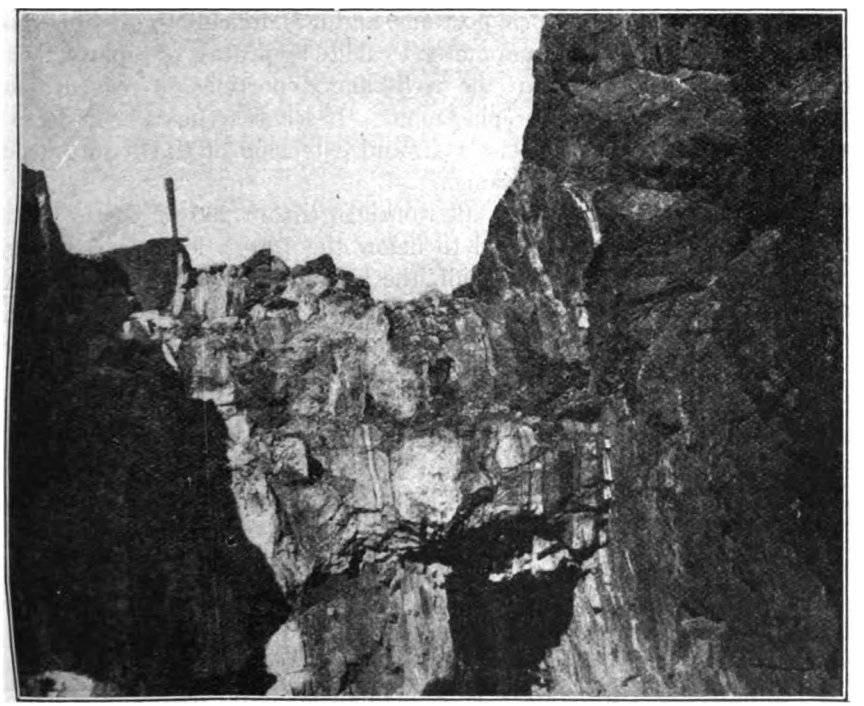
contact on the west is probably a fault contact, as some of the green chloritic andesite occurs in the main shaft workings. The dominant dynamic structure is a pronounced close sheeting, which strikes N. 40° W., with vertical dip. The series is intruded by dikes of rhyolite and the younger dark andesite. Much of the rhyolite carries gold values. A dike of it, 40 ft. in width, located about 600 ft. south of the mine, is said to average about $5 in gold to the ton, the gold occurring chiefly in contained stringers of quartz and calcite.

Ore Deposits.—The Gold Road vein extends from a point about 700 ft. west of the main shaft southeastward through the Gold Road, Rail-



Bulletin 397, U. S. Geological Survey.

FIG. 7.—GOLD ROAD VEIN AT EAST OF LINE ROAD CLAIM, LOOKING NORTHWEST.

road, Billy Bryan and Last Chance claims to beyond the crest of the range, a distance of nearly 1¼ miles. It dips about 80° NE. approximately parallel with the close sheeting in the country rock. It varies in width from 22 ft. on the west to less than 1 ft. on the east. Between the bottom of the mine and the crest of the mountains it has a known vertical range of about 2,300 ft., and its croppings have a vertical range of about 1,300 ft. They consist essentially of iron and manganese-stained quartz, silicified rock, and calcite. In places they form conspicuous reefs or knobs rising 20 or 30 ft. above the surface, as at the main shaft or Gold

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Road mine (Fig. 5). Here the associated hard silicified prominent wall-rock croppings on both sides of the vein have a lateral extent of 20 to 60 ft. The best ore usually underlies these prominent croppings, which seem to represent pool-like courses along which mineralizing solutions deposited more freely than elsewhere. Where the croppings weaken or break down, the underlying portion of the vein generally becomes lean or barren, though the fissure and its walls and filling may continue unchanged.

The vein consists essentially of quartz and adularia, with some calcite and brecciated altered rock, and is locally more or less crustified (Fig. 7). Since the more siliceous portions frequently exhibit a perfection of crustification not found in the calcic portions, and it is difficult to see how this structure could be derived from massive calcite by process of replacement, the quartz-adularia filling in the well-banded portions is regarded as largely primary rather than replacement. It seems to have been deposited in reopenings of the calcite vein and extension of the fissure itself eastward into the axis of the range.

In 1907, the vein in nearly all workings within 200 ft. of the main shaft and from the 300-ft. level to below the 700-ft. level was mostly good milling ore from wall to wall, the amount of waste in mining as shown by the dump (Fig. 5) being very small. Elsewhere, however, as seen on the Billy Bryan and other ground, portions of the vein, sometimes for a considerable extent, are relatively barren.

The vein is strongest on the west, where, as developed in the main shaft workings, it is uniformly about 10 ft. in width. It is usually in sharp contact with well-defined firm walls of the country rock, consisting of andesite, trachyte, latite, and rhyolite. It is, in general, "frozen" to the walls and is locally enriched where stringers extend from it at acute angles into the hanging wall. As a rule, hard and rough walls indicate good ore, and conversely, soft and smooth walls generally correspond with lean ore. The ore consists chiefly of fine-grained, light-greenish or waxy quartz Much of it is platy or hackly with a peculiar chalcedonic or drusy appearance. Much of it is pseudomorphic after calcite and many of the pseudomorphic plates are thickly studded with minute quartz crystals of a still younger growth. The gold, as seen microscopically, and in places by the naked eye, is very finely disseminated, principally in the quartz-adularia portion of the gangue. On the 600-ft. level, however, the vein contained more spar or calcite than on any other level, and here the ore shoot is reported to have had an uninterrupted extent of 1,100 ft. Some sulphide ore containing pyrite, the first encountered in the mine, was found here, and it is reported to have contained higher values than the overlying oxidized ore, which is probably due to arrest in the downward progress of leached concentrates. On the 700-ft. level, the country wall rock, consisting of green chloritic andesite, was more or

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less pyritic and seemed to indicate that the mine was entering the sulphide zone. The mine has not produced much ore from depths shallower than the 300-ft. level. The lowest-grade ore taken out was obtained between the surface and the 300-ft. level, where it fell to $5 a ton. The ordinary mill-run ore, it is said, rarely falls below $8 or $9 and ranges from that up to $22 a ton. It averages about $10 to the ton, but $100 ore and upward is occasionally encountered on the hanging wall, where nearly all the high-grade ore occurs.

On the Billy Bryan claim, nearly ½ mile from the main Gold Road shaft, the vein and the associated rocks show essentially the same characteristics and relations as in the Gold Road mine. The variations and phases of the rocks are well displayed in the large dump at the mouth of the drift. The principal rock corresponds to trachyte. It is in general considerably altered. The croppings of the vein are prominent and wall-like. The vein varies from 1 to 20 ft. in width and dips, in general, steeply to the southwest instead of normally to the northeast. It consists mostly of spar or calcite, which contains bands of greenish quartz. Beyond the southeast limits of the Billy Bryan claim, and, in fact, to the end of the Railroad claim, quartz prevails and carries good values. The quartz is greenish with glassy luster and locally is very brittle, closely crustified, wavy, and crinkled, its structure resembling the fine flow structure of certain lavas. Weed,[22] having access to later development, described the vein filling as "the usual mixture of calcite flakes and waxy quartz varying to a dense waxy yellow quartz in curly or crinkled bands and shreds sometimes showing a faint blackish stain."

In the eastern part of the Line Road claim, and in a considerable portion of the Billy Bryan and Railroad claims, the vein has produced very rich shipping ore from surface workings, and it is under these workings, notably on the Billy Bryan and Railroad claims, that the Gold Road Co. in 1908 encountered good ore in depth.

In its extension eastward through the Last Chance claim, and across the crest of the range, the vein contains some good-looking croppings and openings, but it is split by horses at several points and locally narrows to unworkable dimensions. In August, 1915, the mine was reported to have good-sized bodies of $20 ore on the 800-ft. level, and in November it is said to have encountered on the 900- and 1,000-ft. levels a body of higher-grade ore than any hitherto found on the lower levels.

Recently it is reported that the vein as stoped averages about 12 ft. in width. Three ore shoots are being stoped on the 500-ft. level and at greater depths. These are respectively 1,200 ft., 70 ft., and 700 ft. in length, being separated by small intervals of barren ground.

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[22] Walter Harvey Weed: The Kingman District of Arizona, *Mining World*, vol. 2, No. 23, pp. 1113–1114 (June 4, 1910).

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Tom Reed Mine

General Description.—In the last year or two, owing to new discoveries, interest in the district has centered mainly in the Tom Reed mine and neighboring properties, wherefore this part of the district is known as the Tom Reed district or camp, Oatman district or camp, or simply Oatman. Here the geology and ore deposit are similar to those of the Gold Road camp, except that the deposits occur largely in lower geologic horizons in the green chloritic andesite and still older rocks (Fig. 3*A*). Some of the veins are "blind," being covered by later flows of rhyolite and younger rocks whose contact is traceable by the softened character of the contiguous weathered portion of the underlying rock.

The Tom Reed (formerly Blue Ridge) mine is situated at Oatman about 2 miles south of the Gold Road mine and about 200 ft. below it. It lies on open ground in Blue Ridge wash, near the base of the central part of the range, at an elevation of about 2,700 ft. (Fig. 3*A*). It is one of the well-known mines of the country and contains well-defined ore shoots, which for nearly a decade have been worked with great profit.

It was discovered about 1900 and was soon after owned by a party composed of Ely Hilty and others. About 1901, the Gold Road Co. sunk two shafts on the property, the Ben Harrison and the Tom Reed shafts, each to a depth of about 100 ft. with good results. About 1904, the mine was purchased by the Blue Ridge Gold Mines Co., which installed a mill and operated the mine and mill for about a year and a half, milling on an average about 30 tons of $7 ore a day. In 1906, the Blue Ridge Co. was succeeded by the present owner, the Tom Reed Gold Mines Co., which resumed operations in 1907, and in 1908 the mine was reported to be working and making gold bullion shipments regularly, since when it has been a steady producer. A little later a 12-ft. wide body of $12 ore is said to have been encountered on the 300-ft. level.

The property comprises a group of 11 or more claims, adjoining one another in part end on and extending along the vein for a distance of about 3 miles. The mine is located near the middle of the group.

Ore Deposits.—The country rock is mainly the green chloritic andesite. The vein, the Tom Reed (formerly Blue Ridge) vein, strikes about N. 50° W. and dips about 70° NE. It nearly parallels the Gold Road vein on the north and the Victor-Virgin vein on the southwest, to both of which it is geologically and mineralogically similar. On the Tom Reed property, it has an extent of about 3 miles, Fig. 6, and it is said to be continuous with the Pasadena vein on the northwest, in which event it has a length of about 4½ miles, being probably the longest vein in the district. At the Tom Reed mine it ranges up to about 40 ft. or more in width with the fissure walls usually ill defined. It outcrops boldly for the length of nearly two claims. The croppings consist principally

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of the usual dark iron and manganese-stained quartz, silicified rock, and calcite.

The vein is mainly of the quartz-adularia-calcite type. Of the early-day ore, a considerable portion is reported to have run $25 in gold to the ton for the first 30 ft. in depth and about $12 from that point down.

In the Ben Harrison shaft and its workings, the vein has a width of 16 to 22 ft. On the 100-ft. level the vein consisted chiefly of 16 ft. of crushed quartz and rock with neither wall well defined; but toward the hanging-wall side there was 6 ft. of good-looking, more or less porous, clear quartz ore.

On the 150-ft. level the vein consisted mainly of crushed rock, but the footwall side of the drift was in quartz; the hanging wall contained vugs 6 in. to 1 ft. in diameter, containing blackish, porous, oxidized quartz ore.

Development.—The mine is opened to a depth of 1,400 ft. It has more than 30,000 ft. of underground work, with the longest drift extending more than 4,000 ft. out from the shaft and in ore nearly all the way. The production to June, 1916, was nearly $6,000,000. That for the year ending March 31, 1916, was $661,870.68, the average value of the ore being $22.12 to the ton and the extraction 98.6 per cent. Dividends paid during the year amounted to nearly $164,000, or 18 per cent. on the par value of the outstanding stock. By estimate 11,000 tons of ore were blocked out in the stopes at the end of the year. The mine has paid more than $2,500,000 in dividends. The net realization on the mine by June, 1913, was $3,019,569.75. By June 24, 1914, the 44th dividend had been declared. Later the mine was reported to be paying for the last several years monthly dividends of from 6 to 7 per cent. on the par value of the stock, and the ore then blocked out, by estimate $2,000,000 worth, was said to be sufficient to continue their payments for several years to come. By 1907 the production considerably exceeded $120,000, and that since 1910 is more than $5,000,000. The annual production for the last few years is reported to average about $1,200,000 in bullion, besides which a large tonnage of ore is accumulating at the mine, especially of $10 ore in the workings.

Up to June, 1913, much of the ore produced had been drawn from an orebody between the third and fifth levels, where about an equal amount remained, and this same orebody had been proven to a further depth of 200 ft. below the fifth level. The seventh-level drift, at this time 233 ft. in length, was all in ore of about the same average value as that on the fifth level. Recently, in the Black Eagle section of the mine, the crosscut on the 400-ft. level is said to have passed through 35 ft. of good ore, 30 ft. of which averages $25 to the ton, and the ore tonnage on the 600-ft. level is very large.

Later, good orebodies were reported on the 500, 600, 700, 900, 1,075,

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1,200 and 1,400-ft. levels. The ore on the 1,200-ft. level is said to be similar in character and grade to that on the upper levels. Progress work on the 1,400-ft. level it is said has revealed a vein width of 12 ft. with a large orebody having a known extent of 300 ft., averaging $12 to the ton and containing some high-grade ore.

Explorations on the 1,075-ft. level for 225 ft. west of the shaft have shown the ore shoot throughout that distance to have a width of about 18 ft. and to range from $22.50 to $40 to the ton. On and below this level the vein is reported to be disturbed by a fault, but at the 1,175-ft. level it has fully recovered its former size and values.

Character of Ore.—The ore is similar to that of the Gold Road mine. It consists of a mixture of flaky calcite, waxy quartz, adularia, brecciated altered rock, and pinkish argillaceous material which is frequently of high grade. According to Weed,[23] a dense quartz whose color and luster closely resembles that of beeswax constitutes the richest ore. The ore is not hard and most of the gold is free, especially in the ore from the lower levels, but it requires fine grinding to free the finely disseminated gold and expose it to the action of the cyanide. The gold is seldom visible even in rich ore.

In milling, the total sliming system of cyanidation is employed, followed by treatment of the ore in Pachuca vats and Dorr thickeners. "Dorr thickeners," according to Smith, "appear to be particularly suited to conditions in this district where little silver is present, weak solutions are used, and the slime settles quickly." The ore amalgamates about 50 per cent. of its value on the plates and a high extraction is obtained at reasonable cost by cyanidation. In 1910, the average extraction was $42.46 to the ton. In 1912 it was $23.22. The amount of ore treated in 1911 was 39,447 tons; in 1913, 948,110 tons, with an average extraction of $24.09, and a recovery of 97.05 per cent. The average value of the ore mined in the fiscal year 1914 to 1915 is $21 to the ton. The gold is generally pure, the proportion of silver present being very small. The mill treats about 4,000 tons of ore a month.

Other Mines

Among the new properties which are attracting most attention is the United Eastern which adjoins the Tom Reed mine on the northwest and is often referred to as the "Bonanza." It is but a year and a half old, and is reported to have in sight, according to the estimates of conservative mining engineers, $11,000,000 worth of $26 ore. The mine contains more than 2,000 ft. of drift and has good orebodies on all levels between the depths of 300 and 700 ft. The vein is reported to be 43 ft. wide on the 555-ft. and 665-ft. levels, and the entire width is pay milling ore. Of this

[23] *Op. cit.*

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width, 30 ft. proven for the distance of 650 ft. averages $40 to the ton and carries considerable free gold. Since August, 1915, daily shipments of $30 ore removed during development are being made to the Gold Road mill. A new 200-ton cyaniding plant, in which gyratory crushers and ball mills instead of stamps will be used, is being installed at the mine. The equipment will be adequate for sinking to the depth of 2,000 ft.

Many other new properties like the United Eastern are being opened up and in many of them good ore is being found at depths of from 200 to 500 ft. A score or more are worked by incorporated mining companies.

Extensive developments are being undertaken at the Pioneer (formerly German-American) mine by the Oatman Pioneer Mines Co. which by coöperation and use of its efficient machinery and 400-ft. level will immediately facilitate the exploration and working of adjoining properties. The Pioneer is on one of the three main lode outcrops of the district, and is said to have $1,000,000 worth of commercial ore in sight between the 400-ft. level and the surface. It is working on two veins of the 400-ft. level, of which the northeast, or Pioneer vein, has an 8-ft. oreshoot having a known extent of 600 ft. and averaging $16 to the ton, and on the southwest, or Snowball vein, the crosscut has penetrated a width of 16 ft. of good-grade ore with the outer wall not yet reached. On the 200-ft. level, a 2-ft. shoot of high-grade ore is being worked.

The Boundary Cone mine adjoining the Pioneer on the east has good ore on the 750-ft. level, and is said to contain a 5-ft. shoot of ore that averages $100 to the ton on the 200-ft. level. On the 500-ft. level, where the ore shoot has been proved for a distance of 90 ft., it has a width of 12 ft. and averages about $20 to the ton. The mine is credited with a considerable production of high-grade ore, some of which contained crystallized gold.[24]

In the Big Jim mine, a mile northwest of Oatman, the vein on the 400-ft. level is said to be 51 ft. in width, of which 46 ft. averages about $8 to the ton and 8 ft., on the hanging-wall side, $15 to the ton with some pay streaks which are very rich. On the 485-ft. level, the same ore shoot has been opened for the extent of 200 ft. and is good milling ore, most of which averages more than $12 to the ton. Here the ore is said to be more oxidized and silicified than on the upper levels. The vein parallels the projected course of the Tom Reed vein, but whether it is the northwestward extension of the Tom Reed vein which may here be faulted to the northeast, or a new vein, has not yet been determined. The mine is daily shipping about 30 tons of ore removed in development.

In the Carter mine, ½ mile south of Oatman, the main ore shoot, about 15 ft. in width descending from the 150-ft. level to the 400-ft. level, is said to contain 5 ft. of ore which averages about $30 to the ton, and 8

[24] W. P. DeWolf: The Tom Reed-Gold Road, *Salt Lake Mining Review*, vol. 17, No. 13, p. 16 (Oct., 1915).

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ft. that averages $8 to the ton. This property is said to have shipped, several years ago, some very rich ore taken from between the surface and the 150-ft. level. This same vein is thought to extend through the adjoining Telluride and Lucky Boy properties, where on the 300-ft. level the entire width of 25 ft. is good milling ore.

The Gold Reed mine, a mile south of Oatman, has 4 ft. of $32 ore on the 375-ft. level.

On the Times property, the vein recently opened at 270 ft. in from the portal of the Martin tunnel, reveals 5 ft. of ore, averaging nearly $24 to the ton.

On the north, the Gold Ore mine, ½ mile northeast of Gold Road, is credited with a 9-ft. vein on the 500- and 550-ft. levels containing a 6-ft. shoot of ore which for the distance of 300 ft. averages about $12 to the ton, and a considerable portion of it nearly $100 to the ton. More than 75,000 tons of ore are said to be blocked out above the 550-ft. level. Daily shipments of ore averaging nearly $25 to the ton are made to the Gold Road mill.

The old Moss mine, where the original discovery of mineral in the Mohave County region was made, is being developed by the owners of the Gold Road mine and $60 ore is being mined from the 200-ft. level.

In the Ivanhoe mine, 2 miles northwest of Oatman, the vein, whose footwall is a partially mineralized 75-ft. "quartz porphyry dike cutting andesite and underlying sedimentaries," on the 250-ft. level, has a width of 60 ft., and on the 500-ft. level 6 ft. of milling ore has just been crosscut on the footwall side. Some high-grade ore has been shipped to the Gold Road mill.

Attention is also being attracted to the Secret Pass district, 6 miles north of Gold Road, which with a small mill is making considerable production mostly from high-grade surface ore. Here the occurrence is unusual, the ore, according to Payne,[25] being found chiefly as replacement deposits in the rock walls of a fissure occupied by a dike which the ore seems to postdate. The bullion, which is shipped to the U. S. Mint at San Francisco, is said to average about $15 to the ounce in gold. The population during the last few months of 1915 increased from 100 to 400, with prospecting extending over an area of several square miles.

At about 6 miles south of Oatman, promising deposits are reported in the Black Range zone, where a dozen companies are operating. Concerning the rocks in this part of the field, which have been roughly grouped with the undifferentiated volcanics, little is known as yet.

The deposits occur in a series of veins of which the one on which the principal mines are located is a prominent quartz outcrop known as the Nellie vein. It has been traced for the distance of several thousand feet and opened to the depth of 300 ft. on the Black Range, Nellie and Green

[25] C. Q. Payne: Oral communication.

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Quartz properties. Associated with the more pronounced quartz replacement phase of the vein on the Black Range and Nellie ground, are said to be rich streaks of ore that show coarse gold when panned. The Black Range mine is said to have milling-grade ore of irregular occurrence on the 300-ft. level, of which 3 ft. averages about $30 to the ton. Here, also, the country wall rock extending along the vein, for a width of 60 ft. or more, is impregnated with replacement deposits and averages nearly $6 in gold to the ton.

In referring to the future of the Tom Reed-Gold Road district, some men favorably compare it with Goldfield, Cripple Creek or other large camps and hold that it will become one of the greatest gold-producing districts in the United States. Among the more conservative and seemingly reasonable views is that of Palmer,[26] who believes that it will become comparable with the Tintic district, Utah, that it will become a large low-grade camp with several producing mines which will yield dividends for many years to come.

Fields Similar to the Tom Reed-Gold Road District

Recent investigations[27] have shown that the southern end of the Black Mountains containing the Tom Reed-Gold Road district is the easterly one of a number of similar volcanic areas which extend interruptedly westward on either side of the railroad through the distance of nearly 100 miles to the longitude of Barstow, Cal. In these areas—which embrace the Mohave and Chemehuevis Mountains, the ranges west of Von Triger, Clipper Mountain, the Cady Mountains, the Newberry-Ord district, and the well-known Calico Mountain group and others—the geological and mineralogical conditions are very similar to what they are in the Tom Reed-Gold Road district. The areas lie from a few miles to 25 or 30 miles back from the railroad. Their volcanic rocks, which in character, recurrence, and succession, are in general identical with those in the Tom Reed-Gold Road district, range up to 2,000 ft. or more in thickness, are frequently well-mineralized, and contain strong veins. Most of the areas have been but little prospected, but some, as that of the Calico Mountain group, are productive.

[26] *Op. cit.*, p. 900.

[27] N. H. Darton and others: Guidebook of the Western United States, Part C, The Santa Fe Route, *Bulletin No.* 613, *U. S. Geological Survey*, pp. 142 to 162 and sheets 21, 22, and 23 (1915).

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Discussion

J. Dana Sperr, Jerome, Ariz. (communication to the Secretary*).—Very little accurate information has been published about this district. Most of the geological data appearing in the technical press are based on "careful observations" made from an auto stage and a casual glance through the *U. S. Geological Survey Bulletins*, No. 340 of 1908 or No. 397 of 1909, both by F. C. Schrader. Mr. Schrader's report is still the best publication on the district. A really careful study of this report will uncover only a very few statements which have since proven questionable. This is a remarkable record when it is considered that only a short time was spent by Mr. Schrader in the field and that there was very little development work done at that time.

The mining world and especially Mohave County, Ariz., will feel a sense of gratitude to Mr. Schrader for revising his early report on this district and publishing it through the American Institute of Mining Engineers.

It would be absurd for me to dwell on general conditions, geological or otherwise, at this time, so I shall confine these notes to a few personal observations and opinions which may be of interest.

The "Conversational Geologists" delight in showing a proficiency in distinguishing the rocks of the district from hand specimens and proving Mr. Schrader wrong in the conclusion (erroneously laid to him) that the older andesite is non-productive.

One qualifying statement made by Mr. Schrader (p. 181 of *Bulletin* No. 397) in which he seems to have expressed one fact of greater economic importance than all the others combined, has generally been overlooked.

"So far as learned, the older andesite as a rule does not contain workable mineral deposits except along lines where the latite has erupted through it."

That statement is just as true today as it was 10 years ago. With the possible exception of the Big Jim, not a single commercial ore shoot is known to exist in the andesites which is not intimately associated with a latite. (In this district I prefer to use the name "latite" to classify any rock too acid to be an andesite and too basic to be a rhyolite. It is generally impossible to classify any of the intermediate rocks in the locality from hand specimens or even by the microscope. Mr. Schrader remarks that some of the rocks which he calls "green chloritic andesite" may be latite, so, what I refer to as a latite may, in some instances, be the equivalent of the "green chloritic andesite.")

It is impossible to classify positively the different rocks in the field, and nearly so with a microscope, owing to the extreme alteration which they have undergone. As engineer for the Tom Reed, I had some 50

* Received Jan. 17, 1917.

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specimens examined by C. F. Tolman, Jr., of Stanford University and succeeded in getting three or four definite classifications. However, it is generally possible to map the different flows, at least over local areas.

There is in the district an andesite younger than the vein system, which is a little more basic than the biotite or chloritic andesite and resembles, from the descriptions, the older andesite. This later andesite is found as a capping to a depth of from 200 to 300 ft. on the Black Eagle claim of the Tom Reed. It probably belongs to the undifferentiated

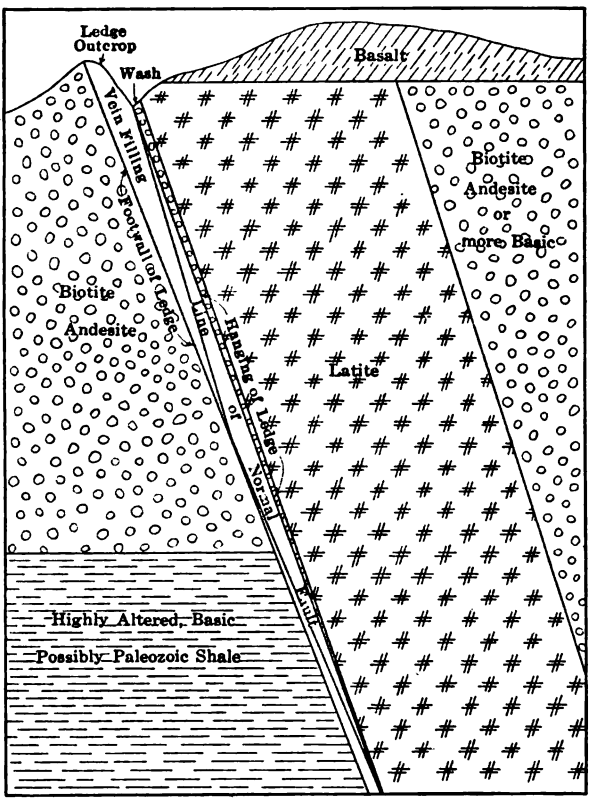

FIG. 1.—TRUE CROSS-SECTION OF TOM REED ZONE, OATMAN, ARIZ.

series mentioned by Mr. Schrader. It accounts for the impression, frequently gained, that the older andesite is sometimes younger than the green chloritic andesite. I have thought it possible that the occurrence of an andesite under the green chloritic andesite described by Mr. Schrader might possibly be due to a faulting which had placed this still later andesite in a relatively lower position over local areas. This possibility might be supported by the fault, which I am informed exists, between the two andesites described as found at the Leland.

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It must be noted that the physical appearance of different parts of the same flow may vary considerably. This fact, I believe, has led to some very erroneous conclusions. My notes show that the foot wall of the United Eastern mine is a nearly normal biotite andesite from where the shaft first penetrated the ledge to at least 600 ft. in depth, although the physical appearance of the rock at 200 ft. and at 500 ft. is decidedly different. Probably the same condition has existed in other places where the ore is reported to be found within the older andesite.

I cannot admit the intrusive character of the andesite of the Tom Reed, as the bottom levels show a nearly horizontal contact between the andesite and a basic rock which may possibly be an earlier flow or a remnant of the Paleozoic sediments. But the latite on the hanging wall is almost certainly an intrusive. This latite is green and chloritic, but the microscope shows it to be a nearly normal biotite latite.

Mr. Schrader notes that the gangue was primarily mainly calcite and dolomitic carbonates, but these minerals have been largely replaced by quartz and adularia. I have observed that the greater the amount of quartz after spar, as a rule, the richer the ore. Lower levels observed show a constantly decreasing amount of quartz with uniform decrease in the gold content. At depth the vein filling seems to grade into a pure spar. Bottoms of well-developed ore shoots show very little replacement. There is no questioning the continuance of the main fissures to greater depths than will ever be opened up. The real question is whether there will be a limit to the barren calcite zone and the conditions favorable to replacement again exist.

As to the vertical horizons, the richest ore ever mined in the district was found between 200 and 300 ft. in depth.

At the bottom of p. 217, Mr. Schrader outlines a general rule to guide the operator in search of ore, with which I am heartily in accord.

If a given vein or vein system in the district shows the quartz pseudomorphic structures, etc., mentioned, its development is an excellent gamble. But so far the calcite veins have proven too uncertain to be very safe.

A condition frequently observed is the occurrence of basalt plugs, often found on the strike of the main fissures. These plugs break up the continuity of the veins, sometimes reversing the pitch and causing other freaks. Some of these plugs come to surface and may even spread as a capping over a considerable area. Others do not reach the surface. Basalts seem to be good things to stay away from.

The accompanying cross-section through the Tom Reed zone may be of interest.

It will be noted that the main fissure is the line of a strong fault. While the amount of throw of this fault is not known, it is probably about 900 ft. The physical character of the rib of andesite on the hanging wall

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of the ledge at the 900-ft. level is very similar to that of the foot wall at or near the surface. No further evidence is known to the writer as to the amount of the throw.

The fault plane marked through the vein filling is a post mineral slip, very regular and distinctly traceable. I recall the first time I observed this slip. A raise had been driven carrying this slip as a foot and as I climbed through the raise I thought it had been concreted, the wall was so smooth and even. Deep vertical striations are the only irregularities.

The latite shows an intense crushing action wherever opened. In early work it generally appeared that the latite was the line of a main fault, but finding andesite on the hanging wall opposite a lower rock on the foot rather disproves this theory.

The andesite found on the hanging wall of the latite does not appear to be the same andesite within which the ledge occurs. It may be a glassy phase of the biotite andesite, but probably it is a more basic variety.

Angular inclusions of the andesite have been found within the latite 50 ft. from the actual contact. This seems to be characteristic of the latite andesite contacts in this zone.

Microscopic examination showed a resorption by remelting of the phenocrysts within the border of the andesite on the foot of the latite intrusion.

All of the rocks show an intense hydrothermal action. Practically all of the specimens collected were too highly altered for accurate determination. All but the latites show high oxidation. Pyrite in fine cubes is the strongest characteristic of this latite.

The drawback to mining is the high cost of prospecting, but when it is remembered that one United Eastern will pay 6 per cent. on 50 or more well-financed prospects and one Tom Reed will do over twice as well, the gambling chances will not seem so discouraging.

JOHN B. PLATTS, Hawthorne, Nev. (communication to the Secretary*).—I will confine my remarks to the Oatman district and to two points in which my ideas are in opposition to those of Mr. Schrader.

First, regarding the older andesite (or earlier andesite), I note that Schrader admits that this formation has greater importance in the district than he allotted to it in his former report on the district. Schrader was the first to describe the older andesite, and it is for him to say what it shall include, but in my work I have taken it to include everything between the Paleozoic and the base of the green chloritic andesite. Between these limits may be found not only the rock described by Schrader near the Vivian mine, but a number of variations from this type. The rock is always light gray in color and is nearly always in the form of consolidated tuff, or breccia and tuff, but sometimes appears as hard uniform lava flows. I was able to trace this formation on the sur-

* Received Jan. 2, 1917.

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face from a point near the Ivanhoe mine to a point several miles south of the Black Range mine, a total distance of about 8 miles. In some places, notably near the Gold Range shaft, the beds of tuffaceous andesite are intercalated with thin beds of limestone. The older andesite beds may often be distinguished from the overlying formations by the difference in dip. They dip from 15° to 20° eastward and the upper lavas dip from 4° to 5° eastward. The upper surface of the older andesite forms a wavy contact with the overlying andesites, indicating a former rolling surface in an advanced stage of erosion. This indicates a long time interval between the eruption of the older andesite and the succeeding eruptions. It would seem that there was first a series of island volcanoes followed by quiet and elevation into continuous land. After a period of erosion and tilting, volcanic activity broke out afresh in the eruption of the green chloritic andesite and after a short period of quiet, the latites. After another short period of quiet the rhyolites were intruded in the form of thick dikes and stocks.

The second point has to do with the genesis of the orebodies. Schrader regards the profitable ore as the result of secondary enrichment, and says, on p. 216: "The gold was probably precipitated in large part along with the manganese dioxide." Statements similar to this have been made by a number of writers. So far the only tenable hypothesis to explain secondary enrichments in gold deposits is that proposed by W. H. Emmons. His idea is that manganese dioxide acts on chlorides, setting free chlorine which acts on any gold present, forming gold chlorides. These being soluble in cold acid water, are carried away and precipitated on the first reducing agent or alkaline substance encountered. It is evident that the gold deposits will not be found with the manganese dioxide but with the precipitating agent. Miners working in gold veins containing rich bunches in the oxidized zone are familiar with the idea that the gold will be found in the iron-stained quartz and that the black manganese stains indicate barren spots. It is evident from the chemical nature of calcite that gold chlorides cannot form in its presence, and that it can only act as a gangue for secondary gold when it is absent from the greater part of the vein that held the primary gold, both from the vein matter and from the walls.

It is unnecessary to enlarge on the application of the above principles to Oatman veins. There the wall rock is so calcitic that it will effervesce with weak acids, as limestone will. The gangue of the largest veins, including the most important so far found, such as the United Eastern, The Big Jim, and the Aztec, contains rather more calcite than quartz. It is, therefore, evident that any secondary gold deposits must be extremely localized and relatively unimportant.

The rhyolitic intrusions seem to be a more probable source of the gold-bearing quartz than the latites. Gold and silica dissolved with

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alkaline sulphides could have been driven off from them through cross fissures and the more permeable andesite beds, passing outward and upward until they encountered andesite dikes in the case of the prominent siliceous outcrop veins, and calcite bodies in the case of the three large mines previously mentioned. Incidentally, the finding of these large non-outcropping orebodies does not favor Schrader's idea that the siliceous outcrops are a guide to the finding of profitable ore. Also, a number of cases might be cited where prominent outcrops have proved to be the tops of unprofitable veins.

JOHN CARTER ANDERSON, Kingman, Ariz. (communication to the Secretary*).—From an extended study of the Oatman and Secret Pass districts of Mohave County, Arizona, I believe that the genesis of the ore deposits, in most cases, is directly connected with rhyolitic intrusions later than the Tertiary flows; and that the developments of the future will prove these intrusives to be the primary source of the ore.

In support of this opinion I will instance the Wilhelm-Eclipse dike in Secret Pass and the Murdock, Nellie, Black Range dike and ledge in the southern part of the Oatman district.

The Wilhelm-Eclipse dike is a very fine-grained light-colored siliceous rhyolite dike with a strike of N.45W. traversing the full length of the Secret Pass district about midway between two major faults which mark the easterly and westerly boundaries of the district. These faults have lifted the block between so that erosion has exposed the pre-Cambrian granitic complex which underlies the range. The geological conditions present on surface are, therefore, analogous to what would probably be the 2,000-ft. level of the Tom Reed mine in Oatman, or several hundred feet deeper than the deepest opening in that mine. From the Wilhelm group of claims in the southeast end of the district, where the granitic complex is best exposed, to the Nancy Lee mine at the other end of the district, the surface rises several hundred feet. About one-third of the way from the Wilhelm to the Nancy Lee the granite is overlain by the green chloritic andesite. The dike is opened by several shafts from 40 to 200 ft. deep, the deepest being on the Eclipse claim just below the contact between the andesite and the granite. In every opening ore has been exposed. The gold is all fairly coarse as compared with the Oatman gold and some seams and streaks of high-grade have been cut which show wire gold up to ½ in. in length.

The high-grade ore is usually found in the rhyolite along the footwall contact. In the high-grade the gold occurs principally along seams and fracture planes in the rock, or in recementing quartz veinlets showing visible free gold. A cross-cut from a 50-ft. shaft on the Wilhelm cuts 45 ft. of rhyolite which is very much shattered and open, with a great deal of manganese dioxide in shrinkage cracks and fracture planes,

*Received Feb. 14, 1917.

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and carries from a trace to several dollars in gold. The rock is heavily impregnated with hematite, which is clearly pseudomorphic after pyrite; and carries the best values where least altered and the poorest where the manganese is most abundant. At the Nancy Lee, where the thickness of the green chloritic andesite above the granitic complex is several hundred feet, the dike cuts a quartz and calcite cross-vein similar to those of the Oatman district, some good values resulting in the quartz vein near the intersection.

Similar conditions are being disclosed by the development of the property of the Murdock Mining & Milling Co. in the Black Range section of the Oatman district. Through the kindness of Mr. Porter, the General Manager, I have been able to learn of the results of their underground work.

The Murdock property is situated just southwest of the Boundary Cone and covers the extension of a mineralized rhyolite dike. Eastward from the end line of the Murdock the dike is largely covered by wash in its course through the property of the Oatman Syndicate. Still farther eastward its place is taken on the property of the Nellie by one of the typical quartz and calcite veins of the district.

The country rock at the Murdock is the older, or basal andesite, which directly overlies the pre-Cambrian granitic complex. An exposure of the granitic complex is to be seen just south of the dike. A cross-cut on the 300-ft. level from a shaft sunk in the hanging-wall andesite, after passing through 82 ft. of rhyolite and a 12-ft. contact ledge on the foot wall, cuts the granite. At the Nellie the country rock on the foot-wall side of the vein is the basal andesite, while the shaft sunk in the hanging wall was in the next higher flow of trachyte for 250 ft. There is a cross-fault between the Nellie and the Murdock and the block in which the Nellie is located is relatively downthrown with reference to the Murdock.

The rhyolite of the dike on the 300-ft. level of the Murdock is heavily impregnated with hematite and assays low in gold for the full width. The character of the rock, the mineralization, and the gold content are exactly similar to that in the Wilhelm-Eclipse dike coming from the same geological horizon. In both instances the gold is free, relatively coarse as compared with the gold in the producing mines, and is contained in the heart of the hematite.

At the Nellie, the vein filling on surface and where cut on the 350-ft. level is largely calcite and a pseudomorphic replacement by quartz and adularia; but one specimen from the 350-ft. level, said to be from the highest-grade streak cut, was quartz and rhyolite impregnated with hematite and identical in character with the rhyolite of the Murdock.

As the dip and strike of the dike on the Murdock and the vein on the Nellie are practically identical and the connection can be traced on

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surface, I believe that a rhyolite intrusion, exposed by the deeper erosion on the Murdock, came up along a preëxisting fissure already filled with a calcite vein filling and that the solutions emanating from the dike were the agencies by which the calcite was replaced wholly or in part by quartz and adularia and the vein filling impregnated with auriferous pyrite.

This connection between the rhyolite dikes and the quartz and calcite ledges of the district cannot elsewhere be traced so conclusively, as few rhyolite dikes have been exposed by the erosion in the areas covered by the Tertiary flows. In the eroded areas, as at Secret Pass, however, the intrusive dikes are as frequent as veins in Oatman, and I believe that deeper development in Oatman will result in discovery that the ore-bearing quartz and calcite-filled fissures connect in depth with rhyolite dikes having a primary auriferous pyrite in the body of the dike; and possibly a richer contact vein between the dike and the granite.

In drifting along the contact vein exposed on the 300-ft. level of the Murdock, according to Mr. Porter, alternating small shoots of rich and lean ore have been cut; the face of the drift for several feet of advance at times averaging as much as $50. Specimens of this ore appear to be composed largely of brecciated and recemented rhyolite, similar to the high-grade ore from the Wilhelm-Eclipse dike, and show free gold in hematite and in the recementing quartz veinlets. It is probable that at points of greatest shattering, as at intersections and crossings, this recementing will be sufficient in extent to form good-sized shoots of high-grade ore.

Further evidence in favor of the rhyolite as the source of the primary ore is found in certain silicified flows which carry low values wherever sampled. The average of a number of samples recently broken by the writer at random from a silicified surface flow of rhyolite over four claims was 82c. per ton.

What the tenor of the primary ore will be, it is impossible to say, as the oxidation of the pyrite, and possibly the impoverishment of the primary ore, extends below the deepest workings in the district. There are undoubted evidences of leaching and reënrichment in the higher horizons of the veins, probably as a result of the percolation of surface waters later than the original deep-seated oxidation of the pyrite. A similar leaching of a portion of the gold in the primary ore may have accompanied the oxidation of the pyrite to depths as yet unknown. The nearest approach to an approximation of the value of the primary ore is found in the values cut in the body of the dike at the Wilhelm on the 50-ft. level and the Murdock on the 300-ft. level, and I believe that the values which will be found below the present water level of the Wilhelm will be higher than those in the cross-cut on the 50-ft. level.

With reference to the pyritic andesite cut on the 700-ft. level of the Gold Road mine indicating the near approach of the sulphide zone, it is

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my observation that this pyritization of the green chloritic andesite is characteristic of the wall rock, usually on the hanging wall, in a number of instances, and the unoxidized pyrite extends practically to surface, where that formation is exposed on surface. Among the places where I have noticed this occurrence of pyrite in the wall rock is the cross-cut into the hanging wall on the 200-ft. level of the Black Eagle mine of the Tom Reed, in the hanging wall of the 300-ft. level of the main Tom Reed mine, in the cross-cuts on the 300-ft. level of the Oatman Amalgamated and in two places in Secret Pass. The oxidation that has taken place in the veins to an unknown depth seems to have had little effect on the pyrite in the wall rock. This is probably due to the fact that the water of the district is almost wholly confined within the walls of the vein itself. In only one instance that I know of does the pyritic wall rock carry gold minerals. That is at the Orphan group of claims, now the Secret Pass Gold Top Mining Co., in Secret Pass. Here the ore from a winze which enters the pyritic andesite at about 50 ft. carries $8 in gold.

This is the property spoken of by Mr. Payne. The mineralizing solutions here have come up along a major fault which marks the westerly boundary of the Secret Pass district, spreading out irregularly into the wall rock. Several thousand dollars worth of high-grade ore from a shallow glory hole were milled in a two-stamp Tetrault mill by leasers, and a considerable tonnage of surface ore running from $8 to $15 per ton in gold was opened up. This fault is later than the Tertiary flows and the rhyolite intrusions which it cuts. It is easily traceable for several miles and just east of the Orphan mine has lifted the pre-Cambrian granitic complex to a contact with an andesite later than the green chloritic andesite, which in Secret Pass immediately overlies the granite. The granite on the foot-wall side is irregularly mineralized, and in several places high-grade gold ore has been found in prospect holes.

One other ore occurrence in Secret Pass is worthy of particular notice. A rhyolitic dike of a somewhat coarser texture than the Wilhelm-Eclipse dike and having a strike of N. 35 W. which is paralleled by the major fault marking the easterly boundary of the mineralization of the district, has mineralized the granite for from 25 to 35 ft. on each side of the dike. The granite is stained a deep red by iron oxides and a sample across 23 ft. on the foot-wall side of the dike at a depth of 12 ft. assayed $4 in gold. No deeper development has yet been undertaken on this ledge.

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Fuel in Turkey

BY LEON DOMINIAN, NEW YORK, N. Y.

(Arizona Meeting, September, 1916)

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I. INTRODUCTION

APART from local needs in the country's development, the interest attached to Turkish fuels grows chiefly out of the importance of the railway industry in Asiatic Turkey. Geographical conditions make of the tracks in this region the most essential link in a chain of world-girdling lines. By its position Asia Minor can be likened to a bridge in the through routes destined to connect European factories with Asiatic and African markets. An uninterrupted right-of-way from points in central Europe to populous Indian cities must necessarily pass through the valleys of Anatolia. Crossing thence the Taurus Mountains, its natural passage is indicated as the Mesopotamian lowland and the shores of the Indian Ocean. Similarly, land connection between Europe and Africa can be obtained only by passage through Asia Minor, Syria and Palestine.

The occurrence of fuel within an area of such significance from the

[1] Part of the notes on Coal have already appeared under the writer's name in The Coal Monograph published by the Twelfth International Geological Congress. These are presented here in revised and amplified form.

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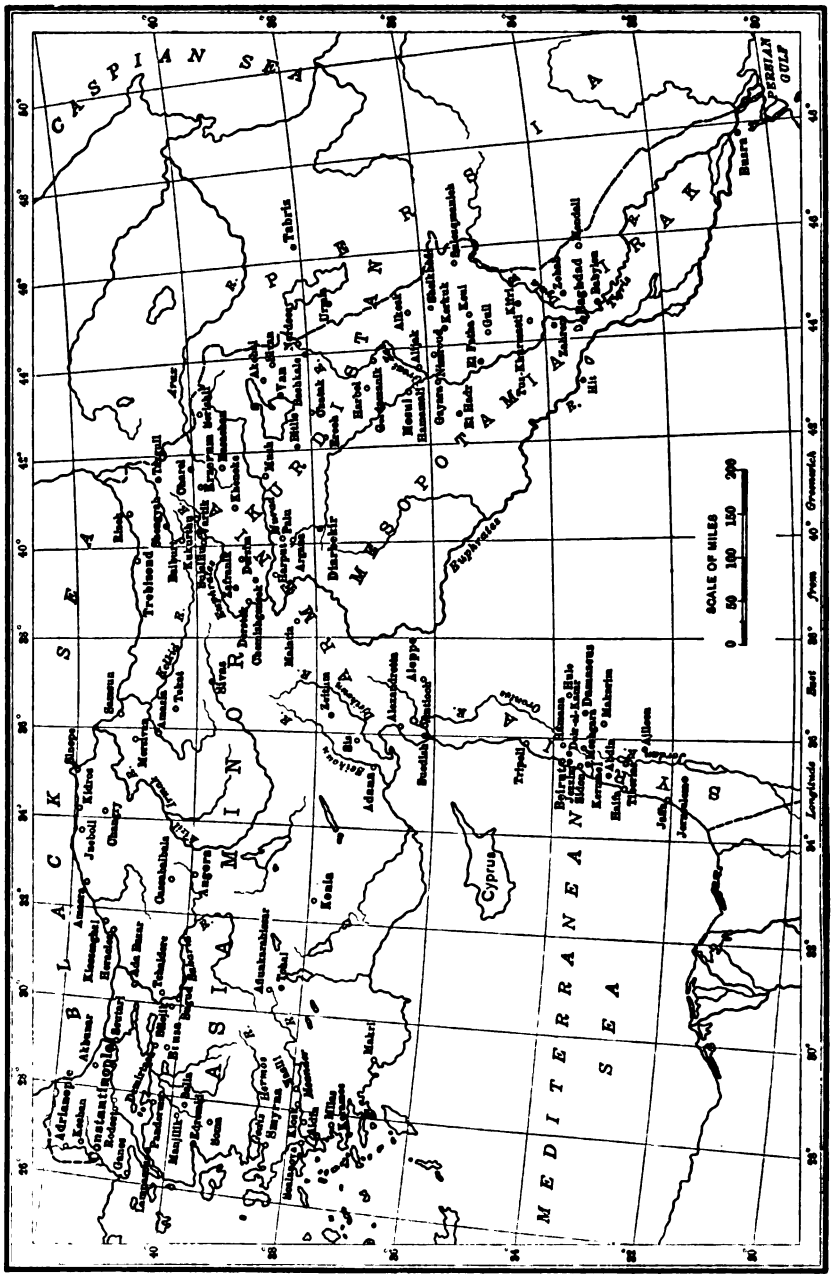

FIG. 1.

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standpoint of world traffic, therefore, deserves investigation. Unfortunately, our knowledge of the fuel resources of the Turkish Empire is still in a very immature stage. A comprehensive geological survey of the country, undertaken in the light of the modern progress achieved in this science, has not yet been attempted. What little is known has been due to the experience of explorers who were often beset with innumerable drawbacks while journeying through the country. The lithological terminology used by travelers is often inadequate. Little reliance can be placed on its accuracy except in the few instances where a geologist has been over part of the field. Some data of local interest can be culled occasionally from the written contributions of engineers stationed at the few exploited districts.

The occurrence of coal and oil has been reported from a large number of localities. In only one instance has mining been carried on in any manner compatible with the demands of modern consumption. Aside from this, work has been confined to desultory attempts that were scarcely adequate to meet even the limited local requirements. In no case can technical data regarding the extent, depth or persistence of seams be had. The present contribution will therefore be necessarily limited to an enumeration of the localities at which the fuel is known to exist.

II. COAL

(*a*) Western Asia Minor

1. *Lignite Beds of the Marmora-Ægean Zone.*—The northwestern projection of Asia Minor, immediately southeast of the Dardanelles, forms part of a district wherein lignite deposits have been worked in a number of localities. The formations of this region can also be traced to the north on the adjoining European coast of the Marmora. The lignite beds occur mostly in Tertiary lacustrine deposits and appear to belong to a zone of transition between the Miocene and the Pliocene. Plant accumulation prior to fossilization is probably connected with the recession of the Sarmatian waters, subsequent to which the depressions occurring in various points of the Troad and the environing regions offered favorable sites for the collection of plant detritus.

The lignites thus formed may have industrial value. Dr. Launay[2] cites occurrences in the vicinity of Ghemlik where seams of the fuel are found sometimes in a remarkable state of freedom from impurities. Here sandstone and conglomerate strata contain small masses of lignite mixed with pyritic sands. Occasionally, however, lignites acquire considerable value in connection with local industries. A deposit of this kind is worked at Manjilik by the owners of the nearby Balia lead mines.

[2] *La Géologie et les Richesses de l'Asie Mineure*, p. 296.

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The product is used in an electric power station feeding the smelters and workings.

The coal-bearing strata are found at a distance of about 5 km. north of Manjilik in the valley of the Deirmen Dere. Their total thickness attains 10 m. and their dip varies between 30° and 70° W. Coal outcrops can be traced over a length of 10 km. The lignite contains on an average 30 per cent. ash. Its seams lie between sandstone strata capped by portions of a large flow of trachytic rocks characteristic of the locality. A small mine railway connects the coal workings with the mines in the vicinity of Balia and extends thence southward to Osmanlar.

Some lignite is also found near Lampsacus, but it has not yet been mined. The fuel-bearing strata underlie clay beds which are worked extensively for the well-known pottery industry of the Dardanelles. In the Troad, lignite is extracted near Edremid. The fuel is transported to the seaport of Akchai where it is stacked along the water front and sold in small quantities. Farther south, at a short distance from Soma, the terminal of a branch line from Manissa, a good quantity of this coal has been mined on a small scale. Mining is carried on at Soma by methods which are probably not duplicated elsewhere. The inhabitants of this village and neighboring settlements merely come and help themselves to the quantity of lignite they need and on which they pay a small tax to the local representative of the government. Near Panderma, at about 4 km. from Tchamakdagh, some exploration work was undertaken about 20 years ago in the Mesepsif Valley. The following sequence was determined in this locality:

1. A basal igneous complex, the upper zone of which consisted of a highly siliceous flint-like band of rock averaging 2.5 m. in width.

2. Thick beds of marls with which lignite seams, varying in thickness between 2 m. and 0.86 m. were intercalated. The coal belonged to a high-grade lignitic variety. Samples of it gave the following analysis: Volatile matter, 32.6 per cent.; fixed carbon, 45.4 per cent.; ash, 12.6 per cent.

The Tertiary succession in this district is best revealed by the section afforded in the gorge of the Dovantzi River. The basal igneous is overlain by thick beds of tufa, above and lying conformably with which, clayey marl beds occur with lignite seams about 0.30 m. in thickness. This series underlies relatively thick strata of bituminous marls. The clay above or below the lignite-bearing strata can be easily detected by large blocks of jasper and chalcedony that stand out from its mass.

West of this region, lignite outcrops are known at Demirtash, 20 km. north of Brousa. Mining was also carried on formerly at Tchaidere near Bilejik and at Gueve near Sugud. The product from the last-named locality was used in the silk factories of Bilejik and Keupli. The most westerly extension of this Tertiary lignite is found in the mountains environing Tchai near Afiunkarahissar. Seams having an occasional

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thickness of 2 m. have been discovered here at an altitude of 1,840 m. lying over calcareous conglomerates and capped by clay beds. In the quaternary alluvial plain of the Mendere River south of Smyrna, lignite with a dip of 34 to 45° N. has been mined near Sokia station. The seams are interbedded with schistose strata and can be detected along a line 10 km. in length.

The southwestern coast of Asia Minor with its hinterland does not appear to be devoid of coal. Seams are known near Makri in the foothills of the Eljikdagh Mountain. In the Province of Smyrna, lignite beds exist at Milas, Aidin, Kiosk, Nazili, Scalanova and Keramos. The Nazili beds have been developed for local use. Practically every one of the other deposits is susceptible of development to supply local requirements.

2. *The Black Sea Basin.*—The presence of coal-producing measures has been recognized along the Black Sea coast between Heraclea and Ineboli, a distance of about 175 km. An economic province can be outlined in this district on the basis of the presence of a number of long and narrow parallel bands of carboniferous rocks which comprise[3] Culm, lower Carboniferous limestone and upper Carboniferous coal measures. These bands have a general N.70° E. trend. They are surrounded by Upper Cretaceous formations consisting of nodular calcareous beds, chiefly Caprotina limestone.

The unity of the geological sequence appears to have been established by paleontological[4] as well as lithological data. The strata can be classified as follows in ascending succession:

1. A basal Carboniferous limestone containing *productus giganteus*, *syringo-phyllum* and other polyps.

2. Coal-bearing measures containing *sphenopteris bœumleri*, *neuropteris gigantea* and *neuropteris schlehani*. Above and lying unconformably, the Cretaceous is represented in beds of great thickness by:

3. Grayish crystalline limestone with *requenia gryphoides* and *toncasia*.

4. A complex of fairly fine-grained, dark-colored, clayey and sandy strata.

The correlation of this sequence with the Cretaceous of the Balkans has been established.[5] The latter is itself the prolongation of the Alpine zone on which outcrops of Carboniferous strata appear as at Heraclea. This similarity in the geological relationships, prevailing on the southwestern coast of the Black Sea, and in a portion of central Europe, may have a wide significance which does not lie, however, within the scope of these notes.

Differentiation of the fossil flora found at Heraclea leads to a local sub-

[3] Edward Suess: *The Face of the Earth* (Sollas' translation), vol. 3, p. 319.

[4] H. Douville: *Comptes Rendus, Académie des Sciences*, 122, pp. 678 to 680, 1896.

[5] R. Zeiller: *Comptes Rendus, Académie des Sciences*, 120, p. 228, 1895.

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division into three stages corresponding to the Culm, Westphalian and Sephanian stages of the European Carboniferous. These are, respectively, the Aladjaaghzy or lower stage, the Cozloo or middle stage, and the Caradon or upper stage. In the Heraclea coal field the first two are economically the most important but the best coal is derived from the middle stage.

The Heraclea coal field is at present the best developed in the region, as well as in Turkey. It occupies a belt about 60 km. in length along the Asiatic coast of the Black Sea, between the seaports of Heraclea and Filios. The town of Heraclea itself lies at a distance of 135 nautical miles from Constantinople. The coal beds outcrop near the coast and are not known to extend farther than 8 km. inland. It is believed, however, that they strike southeasterly from the eastern boundary of the district to which mining operations have been confined and that they may be re-encountered at a greater distance from the seashore beyond. The coast facing this section of the Black Sea is precipitous. Narrow valleys and ridges parallel each other at right angles to the shore line. The elevation of the land immediately fronting the water does not exceed 60 m. The altitude increases gradually southward until a maximum of 500 m. is attained at a distance of about 3 km. from the coast. Here long chains of hills with a general E.-W. trend are met.

The western boundary of the basin is determined by a long fault-line occurring at Kiosseaghzi at a distance of about 10 km. east of Heraclea.[6] The entire measures appear to have sunk in depth at this point. Some coal is won here from narrow seams which can be correlated with the Cozloo stage. The valleys of Kiretchlik, Chaoushaghzy and Ilisoo appear immediately beyond. Within their limits the Aladjaaghzy series can be first observed. The Cozloo depression lies about 20 km. farther east and is the seat of the widest and most actively exploited veins of the entire district. From this last point the coal measures are detected about 2 km. farther east, to Domooz, where they assume a gradual southerly strike inland. Cutting across the Zoongooldak Valley, where their width attains 5 km., they extend beyond Tchatalaghzy Valley to Sooksoo. Mining ceases at this locality, but coal is known to occur as far east as Djide.

The Aladjaaghzy stage is best represented at the locality bearing that name. The strata appear first at about 0.5 km. west of the settlement. They have a general east-west strike and dip from 10 to 30° south. Fifteen seams of an average thickness of about 0.75 m. are distinguished by different names. Although they are considered locally as different beds, the profound faulting to which the region has been subjected, subsequent to the formation of the coal, tends to confirm belief in the identity of

[6] S. Stassinopoulos: *Bulletin, Chambre de Commerce Française*, Constantinople, October, 1908.

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many of the seams. The uniformity of their structure has helped the unmethodical native to carry on work with relative ease. The same measures have been developed, to a less degree, in the valley of Kiretchlik, as well as at Tchaoushaghzy.

A zone of transition between the Aladjaaghzy and the Cozloo measures is revealed by the presence of a number of almost vertical seams. Their thickness is the same as that of the seams below. They occur along a double east-west line of faulting, which also constitutes the southern extremity of the coal basin, since beyond it, to the south, only Carboniferous limestones are encountered.

These vertical seams therefore indicate the beginning of the Cozloo stage, which is best represented in the valleys of Zoongooldak, Kilimli and Tchatalaghzy. The strata in this zone dip respectively to the north and south so as to form an anticlinal fold which is plainly discernible in the valley of Kilimli. Twenty-five veins are distinguished locally, but the absence of any detailed geological survey of the district and the lack of maps preclude adequate differentiation. The seams have an average thickness of 1.5 m. The reserves of the Cozloo mines are estimated at 50,000,000 metric tons.

A possible zone of transition marks again the upper level of the Cozloo stage. It is best seen in the vicinity of the Kiosseaghzi fault. Two systems of seams occur here and are known as the Tsamly and Beylik groups. The former comprises fairly thick seams, some of which attain a maximum width of 2 m. Their dip is northerly, many being almost vertical. The Tsamly system can be traced to the very shore, at the village bearing that name. It extends over a distance exceeding 4 km. in length, to Candilly Valley.

The Caradon stage appears above this zone of transition. Eight seams are distinguished as belonging to this series. They have been worked at Caradon where they are well developed, as well as at Tchatalaghzy and Cozloo. Their thickness varies between 1 m. and 1.5 m. Beds of slate of an average width of 1.5 m. are intercalated between the seams. The Amasra basin, comprising the Tchatalaghzy district, also appears to form part of the same series. Five seams of similar average width are known here. It is also surmised that the outcrops east of the town of Amasra near Capoosoo constitute the prolongation of this group. In addition a number of outcrops observable in the valley of Ilisoo near Sefedler village probably bear the same relationship.

The coal mined at Heraclea belongs to the bituminous variety. It is slightly higher in ash than the corresponding average type from European basins. It may be divided into two classes: (1) that obtained from the veins of middle series, containing from 30 to 40 per cent. volatile matter; (2) that mined from the lowest stage, containing from 40 to 45 per cent. volatile matter. The first is excellent for coking purposes, while the

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Aladjaaghzy product is used chiefly in the manufacture of illuminating gas and for steam generation.

The following table shows the annual output for the entire region.

Annual Production of the Heraclea Coal Field

Years	Metric Tons
1884	70,997
1885	80,129
1886	88,892
1887	97,846
1888	109,409
1889	146,366
1890	137,282
1891	166,230
1892	168,727
1893	173,456
1894	159,687
1895	147,445
1896	166,170
1897	122,890
1898	211,514
1899	253,830
1900	390,428
1901	341,221
1902	364,206
1903	453,807
1904	518,874
1905	592,874
1906	610,400
1907	625,000
1908	650,000
1909	675,757
1910	
1911	750,000
1912	646,281
1913	566,118

Bituminous coal of excellent quality is known in the foothills of the mountains surrounding the seaport of Kidros on the Black Sea. This occurrence may be taken as forming the easterly end of the Black Sea coal basin. It cannot be doubted that this economic province is the seat of an important coal supply. The fuel has also been found in the vicinity of Changry south of this bituminous field. Inferior grades are reported from Cassabaibala in the vilayet of Angora where the discoveries consist mainly of lignites.

(*b*) Eastern Asia Minor

A large number of coal seams of all grades are known to occur in the region extending east of the 34th meridian east of Greenwich to the Russian and Persian boundaries. Lignite was reported by Cuinet[7] from

[7] *Turquie d'Asie*, vol. 1.

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Tavshandagh Mountain in the vicinity of Mersivan. The same observer noted coal outcrops in the immediate neighborhood of Tokat, as well as in a district lying 60 m. southeast of that town. Outcrops have also been detected at Cardashlar at 2 hr. distance from Sivas. Lignite is known at Ghemerek and at Tonus in the same district. Farther south lignites have been observed by travellers in the vicinity of the Argana copper mines. In the northern portion of Mamouretualziz province, coal is known in a band, which practically extends from its eastern to its western boundary. The localities are Chemishguedsek,[8] where the coal is stated to be of good quality, Zafranik, Dersim, and Derstek. At Shengyah also, near Baibourt, lignite occurs, as well as in the Myriam Dagh. The Chemishguedsek seams are conveniently situated close to the banks of the Menzurchai, an affluent of the Karasu or eastern Euphrates. They lie at about 60 km. northeast of the Keban silver-lead deposits south of the last-named town. In the Erzerum province some lignite has been mined to supply local requirements. The seams are generally imbedded in the Upper Eocene. They are well developed at Kheneke, near Migri in the valley of the Arax, as well as east, in the Kashkaldagh Mountain. Beds are known at Kasachan, Hortukderessi, Serishli near Bayazid road, Lezghiaf, and Ercovan.

The annual production of the more important localities is distributed as follows:

Production of the Erzerum District

	1910, Metric Tons	1911, Metric Tons
Kheneke (Narman)	1,300	450
Vartik (Terjan)	86	.....
Kukurtlu (Erzerum)	120	50
Sivishlu (Erzerum)	860	45
Tazegul (Erzerum)	.....	20
Charel (Erzerum)	.....	20

Coal has been mined at Rizeh north of Erzerum. Proceeding southward from Erzerum the occurrence of a deposit of bituminous coal is reported from the mountains lying west of the Mush plain. Southeast of the last-named locality a hard variety of coal resembling anthracite is found near Erooh in the province of Bitlis. The coal is stated to occur in abundance in this region between the villages of Tchemak and Dergal. The village of Komurhan, between Harpoot and Malatia, owes its name to the coal seams worked by the natives.

The same variety of coal is found farther east near Palu. The meager

[8] J. E. Spurr: *Engineering and Mining Journal*, vol. 74, pp. 308 and 438 (Oct. 4, 1902).

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reports obtainable from this relatively inaccessible town lack definiteness and merely suggest the possible existence of an anthracite basin in this district. The Palu coal outcrops at 25 km. from the Murad Su in the bottom of a valley. The fuel is used by blacksmiths of the region. It will probably acquire importance owing to its proximity to the copper mines at Argana.

West of the Persian boundary, the province of Van seems to have been favored by nature with ample reserves of fuel. At Bashkale, in the Hekiari district, a number of lignite seams 0.15 m. in width are known. Near Tcough Pass, at about two days' horseback ride from Nordooz, the same coal occurs again. The coal-bearing strata attain here a thickness of about 14 m. At Akchai, about 24 km. from the harbor of Cheraker on Lake Van, a bituminous variety was discovered. Coal is also known at Sivan near Archek, 27 km. east of Van, where a number of seams have been brought to light. At Chahmanis in the district of Chatak a number of seams occur. Their width occasionally attains 1 m.

The occurrence of the fuel is also reported from Beirootdagh Mountain, where iron has been mined since times immemorial by the natives of the town of Zeitun. This region has been, perhaps, least visited of all by travelers in Asia Minor. North of Sis, toward Hajin, anthracite float has been detected in the valley of the Seihoon.

(*c*) Syria

Two deposits of lignite are worked in the province of Beyrut. One is the Ainamade mine which lies near the village of Kermael and yields about 1,000 tons annually. The other occurs at Haitoora near Jezzim and produces about 500 tons per year. Outcrops have also been traced in the valley of the Nahrelkalb. Both deposits are capable of yielding larger outputs. In the Lebanon, coal that is probably lignitic is reported from Falooga near Hamana, as well as at Abdin near Bekfaya and Mreyjatt. The first-named locality was the seat of a native exploitation which was undertaken on a small scale to supply the silk factories of the district with fuel. Other Syrian localities from which coal is reported are Meshgara in the Beka's Mountains and Huleh, in the province of Damascus. The environs of Tyre and Safed are similarly credited. At about 80 km. southeast of Aleppo, coal outcrops have been noticed by various explorers. The accounts relative to their importance are conflicting. Two other localities regarding which very little is known, are Suedieh on the Mediterranean coast and the Ajiloon province in Palestine.

(*d*) Mesopotamia

Coal was mined about half a century ago in the northern section of this ancient province at Harput, about 45 km. east of Jezireh. The product

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was used for a while on river boats plying on the navigable sections of the Tigris. The fluvial transportation industry appears to have stimulated research for the fuel, since a little mining was also undertaken for the same purpose in the Jebel Hamrin 90 km. northeast of Bagdad. Coal has also been found in the vicinity of Mosul, but the deposit has not been worked.

At about 160 km. northeast of Bagdad a small amount of coal has been mined by the natives since 1890. Mining is confined to the low hills lying east of Kifri, near the Nasalah village. The coal is won from open cuts and shallow workings. Its quality was found to be poor at that level, and deeper digging has not yet been undertaken.

(e) Turkey in Europe

The environs of Keshan in the province of Adrianople have been prospected for coal somewhat more thoroughly than other districts of European Turkey. The beds are found at the edge of a synclinal sandstone basin, 13 km. in diameter. From a point 1.5 km. northeast of Keshan a single seam was traced[9] for a distance of 5 km. along the southern edge of this basin at about 165 m. above sea-level. The coal mined here is bituminous. One of the seams worked was about 1 m. thick and had the characteristics of cannel coal, burning with a long flame and emitting little smoke.

Its specific gravity was 1.37 and it yielded about 2 per cent. in ash. The section of a prospect bore-hole revealed the following sequence:

	Meters
Red, stony clay	10.35
Volcanic ash and rhyolitic breccias	14.85
Sandstone and dark-colored beds	2.00
Coal and bone	0.45
Shale and clay	1.65
Sandstone	1.70
Shale and clay	0.20
Coal	1.10
Sandstone	0.45

The evidence gathered underground showed that the coal rests on sandstone and is capped by a hard clay roof about 0.60 m. in thickness.

At Bosztepe, 9.6 km. west of Keshan, the same variety of hard cannel was discovered. In the opposite direction several seams are known along the Marmora coast between Sarkeui and Ganos, at a distance of about 48 km. southeast of Keshan. The thickness of these veins varies between 0.30 and 0.45 m.

The outcrops of another deposit, which contains seams about 0.70 m.

[9] T. English: *Quarterly Journal of the Geological Society*, 1902, p. 153 *et seq.*

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thick, are known. The coal seams are here interbedded with oil-bearing formations described below, with which they have a common southerly dip into the hill forming the southern slope of the Kavak River valley at about 130 m. above sea-level.

The prolongation of the Marmora-Ægean lignites can be followed in the vicinity of the seaport of Rodosto in the Sea of Marmora. Deposits of lignite are also known near the villages of Akbunar, Tchiflik and Agatchly at a short distance from Constantinople.

III. OIL AND ALLIED HYDROCARBONS

(a) Early Use of Oil and Bitumen in Turkey

The occurrence of hydrocarbons in Syria and Mesopotamia was known to the ancients to whom the products had commercial value. The "fountains" of Is—the modern Hit—were exploited by the Babylonians who used the bitumen as a mortar to bind the sun-dried bricks with which public and private buildings were constructed. This cementing substance is probably the "slime" of the old testament. Ruins of edifices, as well as statues, bear evidence today of the enduring qualities of this binder. It is believed that the bitumen was applied in the plastic state and then gradual induration took place by the evaporation of a portion of the bituminous oils.

The testimony of Herodotus on this point is instructive.[10] The father of history describes the construction of a wall for a moat surrounding Babylon for which hot bitumen was used throughout as cement. He states that, "The bitumen used in the work was brought to Babylon from the Is, a small stream which flows into the Euphrates at the point where the city of the same name stands, eight days journey from Babylon." He also adds that lumps of bitumen are found in great abundance in this river.

The binding property of bitumen was probably widely recognized by the Babylonians. Under the title of "Discoveries in Babylonia,"[11] T. G. Pinches alludes to the discovery of a Semitic head with ivory eyeballs inserted by means of bitumen in the eye sockets made to receive them.

The embalmers of Egypt consumed large quantities of bitumen in the process of mummification. The substance was imported chiefly from Mesopotamia although it was also obtained from Syria. It is interesting to note in this connection the derivation of the word mummy from the Coptic root "mum" signifying bitumen. Pliny describes[12] the Babylo-

[10] Book I, p. 179.
[11] *Rec. of the Past*, vol. 9, No. 11, 1910.
[12] Book II, cx.

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nian plains as presenting the aspect of a flaming basin fully one *jugere*[13] in extent. Greek writers refer to the hydrocarbons of Mesopotamia and Strabo cites Eratosthenes when alluding to the abundance of asphalt in Babylonia,[14] as well as to its uses as a mortar and in calking rafts and river boats.

In the old civilization of the Mesopotamia basin, bitumen was used in ornamentation. Alabaster vases inlaid with bitumen spread in fanciful geometrical designs were recovered from the ruins of Ur-Engur's temple.[15] Pottery marked with characters in bitumen was found in nearby localities.[16]

The recognition of the value of bitumen as a commodity of commercial value is attested by the fact that part of the tribute paid to Thothmes III at Karnak from Mesopotamian cities consisted of 2,040 minæ[17] of "sift" bitumen sent by the chief of "Ist."[18] Today the substance is known by the name of "zift," not only in Mesopotamia but throughout Turkey.

Later proof of the value of these hydrocarbon deposits to the ancients was well set forth in the year 311 B.C. when Antigonus sent Hieronymus of Cardia with an army for the purpose of taking the asphalt works of the Dead Sea out of the hands of the Nabatei.[19] These dwellers of the Syrian and Arabian frontiers repelled the invaders and continued their exploitation.

The description of Hit by an English traveller who probably visited the locality at the close of the 17th or the beginning of the 18th century has been recorded by Rawlinson.[20] The text runs as follows:

"Having spent three days and better, from the ruins of old Babylon we came unto a town called Ait, inhabited only by Arabians but very ruinous. Near unto which town is a valley of pitch very marvellous to behold, and a thing almost incredible wherein are many springs throwing out abundantly a kind of black substance like unto tar and pitch which serveth all the countries thereabouts to make staunch their bark and boats. Every one of which springs maketh a noise like a smith's forge in puffing and blowing out the matter, which never ceaseth night or day, and, the noise is heard a mile off, swallowing all the weighty things that come upon it. The Moors call it 'the mouth of Hell.'"[20]

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[13] 0.62 Acre.

[14] Book XVI, 15.

[15] E. J. Banks: *Bismya*, p. 270.

[16] The reader is referred to the work cited immediately above for description of other objects in which bitumen was used.

[17] About 150 lb.

[18] Rawlinson's *History of Herodotus*, New York, 1859, vol. 1, p. 245.

[19] E. Reich: *Atlas Antiquus*, text facing Map 18, *loc. cit. supra.*

[20] Passage taken from *Collection of Voyages and Travels* from the Library of the East of Oxford, 2 volumes, London, 1745, vol. 2, p. 752.

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(*b*) Syria

Indications of oil in the Yarmuk River valley southeast of Lake Tiberias had always been reported to foreigners by natives. In 1912, oil-drilling was started near Makarim on the Hedjaz Railroad, by the Syrian Exploration Co. The locality lies in the heart of a desert region of contact between preglacial lavas and Upper Cretaceous rocks. It is easily attained from Haifa by rail across the plain of Esdraelon and the Jordan River valley. Drilling is somewhat hampered by the necessity of having to send to Haifa whenever parts of the machinery break. Fevers which prevail in the region and to which the natives themselves do not appear to be immune also constitute drawbacks. At the time of writing,[21] drilling had attained a depth of 600 ft. with a noticeable increase in the smell of petroleum. It is expected that the plan will be encountered beyond 1,000 ft. Both the rotary and cable systems are used in drilling, although the hardness of the rocks is in favor of the use of the latter method. The geological column in this locality is represented by limestone at the surface. Sandstone occurs below and in the first 300 ft. It is followed by hard slates at about 450 ft. Other Syrian localities from which petroleum is reported are the Ajiloon, Haraun, and El Kork districts.

(*c*) Mesopotamia

The oil fields of Mesopotamia lie in the area of transition between the plateau of Iran and the Mesopotamian depression. The occurrences of hydrocarbons are encountered north of latitude 30 N. in the valleys of the lower Euphrates, Tigris and Karun Rivers. Thus defined they fall within the three Turkish vilayets of Mosul, Bagdad and Busra and in the Persian province of Shushistan (also known as Arabistan). The liquid fuel appears to exist also in the mountainous region lying northeast of Mesopotamia as far as the Black Sea coast.

The oil industry is in a higher stage of development within the Persian area than in Turkey. Nevertheless, the Turkish field is considerably larger than the Persian, which is restricted to the valley of the Karun, the only navigable river in Persia and one which cannot be compared in size to the Tigris or Euphrates.

The oil-bearing lands extend in a general NW.–SE. band parallel to a line of Tertiary folds rising on its eastern border. Here, as elsewhere in Asia, oil is found at the depressed extremities of mountain chains wherever the anticlinal slopes descend to the level and proximity of the sea.[22]

This elongated basin abuts against well-defined limestone ridges which constitute the westernmost outliers of the Kurdish and Pusht-i-Koh Mountains. These successive walls of limestone begin northeast of

[21] June, 1914.

[22] Dr. Launay: *Géologie et Richesse, Minérale de l'Asie*, p. 444.

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Mosul[23] and are prolonged with a southeasterly trend beyond Alkosh and Sheik Adi. A range of the same connects Girdamanik on the Great Zab with Keui on the Lesser Zab. The Kara-dagh west of the Suleymanieh district forms the watershed between the last-mentioned river and the Dyala. Proceeding farther west, the traveler attains the lowland area which comprises the towns of Kasr-i-Shiru and Zohab.

West of the Pusht-i-Koh uplift the limestone ridges are replaced by a line of sandstone hills of lower elevation which extend along a broad and undulating country formed by conglomerates and characterizing the environs of Mosul. The Kararchak and Baravan Mountains are types of these isolated sandstone ridges rising above a plain of conglomerates.[24] The Jebel Hamrin, which rises to a height of 600 ft. north of Mendali and merges in the Tigris plains beyond Kala-Serghat, also consists of sandstones and conglomerates. These different rocks disappear below Hit and Tekrit where the great alluvial plain of the lower Tigris and Euphrates begins. This deltaic area extends to the Persian Gulf.

The oil generally occurs in Miocene rocks, which consist of brightly colored sandstones, marls and limestones permeated with salt. As a rule, the petroleum and bitumen basins are found in soft whitish limestones. Veins and seams of gypsum due to epigenetic modifications of carbonate of lime in the presence of abundant sulphur and sulphurous waters are frequent. The usual accompaniment of sulphurous springs, often thermal, can be observed. The natives attribute curative properties to these sulphurous manifestations.

The northernmost of these Mesopotamian valley occurrences is found at Harbol in the Caza of Sakho, 60 miles north of Mosul within the easternmost extension of a patch of Eocene country. Thence southward to the shores of the Persian Gulf, natural oil pits are numerous.

At Hamman Ali, 15 miles south of Mosul, a number of small basins are found at a distance of half a mile from the right bank of the Tigris. Here petroleum can be collected by skimming the pools, while threads of ceaselessly oozing bitumen can likewise be obtained.

To the southwest of Alijak, on the Great Zab, an elongated pit about 120 m. in length contains petroleum on the surface of its waters. The oil at this locality is reputed to be poor in quality and the natives make no attempt to gather it. Springs are also reported immediately north of Tell Yazer, on the Chorr Derrah River east of Nimroud.

Proceeding southward, the important wells of Gayara are attained. The supply of oil at this point is believed to be considerable. The fuel flows from numerous sources, forming oil lakelets which attain 200 ft. in diameter. The quality of the yield is stated to be excellent, the oil being particularly free from sulphur and brine. Thick deposits of asphalt are

[23] F. R. Maunsell: *Geographical Journal*, May, 1897.
[24] F. R. Maunsell: *Loc. cit.*

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also found in this locality. The southern end of this deposit reaches El Fatha Defile, where the Tigris passes through Djebel Hamrin. Here bitumen exudes abundantly in long threads which darken the whitish-colored limestone cliffs. The heavy flow of oil throughout the Gayara district has enabled the natives to create a refining industry which, although primitive, satisfies local needs.

West of Gayara lie the ruins of ancient Hatrae, known to the Arab dwellers of the region by the name of El Hadr. Here a number of unexploited oil springs are met in the vicinity of the settlement.

At Baba Gurgur in the low sandstone and conglomerate ridges surrounding the town of Kerkuk an area of about 50 acres constitutes the site of primitive operations by the natives. The oil collected here is derived from pits dug to a depth of about 4 m. It is shipped to Bagdad and all of eastern Mesopotamia. This locality is noted for the abundance of natural gases escaping from the vicinity of the springs. It owes its Arabic name of "father of murmurs" to this phenomenon. During 1901 a fire which spread to the entire field of work caused the suspension of the small native industry. Refining of the oil is also undertaken here by the natives. From a technical standpoint this district is interesting chiefly on account of the presence of alabaster and salt, the association of which with hydrocarbons has been noted in Sicily.

High-grade petroleum is derived at Guil. Although crude methods of refining are in vogue at this locality, the burning qualities of the final product is such as to lead the natives to prefer it to imported brands. This does not appear to be the case at Nimroud, however, where the oil is impure and almost black in color throughout a band of 800 m. in length, from which it flows into the river.

A rich field is found at Tuz-Khurmatli, where a heavy flow of oil can be observed for a distance of over 1 mile along the right bank of an affluent of the Shatl-el-Adhaim.

Deposits of asphalt attaining a total depth of 4 to 5 m. are found at this point. In spite of the quantity of the oil, the consumption is limited to local needs and occasional unimportant shipments in sheepskin bags to neighboring villages. The annual net profit derived by the native proprietors of this basin is about $1,000.[25]

To the southeast of this favored locality the springs of Kifri appear at a distance of about 2 miles east of the settlement and at the foot of Neft Dagh (Naphta Mountain). Here again an asphalt deposit about 6 ft. in thickness surrounds the springs.

Flows of minor intensity yield their product in the Jebel Oniki Imam and at Zahru on an island in the Chabur district. The last-named locality is credited with abundant reserves, upward of 30 oil rivulets having been counted along a river front 8 miles long.

[25] *The Near East*, March 20, 1914, p. 645.

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In the vilayet of Bagdad the environs of Hit have been exploited for oil and bitumen by the natives since times immemorial. The occurrences are found south of the village between two wadys: The Kubessah and Mohammedieh.[26] Two of the largest springs are seen at about 1 mile's distance from the town.[27] They present the appearance of circular ponds from which naphtha bubbles up with gas. Shallow reservoirs are also provided for the water, the evaporation of which leaves salt, which is a valuable product to the natives. The alluvial plain is dotted in this locality with gray buttes from the base of which asphalt is emitted at a temperature of about 25° C. The viscous product trickles slowly toward the river over previously deposited layers of the same substance. The oil flowing from the natural wells in this region is accompanied by intensely saline sulphurous waters. The same phenomena can be observed at Ramadieh about 20 m. farther south. At Naphata, in the same district, the quality of the oil deteriorates, according to the testimony of the natives. Hit is also the center of a native boat-building industry, which provides coracles and rafts for fluviatile navigation. Bitumen is used extensively for calking these vessels. The shipwrights buy it from the gatherers in the form of small cakes which the Turks call "Karasakiz" and the Arabs "Jir." It is applied hot after having been melted over a fire made in the ground at close proximity to the primitive shipyards and after having been reduced to proper consistency by admixture of sand and earth.[28]

Perhaps the most important of the Mesopotamian basins is found at Mendali at a short distance northwest of the town. Observers concur in admitting that copious flows are noticeable in this locality. A dozen springs from which oil is emitted with considerable force exist here. The high-grade character of the petroleum is revealed by the refined product, which has excellent burning qualities in spite of the crude process by which it has been obtained by the Arabs. The wells were worked in 1888 and the product transported to Bagdad in skins on camels. A certain quantity of the product was exported to India for consumption by paint dealers in the preparation of their colors. The oil was used for lighting purposes in its crude state until superseded by refined foreign products. At present oil is refined by the natives at Mendali according to the methods used at Gayara. In addition to the foregoing localities, oil is reported from Zacho, Saad and Doyat in the vilayet of Mosul, as well as from Darondieh, Kizil-Rubati, Yakouba Ana and El-Haim in the vilayet of Bagdad. Ainsworth also records a site of oil springs in his "Table of Sites on the River Euphrates,"[29] as occurring between Shifat and Deir, north of the point of confluence of the Khabur River. It might be added here that the Mesopotamian oil question was a "burning" one during the

[26] The Reclus: *Géographie Universelle*, vol. 9, p. 389.

[27] W. F. Ainsworth: *A Personal Narrative of the Euphrates Expedition*, vol. 2, p. 440.

[28] F. R. Chesney: *Narrative of the Euphrates Expedition*, p. 78.

[29] *The Euphrates Expedition*, vol. 2, Appendix, p. 385.

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work of revising the Turco-Persian frontier in 1913. As a result of agreements reached by the two countries involved, the springs at Chiarsukh are now in Turkish territory,[30] as well as at Kasr-i-Shirin in Persia.

The Chiarsukh springs occur at the foot of the Koh-i-Ahengeran, along an anticlinal fold which fringes the Chiarsukh River. The oil flows accompanied by brine and ozokerite from sandstone strata underlying marls. An insignificant native industry is based on the existence of the fuel. It is proposed to pipe the oil from this area to Bagdad.

At Kasr-i-Shirin a small English refinery is operated. Its output supplies the requirements of the Kermanshah and Hamadan trade.

The Persian wells are encountered at Shuster, whence the product is collected at a tank station south of the town on the Karun River. A pipe from this point carries the flow past Mohammerah to the Abadan refinery. Oil is also obtained at Ram Hormuz, southwest of Shuster.

The process of refining in the Turkish districts is in its rudimentary stage. The kerosene is collected from stills of the well-known alchemist's alambic type resting on a rough earthen stove about 4 ft. high. The annual output of the Gayara stills consists of from 2,000 to 2,500 4-gal. cans.[31] It is used principally for oil engines, American and Russian oil being employed for lighting purposes. The Gayara petroleum is sold in Mosul at 2 pt. (10 c.) a gallon retail.

At Abadan on the Shatl-el-Arab River, in Persian territory, and at a distance of about 50 miles south of the Turkish seaport of Basra, a small refinery was put in operation at the end of 1912 by the Anglo-Persian Oil Co.[32] Both at the refineries and the other works of the oil company, the Arabs have shown little ability for mechanical tasks. The driving of engines and in general the performance of tasks requiring skilled knowledge is entrusted to Kurds and Lurs.[33] The construction of this refinery was due to the discovery of oil near Ahwaz some 140 miles inland. A pipe line now connects the two localities. The daily capacity of the refinery is estimated at 6,000 4-gal. cans. Gasoline and naphtha can also be produced at the plant.

The Mesopotamian oil field should not be regarded as an isolated basin. There is reason to believe that it is merely a part of a vast oil-bearing belt, which can be traced in Burmah, the Mekran and Quatta, whence it proceeds to the Persian Gulf and is prolonged northward through Mesopotamia and Armenia to the highly developed Russian districts of the Caspian Sea. The occurrences described above constitute

[30] *The Weekly Times*, June 26, 1914, p. 513.

[31] *Daily Consular and Trade Report*, Nov. 1, 1913, p. 587.

[32] This company is capitalized at $10,000,000 and is credited with being associated with the Burmah Oil Co., which bought the rights of W. K. d'Arcy, who obtained an oil concession in 1901.

[33] *Diplomatic and Consular Report*, No. 5264, Am. Ser. (1914).

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sufficient indications of the probable existence of considerable quantities of the liquid fuel. Nevertheless, abundance of flow at the surface cannot be considered as evidence of vast subterranean reserve. The data of experience culled in the exploited fields tends to prove that steady flow is to be preferred to copious jets. The intensity of folding within the fringe of Tertiary rocks lying east of the oil region might perhaps provide criteria of economic value. At all events, it yet remains to be seen whether the maximum yield will be derived from the Mesopotamian Valley itself or the petroliferous region extending farther north.

During the past few years American and Russian exporters of oil have built great hopes on the Mesopotamian market. One of the consequences of the important reclamation work undertaken by the Turkish government in the region has been that a large number of oil engines and pumps have been imported into the district for irrigation purposes. By the end of 1913, 500 engines averaging 10 hp. were operated in southern Mesopotamia. Five- to seven-inch centrifugal pumps and 7½-hp. kerosene engines are in constant demand for irrigation purposes. The crops raised on the lands irrigated by these engines are valued at present at $1,500,000 annually. It is believed that the employment of these engines will increase, and that the imports of oil into the district will also be heavier. At present about 100,000 cases of petroleum are imported annually from the United States.

The construction of the Bagdad railway, which of late has proceeded very satisfactorily, will undoubtedly stimulate the development of the region. The line traverses the eastern part of the basin. Trials have been made in recent years to use oil as fuel on German railroads and it is stated that the results have been satisfactory enough to induce the railway company to build large storage tanks.

The growing use of oil as a fuel and its actual replacement of coal in many regions—the entire Pacific Coast from Alaska to Peru is a notable instance—attaches more than ordinary interest to the Mesopotamian field. As a power generator oil tends to supplant coal on sea, the present tendency being to increase the number and size of ships operated by internal combustion engines. The "Wotan," a motor ship equipped with a 2,000-b.hp., 2-cycle Diesel motor, with tank space for 6,780 tons of oil, besides 900 tons of bunker oil and 100 tons of water, could recently be seen in New York harbor.[34] By the year 1912, one-third of the ships in the active list of the British navy were run either entirely by fuel oil or supplementarily to coal. The problems involved in the safe storage and thorough atomization of the oil appear to have been satisfactorily solved.

As far as can be ascertained, all the Mesopotamian oil occurrences are found on state lands, with the exception of the Tuz-Khurmatli field. Previous to 1908, the entire region had passed into Ex-Sultan Abdul-

[34] *Dun's Review*, July, 1914, p. 88.

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Hamid's hands. But with the fall of this monarch, his property reverted to the state and the right to operate in the oil field is obtained by the acquisition of concessions.

(*d*) European Turkey

Oil exists in the Tertiary formations north of the peninsula of Gallipoli. The district was visited and described by Lieut.-Col. T. English.[35] This observer states that oil was obtained in fair quantity from borings about 300 ft. deep at a distance of 40 miles southeast of Keshan, close to the junction of Deli Osman River and Milos Brook. The locality is situated at 320 ft. above sea-level. The section of the bore-hole was as follows:

	Feet
Yellow loam and boulders	35.6
Gray marls	4.6
Gray sandy limestone	1.0
Blue and red marls	12.0
Hard gray limestone	0.6
Gray marls	4.6
Chalky limestone	0.6
Clay	0.6
Limestone	0.6
Marls	11.0
Soft calcareous conglomerate	2.0
Marls	18.0
Sandy marls	4.0
Hard limestone	0.4
Sandy marls	11.8
Blue and red marls	32.0
Running sand with brackish water	3.0
Yellow clay	4.0
Blue and red yellow marls	30.3
Cream-colored limestone	0.3
Blue and yellow marls	47.0
Oil	
Limestone	0.6
Marls	15.6
Hard limestone	0.9
Marls	15.0
Sandy marls	28.0
Oil	
Yellow clays	0.6
Gray marls	....

The flow of oil was accompanied by excessively saline waters. Its specific gravity was determined at 0.825 and its paraffin contents at 10 per cent.

The oil-bearing formations extend for a distance of 15 m. between Sarkeui and Ganos and attain a thickness of about 1,000 ft. A small variety of *Melanopsis* was found in them. Northeast of the exploratory drill holes, the same rock series occur as far as the village of Milos, where they are capped by conglomerates containing *Planorbis* and *Cyrena*.

[35] *Quarterly Journal of the Geological Society*, vol. 58, p. 157 (1902).

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Potash as a Byproduct from the Blast Furnace

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SINCE the outbreak of the European war, few problems of raw-material supply have commanded more nation-wide attention than potash. It is well known that before the war the domestic production of potash was an insignificant percentage of the imports. The average annual importation of raw potash salts for several years prior to 1914 was slightly over 300,000 net tons, and of other potash manure salts about 700,000 tons. The sudden and almost total cessation of these imports created a unique and stringent situation. Methods of recovering potash from feldspar and other native mineral sources, and reclamation from hitherto waste products, have received a marked impetus. Soaring prices have been a keen incentive to research and industrial development.

Although several brief articles concerning the possibilities of potash recovery have appeared in recent trade journals, it still may be a matter of some surprise to the average technical mind that potash might be reclaimed as a profitable byproduct in the manufacture of pig iron. At the present time it may be of special interest to present some data, largely of a technical nature, on this subject.

A fairly thorough search of the literature reveals a number of articles concerning salts of the alkali metals in the blast furnace. Many of them deal with the theoretical rôle of alkali cyanides in the furnace. Several discuss the probable effect of the alkalies on the furnace brickwork. Two or three ambitious patents have been granted for reclaiming potash or other products from blast-furnace gas. However, we have heard nothing further as to the practical application of these patents. Little literature of importance has appeared during the last 10 years concerning alkalies in blast-furnace practice, and I have discovered no record whatsoever as to the actual sale or commercial disposal of flue dust for its potash content until this was inaugurated in our plant at Bethlehem. A bibliography of the most important articles discovered is appended.

About 4 years ago, in the course of investigating blast-furnace stove efficiencies, I analyzed the fine, yellowish fume of which a considerable

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quantity was removed from the bottom of the stove checkerwork. The sample was found to contain, among other constituents, about 15 per cent. water-soluble potash, which was somewhat surprising. This induced an investigation, which showed that considerable quantities of this material, hitherto a waste product, could be recovered from our stoves and gas-fired boiler settings. A search for a market was made without immediately satisfactory results. One fertilizer dealer claimed that the alumina content was too high. Others wished to make practical tests, and we furnished large samples for a full season's demonstration, satisfactory results being reported. But the pre-war-time prices offered hardly seemed to justify the trouble of reclaiming the dust.

With the beginning of the war and the subsequent spectacular rise in the potash market, conditions were changed. Knowing just what dust to recover, reclamation was immediately commenced, a satisfactory contract was negotiated, and this company has been disposing of the dust at a good profit ever since.

Having introduced the subject thus far in the same order in which it was initially investigated, we may now turn logically to an inquiry as to the source of potash in the raw materials charged, behavior in the furnace, and methods for recovering it as a byproduct, or of eliminating it as a nuisance. As a matter of both scientific and practical interest, a consideration of sodium will also be attempted, though not so thoroughly as its more important kindred metal, potassium.

Occurrence of Alkalies in Raw Materials

Potash doubtless occurs almost entirely in some form of feldspar or clay in all the materials entering the blast furnace.

At Bethlehem we receive ores from many quarters of the globe, and an attempt was made to discover whether the relative percentages of alkalies in the various ores bear any special relation to the source, mineralogical nature, or to the silica and alumina contents of these ores. No striking relation was discovered, except that the manganese ores, from widely separated sources, were found to contain relatively high percentages of potash as compared to most iron ores. Iron ores of this country, including those of the South, containing upward of 1 per cent. potash, or over, seem to be restricted to small areas.

The percentage of potash in different varieties of limestone and dolomite used as flux, varies greatly, largely on account of intermixed clay, and may be surprisingly high. Per unit weight, the potash content of our flux charge at Bethlehem is considerably higher than in either the average ore or coke charge.

Few data are available on the alkali content of coke from different localities. Various standard Connellsville cokes which we have examined

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have shown a much lower alkali content than our local byproduct coke made from West Virginia coals.

It may be a matter of some surprise to note that, with one exception, the soda contents of all the iron ores, the coke and the flux, subsequently listed, are much higher than the potash contents.

Before presenting any analyses, it may be mentioned that the difficult analytical determinations of potash and soda, and the complete analyses of the various miscellaneous samples, were executed almost entirely by two expert analysts, who literally spent several weeks in this work. Most of the determinations were duplicated to insure their accuracy.

Table 1 shows analyses representing 3 months' shipments in 1916, including hundreds or thousands of car loads of each variety, of the chief materials charged into our blast furnaces. Besides the alkali metals, percentages of silica and alumina are given, also iron contents of the iron ores, all analyses being expressed on the "natural" basis.

TABLE 1.—*Analyses of Materials of Blast-furnace Charge*

Materials	Kind	Source	Fe	SiO_2	Al_2O_3	K_2O	Na_2O
Iron Ores:							
Tofo	Magnetite	Chile	65.0	2.7	0.9	0.21	0.48
Juragua	Magnetite	S. Coast Cuba	54.5	13.0	2.7	0.29	0.77
Mayari	Limonite	N. Coast Cuba	38.0	2.0	10.0	0.06	0.25
Swedish	Magnetite	Northern Sweden	66.0	2.8	1.1	0.27	0.82
Port Henry Conc.	Magnetite	Northern New York	63.5	4.4	1.8	0.28	0.68
Harmony Cobbed	Magnetite	Northern New York	63.0	10.0	3.4	0.25	0.55
Barton Hill	Magnetite	Northern New York	64.0	8.3	1.9	0.22	0.77
Cheever	Magnetite	Northern New York	59.5	11.4	1.0	0.21	0.92
Sterling	Magnetite	Southern New York	55.0	8.6	2.0	0.33	0.95
Manganate	Hematite	Lake Menominee	45.5	7.2	3.3	0.24	0.47
Norman	Hematite	Lake Gogebic	55.5	5.4	1.5	0.29	0.32
Eureka	Hematite	Lake Gogebic	54.5	7.1	1.5	0.26	0.35
Mary	Hematite	Lake Marquette	50.5	7.8	2.5	0.28	0.55
Manganese Ores:							
Brazilian	.....	Brazil	....	7.5	3.0	0.88	0.21
Cuban	.....	S. Coast Cuba	....	....	....	1.12	0.77
Oriental	.....	India	....	....	....	1.68	0.19
Fuel:							
Lehigh byproduct coke	.....	West Virginia Coals	....	5.1	3.4	0.28	0.30
Flux:							
McAfee limestone	.....	Northern New Jersey	....	2.5	0.9	0.36	0.70
Average dolomite	.....	Bethlehem Vicinity	....	3.5	1.5	0.82	0.97

Two or three of the above ores are mixtures of magnetite and hematite, with the former predominating.

In the course of this investigation, samples of slag and flue dust from all of our Bethlehem furnaces were collected continuously over a period of 4 weeks during June and July, 1916, and analyzed for the alkalies. The

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analyses in Table 1 of ores, fuel and flux, are fairly representative of the materials charged during this period. The same coke and stone (half limestone and half dolomite) are used in all the furnaces. The percentages of potash and soda in the ore mixture charged in each furnace during the above period are as follows:

Furnace	A	C	D	E	F	G
K_2O in ore mixture, per cent.....	0.28	0.27	0.28	0.29	0.28	0.28
Na_2O in ore mixture, per cent....	0.63	0.43	0.65	0.50	0.51	0.63

From the foregoing data, and from known weights of materials charged, Table 2 has been constructed, showing the weights of potash and soda charged per ton of pig iron produced.

TABLE 2

	Potash (K_2O)			Soda (Na_2O)		
	Gross Ton	Pounds	Per Cent. of Total Contributed	Gross Ton	Pounds	Per Cent. of Total Contributed
Average weight in ore mixture, all furnaces, per ton pig	0.0046	10.3	46	0.0092	20.6	58
Average weight in coke all furnaces, per ton pig	0.0026	5.8	26	0.0028	6.3	18
Average weight in stone, all furnaces, per ton pig	0.0028	6.3	28	0.0039	8.7	24
Total.....	0.0100	22.4	100	0.0159	35.6	100
Ratio potash to soda charged.....	1:1.6					

The ratio of potash to soda is considerably higher in the coke than in the ore and stone. From Table 2 it will be seen that for each ton of pig iron produced, nearly 60 lb. of the alkali oxides are charged, constituents which certainly are of considerable importance in the working of the furnace, yet of which relatively little cognizance has been taken in the past.

However, due largely to the relatively high percentage of alkalies in our fuel and flux, and on account of the considerable quantity of alkali-bearing flue dust removed, it is my opinion that more potash and soda are charged into our blast furnaces, per unit of iron produced, than in any other large plant in this country.

Action of Alkalies Within the Furnace

It is probable that a considerable part of the potash and soda charged into a blast furnace is evolved from the top by direct volatilization or

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heat decomposition, though alteration by chemical reaction of the alkaline salts or compounds liberated may occur before they have left the furnace. It is certain that a large part of the alkalies is carried down into the hotter zones of the furnace and converted into cyanides by reaction with red-hot carbon. Some investigators have attributed an appreciable part of their ore reduction to the action of cyanides, inferring that after being oxidized and driven to the cooler upper portion of the furnace, they condense and are again carried down into the reducing zone. Whatever the action, it is self-evident that eventually the same amount of alkalies must be carried out of a furnace that is charged, and this takes place through the following avenues of escape:

1. *In Chemical Combination in the Slag.*—The literature is almost barren with reference to the presence of alkalies in blast-furnace slag. On account of the readiness with which the compounds of these metals are sublimed and carried out of the furnace with the gas, it might be inferred that only a negligible proportion would be found in the slag. This is far from the truth. Average samples from each of six furnaces in operation for two bi-weekly periods were carefully analyzed for potash and soda, with the results shown in Table 3. The slags were all normal for our practice, averaging about 35 per cent. silica, 13 per cent. alumina, 13 per cent. magnesia and the remainder lime, etc. In order to give an idea as to the hearth temperatures, the average silicon contents of the pig iron produced over the same periods are also tabulated.

TABLE 3

Furnace	Period Included, 1916	Per Cent. Alkalies in Slag		Per Cent. Silicon in Pig Iron
		K_2O	Na_2O	
A	9/25 to 7/8	0.48	0.96	1.07
A	7/6 to 7/22	0.38	0.70	1.06
C	9/25 to 7/8	0.17	0.22	1.62
C	7/6 to 7/22	0.18	0.22	1.70
D	9/25 to 7/8	0.42	0.36	1.22
D	7/6 to 7/22	0.40	0.40	1.20
E	9/25 to 7/8	0.56	0.60	1.50
E	7/6 to 7/22	0.65	0.66	1.35
F	9/25 to 7/8	0.29	0.36	1.27
F	7/6 to 7/22	0.40	0.64	1.27
G	9/25 to 7/8	0.34	0.68	1.27
G	7/6 to 7/22	0.35	0.72	1.05
Average for all furnaces, first period		0.38	0.53	1.32
Average for all furnaces, second period		0.39	0.56	1.29
Average ratio potash to soda in slag				1:1.4

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The calculated weight of slag produced during this period was about 0.52 ton per ton of pig iron produced, which would account for a loss of alkalies as follows:

	Pounds	Per Cent. of Total Charged
Average potash in slag per ton pig	4.5	20
Average soda in slag per ton pig	6.3	17

The percentage loss of the two alkalies in the slag is seen to be about the same, being somewhat greater in the case of potash.

In the case of "C" furnace, almost the same weight of alkalies was charged and the same weight of slag was produced per ton of iron, as at the other furnaces, yet it will be noted that the percentages of alkalies in the slag is much lower. This probably may be attributed to the higher hearth temperatures, as indicated by the higher silicon iron produced. However, I am unable to generalize this theory because further data are lacking at present.

On old blast-furnace slag dumps, deposits of a yellowish or almost white substance frequently may be found in sheltered crevices or small grottoes, into which water has percolated through overlying masses of slag, thus becoming more or less charged with soluble salts. The substance left by evaporation may be in the form of stalactites, thin acicular crystals, or of a dry powder. Considerable quantities of this material may be seen at Bethlehem where portions of our slag dump are being removed for concreting and other work. Two representative samples, one of light-yellow material, and the other nearly pure white, were obtained and, after being dried, were found to have the following proximate composition:

Sample	SiO_2	$Fe_2O_3+Al_2O_3$	MnO	CaO	MgO	K_2O	Na_2O	Total Sulphur	Sulphide Sulphur	Cl
White	0.20	0.36	0.04	26.47	None	8.66	4.27	38.3	14.5	trace
Yellow	0.38	0.13	trace	17.60	None	17.64	8.76	37.6	25.6	trace

The above analyses are interesting and instructive. We note the presence of nearly twice as much potash as soda in these samples, whereas in the raw slag the figures are almost reversed. The simultaneous presence of large amounts of potash, soda and sulphur indicate that, to a limited extent, the alkalies are efficient desulphurizers in the blast furnace.

Although not directly connected with the subject of this paper, I wish to call attention in passing to the apparent significance of the relatively

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large percentage of lime and sulphur, and the absence of magnesia in the above leachings. We know that practically without exception magnesium salts are more soluble in water than the corresponding salts of calcium. Our flux at Bethlehem for years has consisted of half limestone and half dolomite; as previously stated, the magnesia content of the slags averages about 13 per cent. The above facts furnish new evidence, so far as can be ascertained, as to the superiority of lime over magnesia as a desulphurizing agent in the blast furnace. Further corroborative tests were made by subjecting fresh, powdered samples of slag to 24-hr.

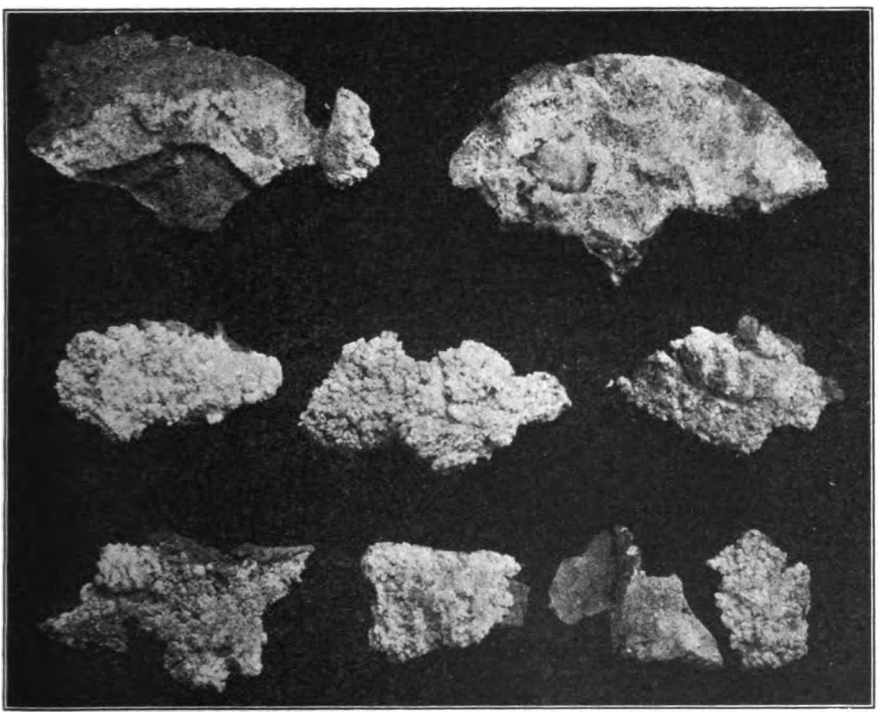

FIG. 1.—WHITE CRYSTALS FROM COOLING PLATES ABOVE MANTEL.

leaching tests in hot and cold water. In every case, appreciable amounts of sulphur (in both sulphide and sulphate state) and lime, were found in about equivalent ratio for combination, and never more than a trace of magnesia.

2. *As Cyanide or Other Volatile or Inflammable Compound through the Iron and Cinder Notches.*—Part of the fume arising from the molten iron, and especially from the slag running from the furnace, is undoubtedly alkali compounds. I have often noticed a peculiar lavender or violet flame around the iron and cinder notches during casting or flushing, which I believe is due to some alkali salt or salts burning. The fume

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arising from the iron and cinder runners certainly contains a considerable percentage of alkalies.

3. *By Liquid Exudation or Deposition from Gas Around the Tuyères, Coolers, Mantel and Cooling Plates.*—Occasionally while removing a tuyère or cooler, a stream of liquid "cyanide," resembling water, will run out of the furnace, or can be seen exuding from the lining.

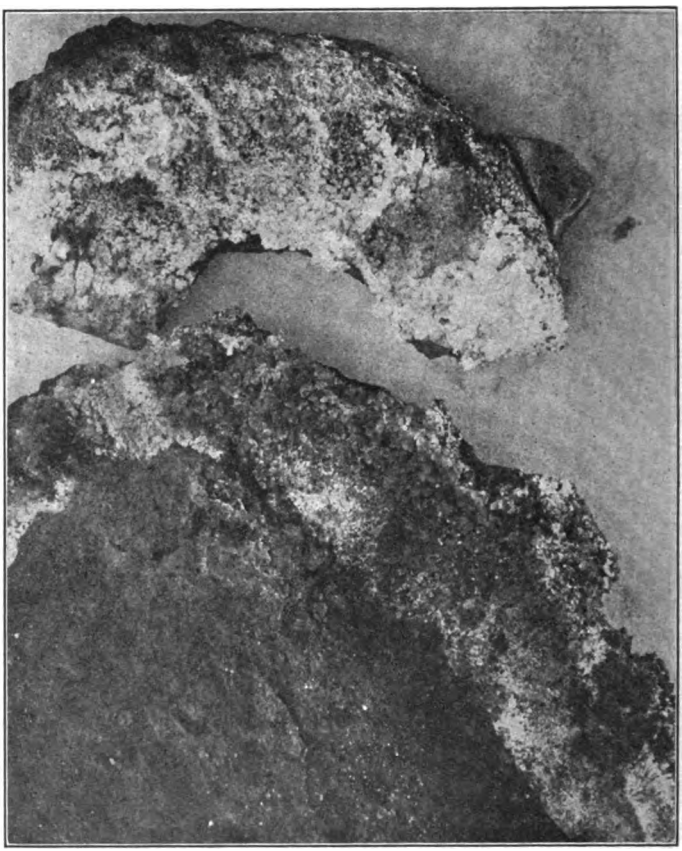

FIG. 2.—YELLOWISH AND YELLOWISH-RED CRYSTALS FROM COOLING PLATES ABOVE MANTEL.

When cooling plates above the mantel burn out, due to the failure of the water supply, and a little gassing has commenced, beautiful white and yellow or yellowish-red crystals often build up around the apertures. In mass, all of these crystals appear to be strongly deliquescent, and hence it is difficult to obtain photographs showing clearly defined crystal faces, though the crystalline nature of the material is clearly apparent from Figs. 1 and 2. When examined on the hot furnace shell with a good hand glass, there is seen sometimes a mixture of different

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kinds of crystals and in different systems. Several samples of brilliant yellow crystals were secured and a few reddish-yellow which are very likely potassium ferrocyanide. Usually, however, the crystals are pure white, and have been found to be chiefly ammonium chloride. An analysis was made of some of these white crystals, slightly contaminated, taken from two different furnaces at different times, the results being: SiO_2, 0.78 per cent.; $Fe_2O_3 + Al_2O_3$, 2.49; CaO, 0.29; MgO, 0.28; K_2O, 0.24; Na_2O, 0.12; Cl, 62.39; CO_2, 0.72; CN, none; NH_4, 30.69 per cent.

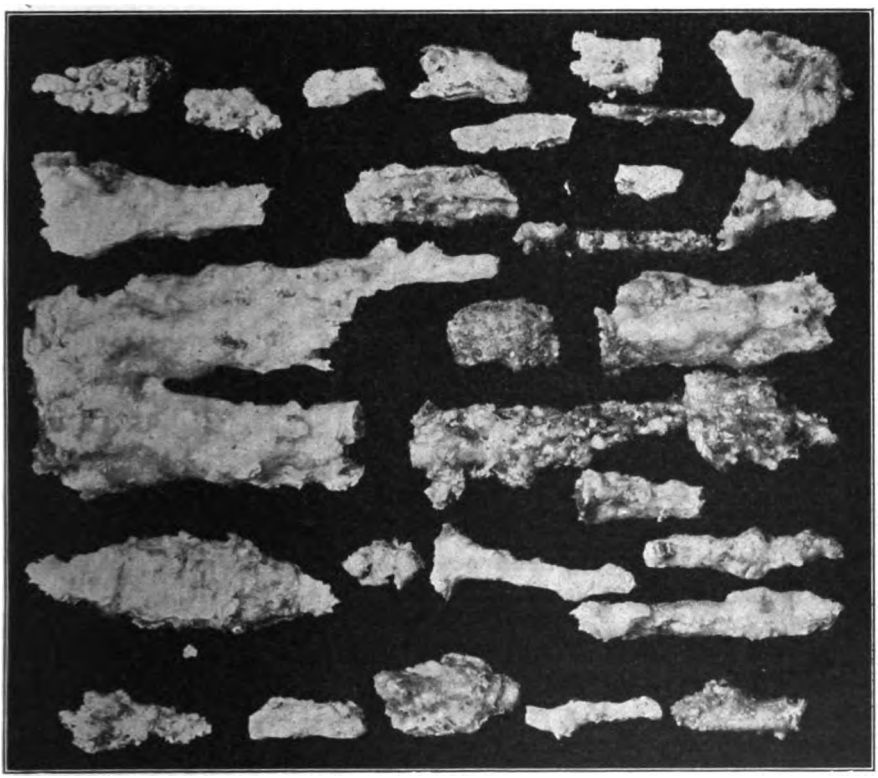

FIG. 3.—WHITE "CYANIDE" STALACTITES FROM SHELL ABOVE MANTEL.

Liquid material sometimes also exudes from around the mantel plates and solidifies in heavy columns or stalactitic masses on the shell. This substance containing some cyanides, is highly deliquescent. Upon several occasions, samples which have been brought into the office in the evening have disappeared into puddles of liquid during the night. Fig. 3 shows some fine specimens. A fairly complete analysis of this material showed the following complex composition: SiO_2, 1.11 per cent.; $Fe_2O_3 + Al_2O_3$, 3.17; CaO, trace; MgO, 0.22; K_2O, 46.15; Cl, 16.94; Na_2O, 18.08; CO_2, 9.64; CN, 7.84; NH_4, none; $Fe(CN)_6$, 5.25; CNS, 0.17.

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The dissimilarity between the composition of this material and the crystals is striking. The excess of potash over soda is noteworthy, as is also the presence of considerable percentages of chlorine in both stalactites and crystals. As will be seen from later analyses of flue dust, an appreciable amount of chlorine, doubtless practically all charged, is carried out of the top of the furnace. Although no special investigation has been made, we know some of it originates in the coke, which is quenched with water rich in chlorides; also, it is doubtless carried by some of the ores as chlor-apatite or other chlorine-bearing mineral.

Whereas certain other investigators have shown considerable percentages of cyanide compounds in dust samples collected through the shell of the blast furnace, or from the flue dust, only the last-mentioned sample, of all collected in this investigation, has shown more than a trace of cyanide. While probably present in considerable quantities in the hearth and bosh, the alkali cyanides forced into the upper zones are evidently almost entirely decomposed before leaving the furnace.

In the case of one furnace which has recently completed a long campaign, most of the cooling plates above the mantel burned out because of failure of water at one time or another. After the wall had worn thin, it was necessary to use a spray cooling pipe on the outside of the shell. To prevent water from entering the furnace, the feed-water pipes were cut off and the holes through the shell covered with "scab" plates on top of asbestos pads. After several months, when these plates were removed, it was found that some of these asbestos pads were dyed deep yellow, and several showed greenish and bluish tinges, indicating complex cyanides. When an old furnace is laboring under high pressure, and is seen to be gassing freely about the mantel, a strong acrid odor, doubtless ammonium chloride, is sometimes evident.

4. *By Combination with the Brickwork or as an Accretion in the Form of Cyanide, Etc., and Removal when the Furnace is Blown Out.*—This is, of course, a relatively small but very interesting part of the alkalies charged during a campaign. Several authors have suggested that destruction of brickwork in certain stacks, particularly in the middle and upper zones, was due to action of alkalies. However, so far as chemical action is concerned, rather more importance has been attributed to reaction between carbon monoxide and iron oxide spots in the brickwork, with subsequent disruption of the brick mass. The destruction of linings is doubtless hastened somewhat in furnaces in which the burdens are relatively rich in alkalies.

The average sample of the hearth brick used in one furnace lining at Bethlehem by careful analysis showed a total alkali content of 1.60 per cent. After blowing out, a sample was secured of a numbr of these brick near the bottom of the hearth, appreciably discolored when broken, which showed a total alkali content of 3.36 per cent. Higher up in the

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lining the alkali enrichment is greater. Samples of pure white, mixed cyanides, etc., have been recovered in various plants from protected crevices in the brickwork, around the boshes after furnace campaigns, and a great deal more would be found if the contents of the stack could be removed dry.

The presence of ammonia around furnaces directly after being blown out is, of course, well known. Sometimes enormous quantities are evolved, the odor being apparent until the entire lining is removed and the hearth cleaned out. The chief source of ammonia gas is undoubtedly the alkali cyanides and ammonium chloride which decompose in the hot water or water vapor introduced to cool down the furnace. In passing, it is interesting to recall that a very small part of the cyanide produced in a furnace is fixed and carried down into the salamander in the form of the peculiar compound, titanium cyano-nitride. The excess accumulation of alkalies in the brickwork, or in salt deposits in a furnace lining after a campaign, may literally amount to several tons.

5. *By Evolution in the Gas.*—The heavy flue dust which is carried out of the top of the furnace by the gas current, of course, contains approximately its normal percentage of alkalies. The finer portions of ore, stone and coke dust doubtless average somewhat higher in alkali content than the entire materials as charged. All the remaining potash and soda in the burden, not previously accounted for, is sublimed and passes out in the form of various salts or compounds as a fine fume.

Various investigators have studied blast-furnace flue dust with special reference to its potash content, but almost entirely from a scientific standpoint. It is true that at least one American and one foreign patent have been issued for the recovery of potash in flue dust, but no evidence has been found that any practical application has thus far been made. And, so far as can be ascertained, the first commercial disposition of the flue dust as a fertilizer was made by us at Bethlehem somewhat more than 2 years ago.

The progress of the alkalies in our practice will now be traced from the furnace top, through the gas mains, washers, stoves, boilers and stacks.

Alkalies in Dry Dust Between Furnace and Washer

Besides the dust carried out by the relatively small amount of gas escaping from the furnace top, and the insignificant quantity permanently deposited along the mains, all the dust under the above heading is removed from the dust catcher. Representative bi-weekly samples of the dust-catcher flue dust were taken over the same period during which the slag samples were secured and analyzed, with the results shown in Table 4.

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TABLE 4.—*Analyses of Dust-catcher Flue Dust*

Furnace	Period Included, 1916	Fe	Ign. Loss	CN	Total		Water Soluble	
					K_2O	Na_2O	K_2O	Na_2O
A	9/25 to 7/8	40.7	17.3	None	0.63	1.45	0.40	0.35
A	7/9 to 7/22	47.2	11.5	None	0.64	1.47	0.27	0.16
C	9/25 to 7/8	59.8	1.7	None	0.50	0.94	0.29	0.30
C	7/9 to 7/22	53.8	8.8	None	0.69	1.07	0.24	0.16
D	9/25 to 7/8	40.5	18.1	None	0.59	1.30	0.40	0.42
D	7/9 to 7/22	43.3	12.9	None	0.88	2.10	0.29	0.22
E	9/25 to 7/8	49.3	9.8	None	0.50	0.90	0.31	0.30
E	7/9 to 7/22	46.3	16.4	None	0.54	1.10	0.17	0.20
F	9/25 to 7/8	46.6	14.6	None	0.42	0.85	0.20	0.22
F	7/9 to 7/22	50.6	8.9	None	0.59	1.20	0.22	0.18
G	9/25 to 7/8	47.4	8.1	None	0.55	1.50	0.25	0.20
G	7/9 to 7/22	43.3	12.2	None	0.87	1.60	0.29	0.24
Average for all furnaces first period					0.53	1.16	0.31	0.30
Average for all furnaces second period					0.70	1.42	0.25	0.20
Average for all furnaces both periods					0.61	1.29	0.28	0.25

Ratio average total potash to average total soda	1 : 2.1
Ratio average water-sol. potash to average water-sol. soda	1 : 0.9
Ratio average water-sol. potash to average total potash	1 : 2.2
Ratio average water-sol. soda to average total soda	1 : 5.2

A much greater percentage of the total potash than of soda is present in a water-soluble condition.

The entire absence of cyanides in this dust, and even in concentrated alkaline samples, recovered further along in the gas system, furnishes further corroboration of the fact that in normal operation practically no cyanide is carried in the gas current from the furnace top. That it may be an occasional constituent of flue dust, because of abnormal furnace conditions, is evidenced by several records at hand, notably by the following abstract.[1]

The greater part of the CN compound dissociates higher up in the furnace and aids in the ore reduction. A portion passes away unaltered with the gases and is deposited in the flues. CN in small quantities was found in flue dust by Ledebur. It may, however, occur in considerable quantities as the Austrian Alpine Co. found to its cost. In the summer of 1901, about 20 tons of flue dust from the Hieflau charcoal blast furnace in Styria was tipped into the river Ems, and an enormous quantity of fish were killed. Cyanogen was found in the dust and 2,800 pounds damages had to be paid.

Also, an instance of a large quantity of cyanides being evolved from

[1] Sir I. Lowthian Bell: *Journal of the Iron and Steel Institute*, No. 2, 1882, p 555.

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the furnace top occurred at the Colebrook Furnaces, Lebanon, Pa., several years ago. I trust that this occurrence will be enlarged upon in the discussion.

It will further be noted that there is a considerable enrichment of this flue dust by the alkali fume. At one furnace where dry gas is used in the stoves, there are two Brassert Witting dry whirlers and a long dry dust main with numerous dust legs between the dust catcher and stoves. This dust is increasingly fine and somewhat lighter in color at each of the points removed. Potash determinations at different times have shown a gradual increase in the successive samples, running as high as 5 per cent. at the stove burners.

Tests made at different times show that our dust catchers remove an average of about 100 lb. of dust per ton of pig iron produced. Using the average percentage of alkalies in flue dust for the four weeks' period, we find:

	Pounds	Per Cent. of Total Charged
Average potash in flue dust from dust catcher per ton of pig.	0.6	1.8
Average soda in flue dust from dust catcher per ton of pig.	1.3	2.3

The ratio of total potash to total soda present is roughly about the same as in the average charge entering the furnaces.

Effect of Primary Washers on Alkalies in Dust

At Bethlehem we have in service a type of tower spray washer, in common use in this country. All of the gas for stoves and boilers, as well as for gas engines, is washed, except from one furnace. Naturally, it would be thought that practically all of the alkaline material in the dust, most of it readily soluble in water, would be removed in the wet washers. The bulk of it is washed out, but it is a remarkable fact that much of the water-soluble alkalies remains in the gas current after leaving the washers. Calculations based on alkalies charged into the furnaces, lost in the slag, dust catcher, stack gases, etc., and recovered from the stoves and boiler settings, indicate that about 20 per cent. of the total potash (apparently only about 5 per cent. of the soda) entering the primary washers, passes through them. The explanation for this fact is that the particles of fume are in such an exceedingly fine state of division that they escape contact with the relatively large drops of water. In my opinion, any washer that will successfully clean blast-furnace gas rich in this fume must employ spray nozzles or other devices that will discharge water in a fine mist, thus insuring intimacy of contact between dust and water particles. Our washers at present perform the

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function of selective precipitation, eliminating the relatively coarse and heavy iron ore and coke particles, while delivering the lighter particles of dust and fume into the primary clean gas main.

Alkalies in Dirt from Primary Clean Gas Mains

The mains carrying gas from the primary washers to stoves, boilers and secondary gas-cleaning plant accumulate dirt or mud gradually, and are washed out at intervals of every 2 or 3 months. This dirt is black when wet but after being dried is dark gray, and after the fine particles of coke dust are burned out, it is of a light-gray or reddish-white color. Sometimes drippings from a clean-out door along a gas main will form beautiful long stalactites. This material is in general appearance

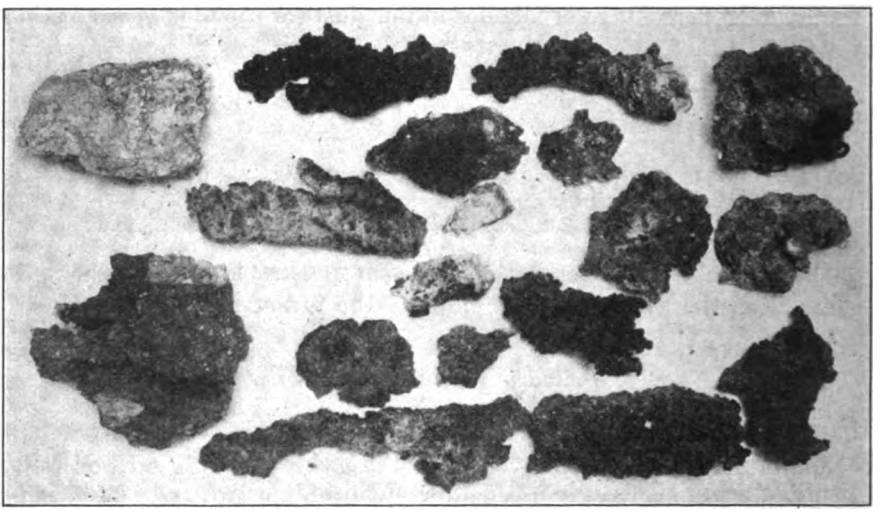

FIG. 4.—STALACTITES, RESULTING FROM DRIPPINGS FROM GAS MAINS.

similar to the stalactites mentioned as exuding from the furnace shell. A typical sample was found to have the following proximate composition, Fig. 4 showing its general appearance: SiO_2, 0.28 per cent.; Fe_2O_3 + Al_2O_3, 0.68; CaO, trace; MgO, 0.25; K_2O, 44.63; Na_2O, 7.13; Cl, 37.22; CO_2, 7.78; CN, trace; NH_4, none. The almost utter absence of cyanide in this concentrated alkaline material is worthy of note.

Alkaline Dust in Stoves and Boiler Settings

The dark, wet dirt carried into the stoves, which at our plant are of the three-pass type, is dead burned, of course, the combustible or volatile constituents, etc., being expelled. The residue, in the form of an impalpable powder, collects to some extent in the bottom of the combustion chambers, a relatively large amount gathers in the bottoms of the second-

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and third-pass checkers while the largest portion is carried out in the stack gases.

In the combustion chambers the dust frits together in friable masses. The accumulation is not great, the wells being cleaned out at intervals of about 2 months. However, the action on the brickwork is serious. All over the inner surface of the combustion-chamber walls, the lower surface of the dome, and the second pass checkers for a depth of several feet, a deep-green glaze is formed, or the brickwork is honeycombed.

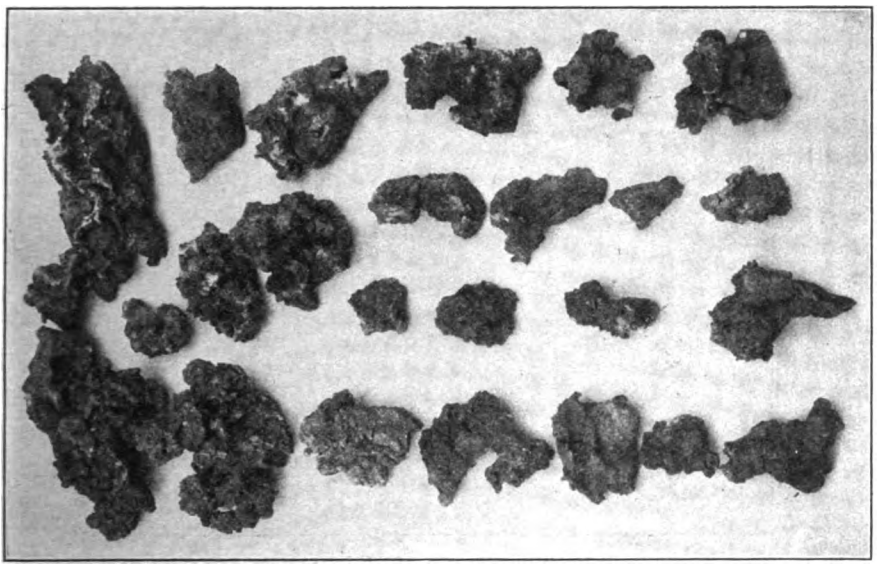

FIG. 5.—FRAGMENTS OF LOW SILICA STOVE BRICK, HONEY-COMBED BY ALKALI FUME.

Along the fire-clay joints between the brick, the action is especially marked. Stove brick of a high silica content invariably glaze or slag, whereas those with a lower percentage of silica are more readily honeycombed or excoriated by the alkaline fume. Fig. 5 shows a typical sample of excoriated brickwork, while Figs. 6 and 7 exhibit surface glazed brick and a completely fused green slag respectively. Analyses of these materials show the following interesting results:

	SiO_2	Fe_2O_3	Al_2O_3	K_2O	Na_2O
Green slag No. 1	60.66	2.30	14.55	16.30	6.03
No. 2	58.44	2.50	12.60	18.07	8.29
Original brickwork.....	79.70	19.70		0.25	
Excoriated brick.....	40.17	26.70		20.84	8.04
Original brickwork.....	52.50	2.00	43.0	1.95	

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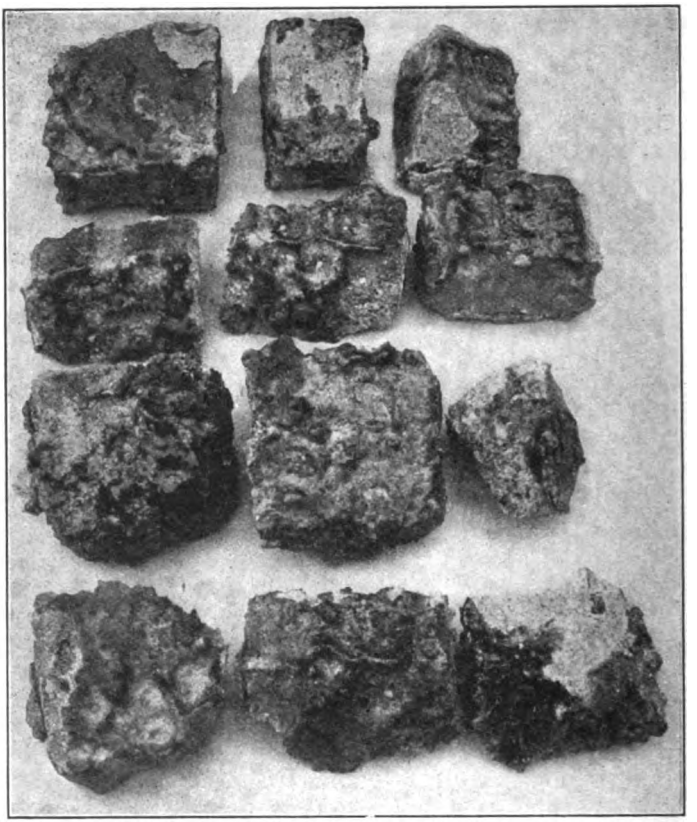

FIG. 6.—HIGH-SILICA STOVE BRICK, GLAZED BY ALKALI FUME.

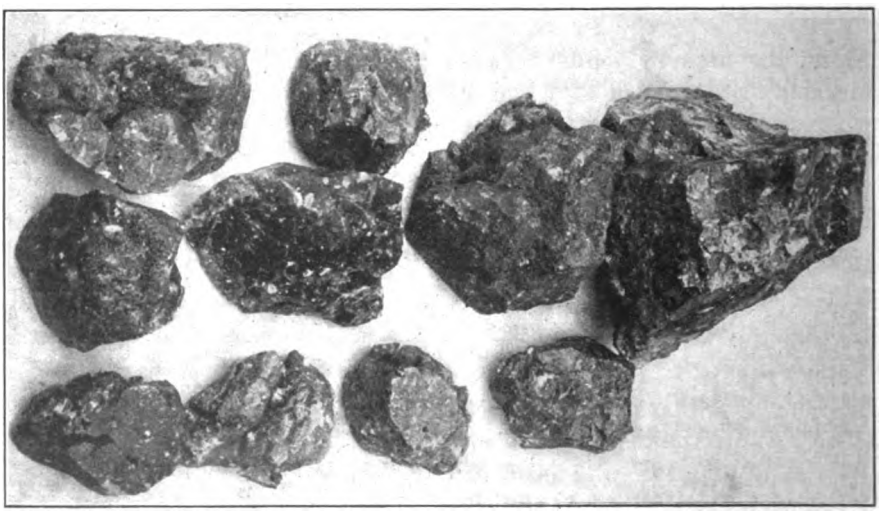

FIG. 7.—GREEN SLAG, RESULTING FROM COMPLETE FUSION OF HIGH-SILICA STOVE BRICK BY ALKALI FUME.

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The thermal efficiency of the stoves is reduced, of course, by this glazing, erosive and honeycombing action, though repairs seldom have to be made during a furnace campaign.

The fine, light-colored dust, which accumulates in the bottom of the second and third passes, is the material of commercial interest to us. It is cleaned out at intervals of 3 or 4 months. A typical pile of this dust freshly removed through one of the stove clean-out doors is shown in Fig. 8.

In the boiler houses the dust accumulates and is recovered every few days from the combustion chambers, from the passes and occasionally from the horizontal flues leading to the stacks. It becomes increasingly fine and richer in potash in its progress. The boiler tubes, of course, become coated with the dust and must be blown off with a steam or air lance.

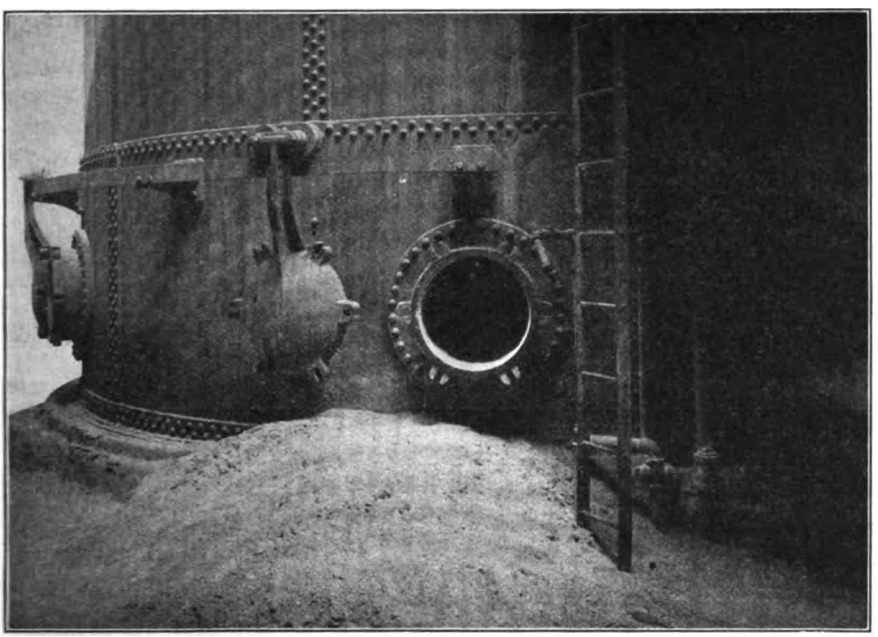

FIG. 8.—TYPICAL PILE OF FLUE DUST REMOVED FROM STOVE, SHIPPED AS FERTILIZER.

The white fume continuously issuing from the top of the stove and boiler-house stacks is approximately of the same composition as that reclaimed in the last passes.

Properties and Quantity of Alkaline Dust Recovered for Sale

As previously indicated, the dust recovered from the combustion chambers and stoves and boilers is a light, friable sinter, whereas the

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larger amount reclaimed from the passes and flues is in an exceedingly fine state of division. When drawn out of the stoves or boiler settings hot, it runs almost like quicksilver, but it absorbs a certain amount of water readily, and becomes somewhat clammy and heavy.

An attempt was made to obtain some data on "angle of repose" of the fine dust. However, the cold, freshly dried dust behaves very differently from the hot material. A large, glass funnel at a fixed height over a plane surface was employed in the experiment. Comparative tests with white sand and two kinds of ore, prepared to various degrees of fineness, down to ultra 100-mesh, all yielded heaps with angles of repose of about 40°, whereas the stove-dust samples varied from 43° to 50°.

Sieve tests were also attempted. By careful sifting, it was found that all of the alkaline dust would pass a 300-mesh sieve. It is a true fume. The small residues remaining on the various sieves, when examined under a magnifying glass, were found to be chiefly eroded brickwork.

Specific-gravity tests, also, cannot be made with accuracy. The lightest dust, without compression, will run well below 0.50 apparent specific gravity. Samples taken from the checkerwork of five different stoves, and showing potash contents of 10 to 14 per cent., when tamped down gently in large glass measuring cylinders, showed the following varying densities: 0.68, 0.69, 0.73, 0.89, 0.96.

Three typical samples of fine stove dust were taken from different furnaces, and after being screened through a 300-mesh sieve and dried, were sent to an expert microscopist, skilled in examining such material. All of the samples showed the same characteristics, and 12 typical photomicrographs are reproduced herewith (Plates 1, 2 and 3) together with quotations direct from the microscopist's report.

"The general characteristics in this dust are: Very fine, spherical particles; the large proportion of them as single particles, but many united into groups and held together as by a gelatinous mass; the large majority of the particles are nucleated, the nuclei, however, varying greatly in size, as do the particles themselves; the nuclei may be several or but one in a particle, and are of a dark orange or brown color; the form of the particles is spherical to pear-shaped, while the form of the nuclei is practically spherical. The nuclei are not always confined to the inner or central part of the particles, but may be on its surface.

"One photograph of each of the samples A, B and C was taken by means of a paraboloid dark ground illuminator, of approximately 500 diameters. They tell nothing of the detail of the individual particles, but they do show clearly the grouping of the particles into aggregates of greatly varying sizes and forms. These aggregations are merely formed by the mutual attractions; that is, each particle is within the radius of attraction of the other. Photograph A shows particularly many single and individual particles, and represents a characteristic field of the samples. Photograph B shows a larger group, or one of the largest groups, while photograph C represents smaller groups. It should be said that a larger group is reduced easily to smaller ones by agitation or stirring or crushing. No detail can be possible at this magnification.

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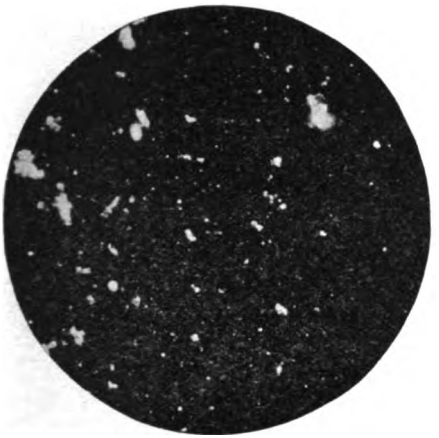

FIG. A.—× 325.

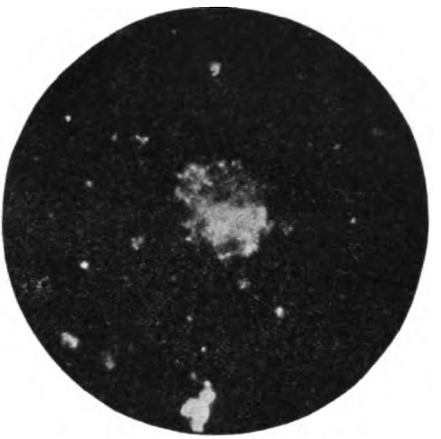

FIG. B.—× 325.

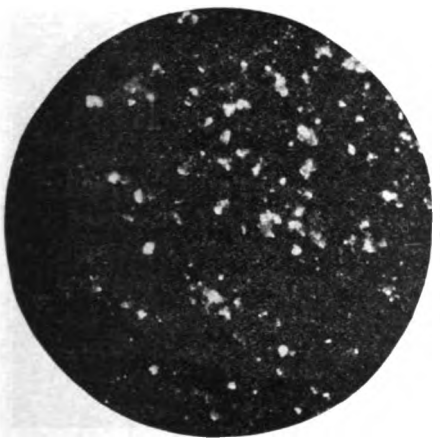

FIG. C.—× 325.

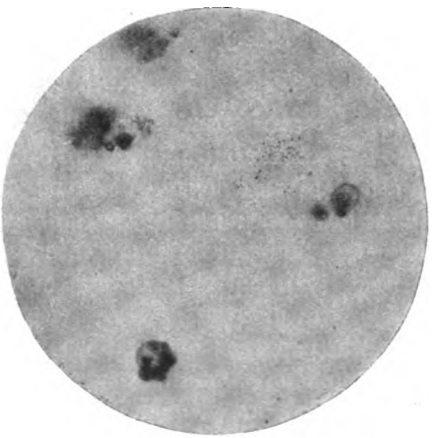

FIG. D.—× 1300.

PLATE 1.—PHOTOMICROGRAPHS OF FINE FLUE DUST RECOVERED FROM STOVES.

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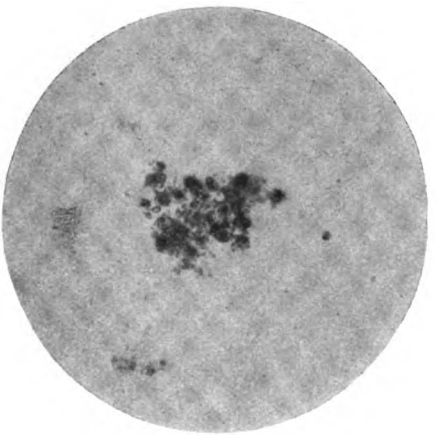

FIG. E.—× 1300.

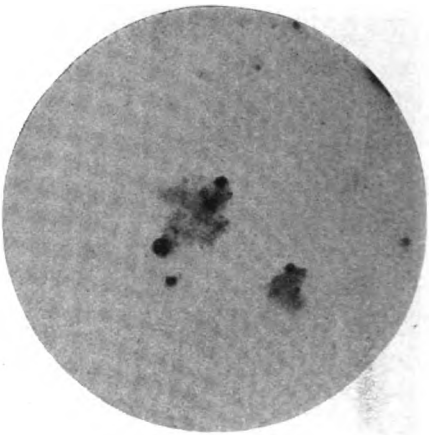

FIG. F.—× 1300.

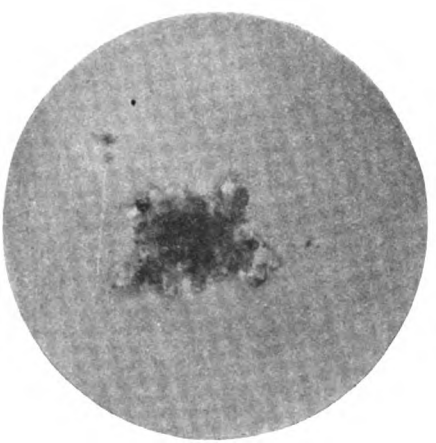

FIG. G.—× 1300.

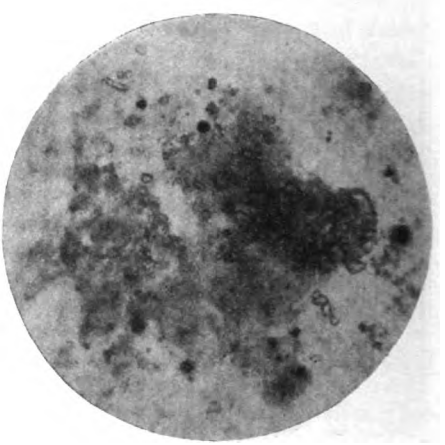

FIG. H.—× 1300.

PLATE 2.—PHOTOMICROGRAPHS OF FINE FLUE DUST RECOVERED FROM STOVES.

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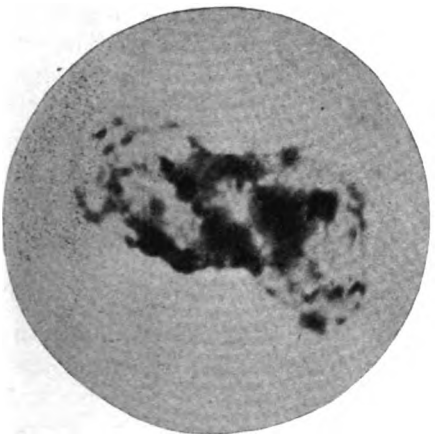

FIG. I.—× 1300.

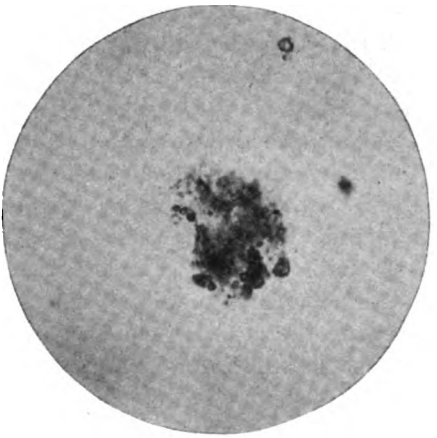

FIG. J.—× 1300

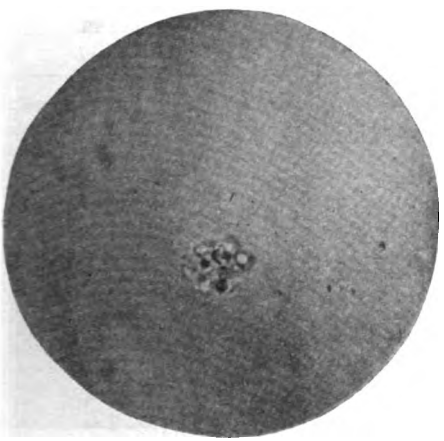

FIG. K.—× 1900.

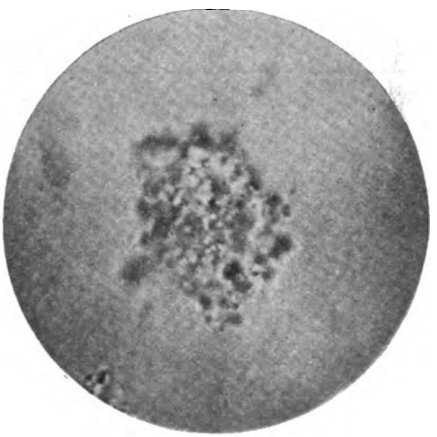

FIG. L.—× 1900.

PLATE 3.—PHOTOMICROGRAPHS OF FINE FLUE DUST RECOVERED FROM STOVES.

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"For detail, higher magnifications, with proper color screens and apochromatic lenses are essential."

Difficulty was experienced with these lenses in getting all the particles in a group into focus. The greatest trouble, however, was in getting both the vitreous groundmass and the orange-brown nuclei to show up together. Under the circumstances, the photographs are creditable and interesting.

Aside from the three above-mentioned, all of the other photographs were taken under a magnification of 2,000 diameters (1300 as reproduced), with the exception of K and L which were taken under 3,000 diameters (1900 as reproduced). The three samples are each represented by four photographs (Plates 1, 2 and 3).

Knowing the exceedingly fine state of division and smooth, spherical nature of the ultimate particles in this fine fume, there is little cause for wonder that so much of it passes through a creditable wet washer untouched.

The water-soluble potash content of the dry, ignited dust, recovered from the stoves and boilers, having previously passed through the wet washers, will vary from about 5 to 20 per cent. Our practice has been to store the material in a large bin, capable of holding two or three carloads. The commercial recovery, begun in 1914, was not thoroughly systematized until the ensuing spring. Table 5 shows the weights and water-soluble potash contents, calculated on the dry basis, of 36 carloads

TABLE 5

Weight of Carloads in Net Tons	Percentage Water-soluble Potash (K_2O)	Weight of Carloads in Net Tons	Percentage Water-soluble Potash (K_2O)
33.0	10.05	34.7	7.04
31.3	11.20	32.8	6.22
26.1	9.95	36.4	10.24
24.9	10.65	33.7	7.15
25.4	10.10	32.5	12.00
24.0	11.31	27.3	7.27
25.7	13.96	29.9	11.38
21.1	8.32	31.3	14.22
27.4	9.32	35.0	7.69
27.7	12.17	34.1	9.01
27.9	9.62	26.3	7.55
24.2	11.91	35.1	7.82
26.1	15.92	39.4	6.05
24.0	15.07	27.8	7.48
34.5	11.00	26.4	8.76
28.2	10.27	32.1	8.60
30.6	10.37	32.5	8.58
33.9	8.73		
30.2	14.35	Total 1,073.5	Average 9.90

Total weight of water-soluble potash 106.3 net tons.

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of alkaline dust shipped in the period of 15 months, Apr. 1, 1915, to July 1, 1916. This represents all the dust removed from stoves and boilers serving four 500-ton furnaces, which could be recovered in normal operation, without special effort. Instead of being dumped into a dirt car, the dust was simply emptied into a central bin. A number of other carloads of lower-grade material, contaminated by flue dust from unwashed gas, were also shipped from the other furnaces, but are not included in this tabulation.

Complete analyses of three typical carload samples and one especially rich stove-dust sample, containing approximately 20 per cent. water-soluble potash, are given in Table 6.

TABLE 6.—*Analyses of Typical Carload Samples and a Rich Stove-dust Sample*

Sample No.	1 Per Cent	2 Per Cent	3 Per Cent	4 Per Cent
SiO_2	22.22	18.07	18.50	22.66
Fe_3O_4	15.24	11.25	2.31	2.30
MnO	0.67	0.67	0.75	0.79
Al_2O_3	11.58	11.04	11.22	11.60
TiO_2	0.06	0.05	trace	trace
CaO	11.69	12.22	13.14	12.49
MgO	7.35	8.49	12.35	6.76
ZnO	2.57	0.82	4.29	2.04
C	5.04	5.43	0.50	0.40
SO_3	6.35	6.13	7.60	10.80
CO_2	1.61	1.94	2.40	0.58
Cl	3.34	5.29	5.40	4.86
CN	None	None	None	None
CNO	None	None	None	None
CNS	None	None	None	None
NH_4	None	None	None	None
Total K_2O	10.87	14.40	16.94	19.71
Total Na_2O	3.74	7.30	7.06	7.34
Water-soluble K_2O	7.55	12.00	15.92	17.04
Water-soluble Na_2O	3.28	3.94	4.44	3.98
Ratio water-soluble to total K_2O	0.69	0.83	0.94	0.86
Ratio water-soluble to total Na_2O	0.88	0.54	0.63	0.54
Ratio total K_2O to total Na_2O	1:0.35	1:0.51	1:0.42	1:0.37

No readily volatile constituents, such as may occur in the unignited dust, of course, are to be found after roasting.

In general, the ratio of water-soluble to total potash is higher than in the case of soda.

The preponderance of both total and water-soluble potash over the corresponding soda determinations is impressive, which seems to show the much greater removal of the latter in the wet washers.

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If we assume that the total soda content in the stove and boiler dust averages 40 per cent. of the total potash content, and the water-soluble potash 80 per cent. of the total potash, as appears reasonable from the foregoing analyses, then the total alkalies recovered in the fertilizer material in the above period is found to be:

Total potash	118 gross tons.
Total soda	47 gross tons.

Calculating on the basis of pig iron produced in the above 15-month period, the alkalies recovered in terms of total charged are as follows:

	Per Ton Pig, Pounds	Per Cent. of Total Charged
Total potash recovered in fertilizer material	0.3	1.3
Total soda recovered in fertilizer material	0.1	0.3

The amount of potash recovered, though of considerable tonnage and value, is an insignificant percentage of the total charged.

At furnace plants using burdens rich in potash where the gas is unwashed, a considerable recovery still can be effected, though some precautions must be taken to avoid the coarser, raw flue dust. Several other furnace plants in the East, acting upon our suggestion, made directly or indirectly, have been recovering potash-bearing flue dust during the last year and a half.

Alkaline Dust Escaping from Stove and Boiler-house Stacks

Some idea of the relative amount of alkalies discharged per unit value of gas from blast-furnace stove and boiler stacks may be gained by observing the color and depth of the fume. Dirty gas, of course, will obscure greatly the fine, white, alkaline fume. In Western practice, with washed gas, the escaping stack gases show a thin, white color. In our practice the stack fume is much heavier.

About 2 years ago we made a series of tests, extending over a period of about a week, to determine the approximate amount of potash lost through our boiler-house stacks. The average results on the two largest stacks were as follows:

Average amount of dust per cubic foot flue gas (62° and 30″), grains	0.12
Average percentage water-soluble K_2O in this dust	14.70
Average percentage total	18.60

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Knowing the average volume of flue gas discharged, and the average volume of fuel gas consumed by the boiler houses, and produced by the furnaces, we can calculate roughly the weight of potash lost through the boiler-house stacks per ton of pig iron.

No actual tests have been made on potash losses from the stove stacks, but they should be slightly less per unit volume of fuel gas burned on account of the greater baffling action of the stove brickwork. We will assume these losses per unit volume of fuel gas to be 90 per cent. as great as from the boiler-house stacks. The soda-potash ratios will be assumed to be the same as in the average stove dust.

	Per Ton Pig, Pounds	Per Cent. of Total Charged
Total potash lost in boiler-house and stove-stack gases....	2.6	11.2
Water-sol. potash lost in boiler-house and stove-stack gases.	2.2	
Total soda lost in boiler-house and stove-stack gases.....	1.1	2.8

Alkalies Lost in Secondary Wet Washers

A considerable percentage of the primary cleaned gas at Bethlehem is further cleaned in Theisen scrubbers for gas-engine use. Practically all of the fine dirt is thus removed, the small amount passing through the fine gas main and into the combustion chambers of the engines having practically no cutting or scouring action, even when as much as 0.03 grain per cubic foot of gas is present. Samples of this dust, recovered near the engines, were of a smooth talcy consistency. The composition is similar to the finest stove dust.

The amount of alkalies removed in the secondary scrubbers, per unit volume of gas, is practically the same as the sum of the alkalies precipitated in the stoves (or boiler passes) and escaping through the stacks. Knowing the gas-engine consumption of fuel gas, the alkalies removed in the wash water, calculated on the same basis as for the stoves and boilers, is readily found to be as follows:

	Per Ton Pig, Pounds	Per Cent. of Total Charged
Total potash lost in secondary scrubbers.....	0.5	2.2
Total soda lost in secondary scrubbers.....	0.2	0.6

Balance Sheet for Alkalies Charged, Removed and Lost

Bearing in mind the extremely difficult nature of the problem, and the necessity for including a number of estimates, I will endeavor to sum-

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marize a rough balance sheet for alkalies, charged and produced in our average blast-furnace practice. Further, it will be remembered that this balance sheet is approximately representative of our local conditions only.

TABLE 7.—*Balance Sheet for Alkalies Charged, Removed and Lost*

	Potash (K_2O)		Soda (Na_2O)	
	Pounds per Ton Pig Iron Produced	Per Cent. Total Charged	Pounds per Ton Pig Iron Produced	Per Cent. Total Charged
Total charged	22.4	....	35.6	
Lost in slag	4.5	20.0	6.3	17.0
Lost in fume, etc., from iron and cinder notches, and shell (est. 10 per cent. of slag loss)	0.4	2.0	0.6	1.7
Lost in gases from top of furnace (est. 5 per cent. total gas losses)	0.9	3.9	1.4	4.1
Lost by combination with brickwork of furnace, stoves, etc.	negligible		negligible	
Recovered in dust-catcher dust	0.6	2.7	1.3	3.6
Lost in primary washers (est. by difference)	12.5	55.9	24.6	69.5
Lost in secondary washers	0.5	2.2	0.2	0.6
Recovered in stove and boiler passes	0.3	1.3	0.1	0.3
Wastage from mains, stoves and boilers (est. 30 per cent. of quantity recovered)	0.1	0.4		0.1
Lost in stack gases	2.6	11.2	1.1	3.1
Total	22.4	100.0	35.6	100.0

Apparently there is a greater loss of soda than of potash in the primary washer. This may be due to the greater solubility of the sodium salts, or to the larger size or possibly to the difference in contour of the fume particles of sodium compounds.

Cause of "Smoky" Gas

Every blast-furnace man is familiar with the phenomenon of "smoky" or "calico" gas. This title refers to a peculiar flecked, mottled or streaked appearance of the gas flame and is concomitant with more or less imperfect combustion. In the worst cases the gas will not burn at all in the boiler combustion chambers, even in the presence of a good wood fire. It is more frequently noticed in boiler than in stove practice, on account of the greater surface of hot brickwork, which promotes combustion, in the latter. To my knowledge, no trouble in gas engines has been attributed to smoky gas. The more or less imperfect combustion of the gas is accompanied by corresponding amounts of carbon monoxide in the flue gases. The phenomenon occurs in both dirty and primary washed gas, in some plants much more frequently than in others, but at no regular intervals. This gas is not only a curiosity, but, in plants where blast-furnace gas is at a premium for steam development, its appearance is the cause of concern to the steam engineering and blast-furnace de-

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partments. The condition of smoky gas may continue for only a few minutes, or it may last for several hours. In a large plant, it can usually be traced to one, two or three furnaces, seldom being manifest in all the boiler houses, unless they are closely segregated and fed from a common gas main.

So far as I am aware, the cause of smoky gas has not been determined definitely and proved. We have taken numerous samples of gas when it was burning badly under the boilers, but the composition was always found to be normal. The temperature of the washed gas was not found to be above normal, indicating high moisture content, nor was there an abnormal amount of entrained water present.

I am indebted to C. H. Rich, Metallurgist, Alan Wood Iron & Steel Co., for the suggestion that smoky gas is concomitant with increased amounts of fume, in the gas, including cyanide or chloride. Determinations, made under his supervision, showed an enrichment of the fuel gas with one or both of these constituents when the gas was burning badly; elimination of the fume automatically restored a free-burning gas.

To test this idea further, I had several large samples of potassium chloride, sodium chloride and potassium cyanide prepared, in part by grinding to about 40-mesh size, and in part to ultra 100-mesh size. A suitable aspirating spray nozzle, operated by compressed air, was inserted through the breech of a boiler gas burner. The salts were introduced easily in a thin stream through this device. In the coarser condition, none of the salts had any appreciable effect on the character of the flame. However, the ultra 100-mesh product of all three produced in a clear flame exactly the same phenomenon as observed in smoky gas. In the case of sodium chloride, which was dry and easily pulverized, the flame could almost be extinguished.

The logical reasoning from these tests is that the cause of smoky gas is a purely physical one, due to the presence of an unusual amount of exceedingly fine fume in the gas, preventing proper contact for combustion between the particles or molecules of fuel gas and oxygen.

It has been our observation that smoky gas is simultaneous with high top temperatures in the furnace, but at irregular intervals. As earlier investigators have suggested, part of the alkali salts probably circulate in the furnace many times, on account of condensation in the upper zone. But the excess accumulation must be expelled at intervals, which naturally correspond to high top temperatures, low stock lines or high hearth temperatures. The fact that cold air admixture in the hot blast, when a furnace is producing smoky gas, will alleviate the condition temporarily, constitutes additional evidence that alkali fume, in considerable quantity, is being evolved during such periods.

A considerable amount of fume also will be noticed when smoky gas is burning. It is difficult or impossible to determine the approximate

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average size of the ultimate fume particles in blast-furnace gas. The clammy, hygroscopic nature of the material makes microscopic work laborious. Judging from the photomicrographs made at 3,000 diameters, accompanying this paper, there are many minute particles which appear to have a diameter of about 0.01 in. in the photograph, and many others which do not exceed, say 0.04 in. The actual diameter of such particles would therefore be 0.0000033 in., and 0.0000133 in. diameter respectively. Assuming 0.3 grain of fume per cubic foot of fuel gas, and an intrinsic density of 2.0, the number of particles of fume per cubic foot of gas is found to reach the enormous total of over 30,000,000,000,000 and about 500,000,000,000 for the two sizes above mentioned. There would be about 32,000 and 8,000 particles per linear foot respectively, and the distance between centers of the particles would be only 0.000385 in. and 0.00124 in. respectively. These calculations are purely speculative, but they indicate the physical interference possible in the proper combustion of gas by the introduction of a quantity of fine fume particles. It will be readily understood that coarse, heavy particles of dust, such as exist in ordinary unwashed gas, per unit weight, will have much less effect in breaking up the continuity of the gas stream than fine fume.

In connection with Mr. Rich's original suggestion, I offer the above theory in explanation of the phenomenon of smoky gas. If there are other theories, or objections to this one, I trust that proper publicity will be made.

The obvious method of preventing smoky gas is simply to clean the gas more thoroughly, though this is more easily said than done.

Methods for Further Recovery of Potash

In our present gas-cleaning practice, it appears that there is a loss in the primary washers alone of over half of the total potash charged, or about 12 lb. per ton of pig iron produced, though, as mentioned later, this amount is probably a little high. The amount recovered, while appreciable, and representing almost clear profit, is seen to be an insignificant part of the total, less than 2 per cent. Potash lost in the slag, around the shell and from the top of the furnace, for all practical purposes, is lost beyond recovery. The greater part of the alkali content of the flue dust removed from the dust catchers could be recovered by leaching in water, but the percentage is too low to justify reclamation in this way. However, the potash now lost in wet washers and from stove and boiler-house stacks offers a legitimate and inviting field for its recovery. According to our balance sheet, it appears that about two-thirds of the total potash charged is now lost in the wash water and stack gases, or about 15 lb. per ton of pig iron produced.

About 2 years ago we obtained estimates for cost of recovery of the flue dust from several of our large boiler-house stacks, both by filtering

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and electrical precipitation methods; but the relatively high cost, and the uncertainty in the potash market, deterred an actual installation. Later it was realized that the principal loss of potash was not from the stacks.

At intervals during the course of the last year or more, we have had in operation an experimental Cottrell electric dust precipitator, connected to the raw gas main leaving one of the dust catchers. It is not my purpose in this paper to discuss the operation of the unit, except to state that practically all the dust and fume entering the treater could be precipitated successfully. The color of the dust recovered varied from a light to a dark gray. Several samples were analyzed and showed a potash content of about 10 per cent. The total dust leaving the dust catcher is evidently very much richer in potash than the relatively heavy particles constituting the dust in the dust catchers. However, with the above knowledge at hand, and judging from check calculations on total dust leaving the dust catchers and its theoretical potash content (by difference), it is my opinion that the estimated weight (by difference) of potash lost in the primary washers is somewhat, though not greatly, too high. For our average practice, there should be not less than 12 lb. of total, and probably about 9 lb. of water-soluble potash, now lost in washers and stacks, per ton of pig iron produced.

It may be mentioned that by weak acid treatment part of the insoluble potash content in flue dust may be rendered water-soluble, though this is not likely to be of practical application. Also, the soluble alkali salts can be recovered in tolerably pure form by leaching and evaporation.

It is not my purpose to develop at length the commercial phase of the subject. With the foregoing figures and present potash-fertilizer value as a basis, anyone can readily determine what a very attractive proposition is the recovery of potash from blast-furnace gas at the present time. A word of caution may be appropriate. As previously indicated, the weight of potash charged per unit of iron produced is above the average at Bethlehem. There are only two or three apparently practical methods for recovery on a large scale of potash from blast-furnace gas, and they are expensive and as yet untried for blast-furnace conditions. The price of potash is certain to fall after the war.

On the other hand, the recovery of potash in connection with the thorough dry cleaning of blast-furnace gas, with certain obvious advantages as against wet cleaning, is an attractive proposition to plants now suffering from burdens rich in alkalies. I venture to predict that in the future dry cleaning will be adopted in many blast-furnace plants, and that many thousands of tons of potash, hitherto wasted, will be reclaimed. Thus will our national resources be strengthened in this important raw material, and the blast furnace will have added another material to its increasing list of byproducts.

The subject of alkalies in blast-furnace practice is of a difficult and

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complicated nature. It will afford much food for thought, both for the philosopher and the practical furnace operator.

In conclusion, I wish to acknowledge especially the coöperation of F. O. Kichline, Chief Chemist, who supervised the difficult analytical work presented in this paper.

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DISCUSSION

CHARLES H. RICH, Conshohocken, Pa. (communication to the Secretary*).—Mr. Wysor has certainly covered his subject in the most thorough and able manner and his paper will no doubt result in enlarged effort toward the conservation of this important byproduct. Not all furnace burdens are as rich in alkalies as those noted by Mr. Wysor. In our own experience we have never gone into any extended examination of the materials entering the furnace burdens, but have made a number of determinations of the water-soluble alkalies of our flue and boiler dust and found maximum of about 4½ per cent. K_2O—too small for profitable reclamation.

We have, however, frequently encountered the smoky gas described by Mr. Wysor. Such gas carries heavy fume, and burns with a fitful flame difficult to maintain. So pronounced was the inhibitory effect of the fume that, in extreme cases, the flame of a lighted match was extinguished by the current of gas and it was found impossible to conduct calorimeter tests without preliminary washing of the gas. When the gas was led through a washer filled with distilled water and pieces of coke, the fume was absorbed and the gas burned without difficulty. Careful

* Received Jan. 29, 1917.

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analyses conducted on the water solution from the washer showed cyanides in increasing amounts in proportion to the difficulty experienced in burning the gas. We note Mr. Wysor's statement that he has never found evidence of cyanides in their gas, ascribing the difficulty of burning such gas to the presence of chlorides. Our determinations were conducted in such manner as to preclude possibility of interference by chlorine.

Some results obtained in a test are:

CO_2	12.00	8.50	9.50	9.20 per cent.	
CO	26.00	29.00	29.20	29.60 per cent.	
CO/CO_2	2.16	3.40	3.18	3.21	
H_2	3.60	4.20	4.00	4.00 per cent.	
B.t.u	101.70	113.50	109.50	110.80	Anal.
B.t.u	100.60	114.10	114.10	111.30	Calorimeter.

It will be noted that the CO/CO_2 ratio of the gases is high. This seems to obtain in such gases to increasing extent in proportion to the difficulty of burning the gas. The gas represented by the three last tests could not be burned without preliminary washing. We calculated from our analyses of the water solution, about 1 grain of cyanides to 1 cu. ft. of gas.

Whether the fume indicates the presence of finely divided chlorides or is due to cyanides, we agree with Mr. Wysor's conclusion that the inhibitory effect is mechanical and is due to the absence of intimate mixture of air and gas molecules. The high CO/CO_2 ratio observed in such gas probably has no bearing upon the observed phenomenon and is mentioned only incidentally.

J. S. UNGER, Pittsburgh, Pa.—On page 278 reference is made to 36 carloads of dust shipped. Did the material in that last sample come from a furnace running on pig iron, ferro alloys, or spiegel?

R. J. WYSOR.—Pig iron; we have not made any ferro alloys, or spiegel at Bethlehem in the last 3 years.

J. S. UNGER.—Do you use domestic or foreign ores?

R. J. WYSOR.—In regular practice, largely foreign ores.

J. S. UNGER.—How much per unit does it cost to produce that?

R. J. WYSOR.—You might say it does not cost anything; we do not change our blast-furnace practice at all to obtain the dust. Although it has been a profitable byproduct, I would rather not have it unless we could reclaim it on a large scale, as we would have better working stoves.

J. S. UNGER.—Does the trade take it as readily as natural salts?

R. J. WYSOR.—The fertilizer dealers have been anxious to get it. This did not happen, however, until the war started.

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J. S. UNGER.—They have been willing to take anything since the war started.

R. J. WYSOR.—Yes. However, we could have sold the material before the war; in fact, about 4 years ago we had a contract ready to sign, but we did not think it would pay us to bother with it then.

W. H. Ross, Washington, D. C.—We of the Fertilizer Division of the Bureau of Soils have been particularly interested in Mr. Wysor's investigations on the possibilities of recovering potash from the blast furnace, and we look upon the results he obtained as among the most important that have yet appeared on the subject of finding new sources of American potash. About 5 years ago we undertook a corresponding investigation on the recovery of potash in the cement industry. Representative samples of raw mix and ground clinker were collected from the different cement plants in this country with a view to analyzing each sample for potash. With the data thus obtained and knowing the ratio between the raw mix and the cement produced and the output of the latter, we thought it possible to calculate approximately for each plant the quantity of potash that escapes daily from the kilns. After partly completing the work, however, it had to be abandoned for a time. It was again taken up last fall, and has now been finally completed. We find that the potash that escapes from the kilns varies in different plants from 1 to upward of 5 lb. per barrel of cement produced, and that the total potash that escapes from all the cement plants in this country is in the neighborhood of 100,000 tons annually. The proportion of the total potash that is volatilized in different plants varies from about 25 to 85 per cent. If cement-kiln practice were standardized so that in all plants, say, 85 per cent. of the total potash were volatilized, the potash produced would approach 200,000 tons. It has been demonstrated that of the potash that escapes from the kilns at least 85 per cent. is recoverable by electrical precipitation and of that which is recovered perhaps as much as 95 per cent. may be considered as available. In at least one cement plant in this country the potash that escapes from the kilns is worth at present prices as much as, if not more, than the cement produced. The potash in the dust of some cement plants is for the most part water-soluble, but in others as much as half of the potash, as determined by the official methods, may be insoluble. In these plants part of the potash seems to undergo a recombination during the burning, with the silicates in the dust. Mr. Wysor shows that part of the potash in blast-furnace dust is also insoluble. In the case of cement dust we find that the recombined potash may be readily rendered soluble by digesting with water under somewhat increased pressure and by this procedure a more concentrated solution may be obtained in one treatment than is practical to secure directly by leaching with water in the ordinary way.

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About 6 months ago samples of dust collected in the stoves and boilers of a number of blast furnaces were sent us by the Research Corporation. On analysis, the potash content of the samples was found to vary from nothing to upward of 20 per cent. It might be expected at first thought that the potash in the samples might have some relation to the total potash escaping from the furnaces. This, however, is not the case. In cement plants using the rotary kilns the potash that escapes from the kilns passes off more or less uniformly, but Mr. Wysor has shown that in the case of the blast furnace the escape of the potash takes place irregularly; consequently, the potash content of a grab sample of flue dust cannot be depended on as representing the average composition of the dust. In coöperation with the Bureau of Mines, we are now undertaking an investigation designed to show as nearly as possible the average amount of potash volatilized annually at the blast furnaces of the country, and in this we are simply extending in detail the investigations which Mr. Wysor has so successfully carried out at the plant of the Bethlehem Steel Co.

L. E. RIDDLE.—I operate a furnace on ferromanganese and ferrosilicon, and, as Mr. Wysor's paper shows, the manganese ores contain about twice as much water-soluble potash as the other ores. I thought we could probably get a much larger percentage of potash, but we have not found more than 2.3 water-soluble potash in our ferromanganese spiegel dust. As a matter of interest, the dust from the checkers on our stoves, this is on manganese, furnishes 1.79; the dust from the combustion chambers, furnishes 1.9; the dust taken from the burners on the stoves furnishes 2.2; the dust taken from the goose necks on the stoves furnishes 1.9, and the average analysis of dust taken from the dust catcher would show slightly under 2 per cent. and as low as 0.8 per cent.

THE CHAIRMAN (JOSEPH W. RICHARDS, South Bethlehem, Pa.).—May it not be possible that the insoluble potash may be converted into soluble potash by the methods suggested?

L. E. RIDDLE.—That might be so; we have not gone into that.

CHAIRMAN RICHARDS.—I would suggest that possibly the insoluble potash in the dust product of a furnace running at a high temperature is due to the coincident volatilization of silica, which then forms potassium silicate. That may explain the large predominance of insoluble potash in the dust of the ferrosilicon furnaces.

F. G. BREYER, Palmerton, Pa.—The American potash industry, if there is going to be one, I think owes Mr. Wysor a big debt for telling us once for all what becomes of the potash put into the blast furnace. In the course of some work at Palmerton, I had occasion to investigate some slates and shales from the hard coal region around Hazleton, Scranton

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and Wilkes-Barre, and I found something that I could have readily found without any analytical determination, namely, that those shales and slate carried anywhere from 2 to 4 per cent. of potash. If you will look through the Geological Survey bulletins, you will find that 3½ per cent. is about the average potash content of shales and slates over the whole United States. Now, as a source of potash material, that looks like a very unpromising start. All the work so far on potash recovery has had to do with much richer ores than 2 or 3 per cent. Commonest among the processes that seemed promising are those involving the use of feldspar, in a blast furnace of some sort. There are plenty of feldspar samples that may run as high as 10 and 12 per cent. potash, but if a large tonnage is required the average will probably not be above 6 per cent. If this feldspar is put into the furnace together with enough limestone and coke to fuse the residue to a slag and volatilize the potash, the potash content of the charge will not be above 4 per cent. So after all, the above mentioned slate is not such a poor start as it might at first seem, particularly if one bears in mind that the slate not only has potash in it, but has enough carbon in it to fuse the whole mass down and flux the slag out of the bottom of the furnace. The only addition required is a small amount of limestone. Three or four patents have been issued to various parties for the manufacture of potash from feldspar or its ores in which the potash is volatilized with coke. Before I make that statement, I might say that nowhere, that I know of, are the coke and the feldspar or potash-rich ores in close proximity, so either the feldspar or the coke must be hauled. For this reason potash, from any American ore we have so far developed in which coke is depended upon for volatilizing the potash, is an impossibility unless potash remains at $500 a ton.

It was in considering the possibility of volatilizing the K_2O from this slate that both my good friend Mr. Wysor and myself found absolutely no data were available on the question of the behavior of potash in a blast furnace. Now that Mr. Wysor has published these excellent potash balances it is possible to predict with certainty what will happen to potash when charged into a blast furnace under at least several sets of very definite conditions.

The raw material I speak of is something that I think is well worthy the consideration of the furnace man when the iron trade is dull again. Here is a material that will carry 40 lb. of potash per ton. We know that an iron furnace will volatilize more at higher temperatures than lower temperatures. With a furnace working primarily for heat, and not to make iron, and without any reduction of iron ore in the neighborhood of the tuyères at all, the temperature we should maintain in such a furnace would be the limit attainable with the combustion of carbon. I feel that under those conditions a volatilization of 75 per cent. of the potash from that material is almost certain. Now, if you can charge into a 1,000-

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ton burden furnace in the form of slate, 40 lb. of potash per ton, on a recovery of 75 per cent. you produce 15 tons of potash a day.

One of the difficulties in the recovery of potash from furnace gases has been the matter of collecting; as Mr. Wysor's paper brings out, only 2 per cent. of the potash is actually deposited in his stoves and places where he can collect it; by far the greater percentage is caught in the first rough dry dust catcher, and a considerable percentage goes beyond. Up to the present time, the electrical precipitation seems to have offered the greatest chance of successfully recovering this very light potash fume. On the other hand, I am a firm believer that the one sure way of catching 100 per cent. of it is to use a bag house, preferably an automatic one.

A furnace as outlined above would be highly profitable at the present time, product 15 tons K_2O per day at $300 per ton, $4,500. What it would do after the war is a question that must be determined by experiment. For the benefit of those who are not familiar with our coal regions, I might add that there are millions of tons of this slate there and there will be millions and millions more. A blast furnace built near to some large colliery to treat this material seems to offer attractions. Blowing engines would be operated by the gas produced. Gas and slag would be byproducts. Prof. Richards suggested that the slag might make excellent material for pillars in the mines.

We have just taken out a patent on the use of this process, but at present have no means of carrying it out to a finish. It looks well on paper, and if we can persuade somebody to try it for a couple of months, it will certainly develop some information that is possibly of great value to the whole country, because if there is anything in the proposal, there is enough potash on those dumps and enough carbon to burn them, to supply this country with all the potash it needs.

The crux of the whole thing is simply in the fact that the raw material costs nothing and both potash and fuel are present in one raw material.

CHAIRMAN RICHARDS.—I would like to ask Mr. Breyer, if that were a bituminous shale, whether there would not be other valuable byproducts from the gas in the furnace?

F. G. BREYER.—There is that possibility in the bituminous shales. On the other hand, they would offer some difficulty in a dry dust-catching system. I would like to have the opinion of some of the blast-furnace men as to what they think of charging 1,000 tons of this material into a blast furnace that would produce roughly 700 tons of slag.

H. A. HUSTON, New York, N. Y.—I was impressed with the statement that the raw material would not cost anything. We have had that same expression about the cost in making alcohol out of sawdust, but somehow or other, when we went at the sawdust, it did cost something. I have grave doubts about any kind of ore being of any commercial

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importance which carries less than 10 per cent., or 8 per cent. perhaps, of water-soluble potash in its original condition, because of the freight and handling charges. All proposals for the fusing of feldspars or other material to make commercial potash must provide for writing off the entire plant before the shipment of foreign potash begins. These war prices are exceedingly interesting and attractive, but unfortunately there is no considerable market for it at war prices. One cargo would break the market without any trouble. We must take that into consideration. If you could write this entire plant off, as the plant at Hoffman has been written off, then the thing would be very profitable, but it takes a long time to pay for a building and plant. We have been wrangling about producing American potash since 1909, and the Hoffman plant is the only one that has done very much along that line in a profitable manner. That produces a 28 per cent. potash mixture which is profitable at the present time, although it is alkaline. The trade wants a neutral potash. I was also interested in the paper on the recovery at cement plants. The standardization of cement plants would operate them in such a way as to volatilize a greater percentage of potash. It is interesting from a chemical engineering standpoint, and one of my friends undertook to look into that and visited one of these plants that was volatilizing so much and found they were getting a high potash recovery, but some of the cement was coming back. They had destroyed their cement in trying to make potash. . He decided that so far as his company was concerned, they would not put that on their plant.

F. G. Breyer.—With regard to the value of this product, it has been lying there ever since the mining industry began. There is absolutely no use for it at the present time, as it is a decided disadvantage to the coal miner, due to the fact that it gives him a heavy overburden when he tries to mine his coal directly beneath those piles. He has to leave a good deal more coal under there than otherwise. Eventually those lands will become farming lands, then what is going to become of those piles? I know that it can be bought for less than 5 c., but I think, as a matter of fact, that the cost of taking it from the head frame out on the dump would be gladly turned over to any company that wanted to take it. The only question would be the disposal of slag, since there would be almost as much slag as original tailings, but, as Prof. Richards suggested, there is a very good use for that in the mines. And take such a case as the fire in the Mammoth vein at Lansford, where a big barrier was made out of cement at tremendous cost. That fire is still going on; they are attempting to put it out in a number of different ways. There is a possibility that it could be choked off with a slag barrier. But leaving the question of slag value out of it entirely, the process is independent of anything else but potash values, on the assumption that you are going to have enough gas to operate your blowing engine, which is perfectly logical,

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and that you will have some gas to sell to the neighboring boiler houses, which is likewise self-evident.

H. A. HUSTON.—Do you suppose you can produce potash for 40 c. a unit by that process?

F. G. BREYER.—That will depend entirely on the way the furnace works on this burden. The physical nature of the material charged has a great deal to do with it, and that is in our favor, because the material on those dumps is largely in good-sized pieces so that it at least ought not to choke up the furnace. There is only one way to go about it and that is to haul some of it to the nearest blast furnace and run it for a couple of months and find out how much potash is left in the slag. If 75 per cent. is driven out, it can be caught; we have caught it from blast furnaces even after it has been burned in the stoves. We caught a material from an iron furnace in the Lehigh Valley that averaged 14 per cent. of water-soluble potash, and we caught 100 per cent. of it economically from the gas leaving the stoves.

W. H. ROSS.—With regard to Mr. Huston's reference to cement manufacture, I might say that the manager of one of the cement plants which has introduced the Cottrell process for precipitating the dust, told us that since the precipitator was installed, they are able to manufacture their cement more efficiently than they did before, instead of less efficiently.

CHAIRMAN RICHARDS.—As far as the possibility of smelting that material in the blast furnace is concerned, it is only a question as to its chemical composition. If it contains enough carbon to reduce by smelting, and is not too high in magnesia, it can be fluxed. There is no doubt, from the blast-furnace end, that it can be smelted; it is nothing much more than a slagging gas producer.

G. A. ROUSH.—This paper interested me from the statistical standpoint particularly. Mr. Wysor's balance sheet shows 22.4 lb. of potash entering the furnace per ton of charge. At the present rate of making pig iron in this country, that is equivalent to almost 400,000 tons of potash per year, assuming that the charge was uniform for the entire country, which, of course, it is not, not to mention something over 600,000 tons of soda, both in the form of oxide. Of this only a small proportion was actually caught. On the basis of the small proportion caught, in this one plant in the course of a few months, the recovery would amount to something like 15,000 tons for the entire blast-furnace capacity of the country; and this evidence, in conjunction with the manifest possibility of increasing the recovery, makes a rather promising commercial proposition out of the operation. Even accepting Mr. Wysor's caution that this charge at Bethlehem carries potash above the average for the country, there is still left a considerable amount of potash to draw on. The

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imports of potash from Germany in the year ending July 1, 1914, were approximately 250,000 tons, and in the year following that, only a little over 200,000 tons. The latter figure was considerably reduced due to war conditions, and later importations are much smaller still. This leaves us then with a normal annual importation of about 250,000 tons as a figure for comparison with the possible savings from the blast-furnace operation, providing means can be provided for the securing of a greater saving of the potash carried over from the furnace.

Then, Mr. Wysor's second caution as to the cost of extraction he seemingly puts in as a caution to not take things as coming too easy, but does not point out some of the advantages on the other side. He says it is going to cost more in order to get a greater recovery, which is true; but in spite of this extra cost we have the promise of methods for producing greater recoveries, particularly dry cleaning, and this sort of a process is not only going to give you a better yield of recovered product, but, as pointed out by Mr. Bradley's earlier paper at this session, it is going to give you the advantage in the operation of the blast furnace on the hot, dry blast, which can be counted as an additional advantage in favor of the collection of the product.

H. A. HUSTON.—The maximum importation to this country in any year was never more than 260,000 short tons of actual potash. About 1,125,000 tons of potash salts are imported. I think a good many of our people have been misled in this country about the consumption of potash by some curious statistics that appear, which are all very high. The chemical consumption in this country is not over 11,000 tons of actual potash.

F. G. BREYER.—I do not think the question of past consumption concerns us so much as the question of future consumption, and 200,000 tons year before last is not the question; the question is, what we are going to import 10 years from now. If we could produce it at any sort of reasonable figure, 200,000 tons would not be more than a fifth of the use, because our farmers are not using the potash they should.

WALTER A. SCHMIDT, Los Angeles, Cal. (written discussion).—Mr. Wysor brings out the interesting consideration that a heavy or dense fume interferes with the proper combustion of blast-furnace gases, particularly when used under the boilers. He makes the suggestion that this results from the breaking up of the continuity of the gases causing a large increase in the distance over which the flame must be propagated. He also says that an exceedingly large number of finely divided fume particles may prevent proper contact of the gas molecules and thus prevent proper flame propagation, but it would hardly seem likely that such a fine state of division of solids could be reached as to interfere with molecular contact.

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I wish to suggest that which to me seems an important factor in the phenomenon discussed by Mr. Wysor; namely, the effect of screened radiation. Any solid will absorb a portion of the radiant energy impinging upon its surface, dark bodies absorbing more than light ones. If a particle absorbs heat waves, it then itself becomes a center of radiation, but being of a lower temperature than the source of the primary radiation, it establishes proportionately a much feebler radiation in any particular direction. An incandescent body or a flame will have its free radiation greatly checked or retarded if surrounded by a screen of such solid particles. This can easily be demonstrated by simply inserting a wire-mesh screen between an intensely hot flame and one's face, this instantly causing a cessation of the burning sensation. I have often used this well-known means of protection when observing operations within cement kilns and certain metallurgical furnaces. Comparatively coarse wire cloth, for example 10-mesh, is quite sufficient to screen the intense radiation yet at the same time permit of ready observation through the openings of the fabric. This is not a phenomenon of heat absorption by the metal of the wire cloth, as the shielding action continues indefinitely.

My thoughts in connection with Mr. Wysor's disclosure is that the particles composing the fine fume shield the radiation of the flame and prevent its propagation to any great distance from the center of ignition. Under certain conditions this action could probably be sufficiently pronounced to extinguish the flame altogether. Mr. Wysor's statement that the phenomenon under discussion is less noticeable in the stoves than under the boilers is significant, as the incandescent checkerwork in a stove undoubtedly decreases the importance of radiation from the center of ignition as a means for maintaining proper flame propagation.

I offer this suggestion for what it may be worth, being in total ignorance of first-hand information on iron blast-furnace operation.

F. G. Breyer.—I cannot agree with that explanation, since it does not seem to correspond to the facts in a similar case. We can burn zinc vapor readily to a very dense fume containing 10 to 20 times as many solid microscopic particles per unit of volume as blast-furnace gas, and therefore with that much greater screening action than offered by the particles in the case of the dirty blast-furnace gas. My opinion as to the cause of it is that it is inherent in the potash itself. The fume when it comes into the boiler is relatively cool and the potash is there as a solid. If you suddenly generate heat and vaporize the potassium chloride, the latent heat of vaporization is way above that merely required to heat up solid particles to the temperature of the products of combustion, which heat absorption may sufficiently retard the flame propagation to cause the effect noted.

J. S. Unger, Pittsburgh, Pa.—Mr. Wysor has presented a paper on a comparatively new subject, which is not only very interesting, but

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of great importance to the whole country as well as to the iron and steel industry, as it furnishes a new source for potash.

The shortage in the potash supply has led to the investigation of a great many materials as a possible source of potash, and it is a question to which we have given considerable attention, our investigations covering various waste products from six of our plants with a view to recovering the potash or using the waste product itself as a fertilizer.

Our studies have covered the determination of the potash in blast-furnace slag, water from slag-granulating pits, blast-furnace flue dust, deposits in blast-furnace boiler combustion chambers, in flues leading from the boilers to stack and deposits in the stack, deposits in the hot-blast stove combustion chambers and the dust accumulations in the last passes of stoves or flues near stack, open-hearth furnace slag and the deposits in the regenerator chambers of open-hearth furnaces.

Our blast furnaces operate principally on a burden of Lake Superior ores, Ohio and Pennsylvania limestone and Connellsville coke, and the amount of potash in our various waste products would naturally differ somewhat from that found at Eastern blast furnaces which are run on an entirely different burden.

We found that our blast-furnace flue dust carried only a few tenths of a per cent. of potash; the blast-furnace slag, about ½ per cent.; water from slag-granulating pits, traces; deposits in blast-furnace boiler combustion chambers, about ½ per cent.; and deposits from the last passes of hot-blast stoves, from 1 to 1½ per cent. In a general way, the potash we found in the various materials was either insoluble or, in the most favorable cases, about one-third water-soluble.

Considering our results as a whole and the amount of the various products produced, a sufficiently high percentage of potash was not found to make it desirable to attempt to recover the potash from such materials.

Sometimes the potash is fixed, passing through the furnace, and is found in the slag, at other times it appears to be easily volatilized and is found in the largest quantities in such portions of the furnace as are comparatively cool, such as the gas flues, regenerator chambers or stack. Occasionally we have found in locations quite distant from the furnace a few ounces of a grayish or reddish fume very high in potash, but the quantity is too small to be considered. As an example, we found 58 per cent. of potash in a fume adhering to a blast-furnace stove chimney valve, 21 per cent. on the brick near the stack in a blast-furnace boiler, 20 per cent. in a fume in a boiler stack, 43 per cent. in a deposit on the piston of a gas engine, and 7 per cent. on the vanes of a Theisen gas washer. These figures indicate that the potash is carried for quite a distance to a cool spot before most of it is deposited.

From Mr. Wysor's work, it appears that there would probably be a larger yield of potash from furnaces working on certain alloys, and we

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are at present making determinations of the amount of potash in various deposits around furnaces working on ferrosilicon and ferromanganese.

R. J. WYSOR.—I think I can account for the difference in practice as found by Mr. Unger and Mr. Riddle. As noted in my paper, the western blast-furnace burdens are lower in alkali content than those at Bethlehem, ours being an exception, and in the second place, their hot-blast and top temperatures are lower than ours. These temperatures largely determine the proportion of alkalies discharged from the cinder notch, and from the top of the furnace. I believe, Dr. Unger, you found more potash in your slags, than we did at Bethlehem.

J. S. UNGER.—Half of 1 per cent.

R. J. WYSOR.—We found less than 0.4 per cent., as an average in the slag, with our burden richer in potash. Furthermore, if your investigation of the potash collected at the various points mentioned was made on dry gas, I can state positively that most of the alkalies escaping from the furnace passed out through the stove and boiler stacks. If you investigated washed gas, then most of the alkalies were carried out by the wash water in the primary washers. The more perfect the performance of the primary washers, the less potash-bearing dust will be recovered from stove and boiler passes.

CHAIRMAN RICHARDS.—Enough attention probably has not been paid to the function of the vapor tension in the volatilization of these constituents. We know very little about the vapor tension of these salts, but the question as to whether they go out at the top or come out at the slag notch is largely a question of the temperature at the top; the vapor of the salt is carried as vapor, at a very small partial tension, up the furnace, and if the top temperature is low, you carry out proportionately less; you cannot carry more than a very small amount.

In the combustion of the gas in the stoves, is it not possible that the enormous number of small particles which Mr. Wysor speaks about in his paper act as such efficient radiators of heat that they check the development of a high temperature in the gas? They may radiate the heat so efficiently and with such enormous radiating surface that they check the rise in temperature and keep down the actual temperature of the gas.

CHARLES CATLETT, Staunton, Va. (communication to the Secretary*).—I hesitate to discuss in general terms a paper such as Mr. Wysor's, which bears evidence of so much careful and detailed investigation, yet it is the broad possibilities which his paper presents that are really attractive and his cautious conservatism may make us unappreciative of these possibilities.

Mr. Wysor calls attention to the fact that the iron-ore mixture which

* Received Mar. 12, 1917.

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he uses, carrying 0.28 per cent. of potash, is probably above the average. No doubt this is true, but on the other hand it may be very much less than the mixture at some blast furnaces or very much less than would be the case with special mixtures bearing the question of potash in mind. Unfortunately, iron ores are not ordinarily examined for potash, and with the ordinary analyst there is opportunity for very considerable error in the determination. The 10th Census gives 270 complete analyses of iron ore, on the accuracy of which we may rely. Of these, there are 21, or 8 per cent., that carry more potash than Mr. Wysor's mixture; 14 of these carry approximately twice as much, or more; eight carry approximately three times as much, or more; three carry approximately five times as much; one carries over seven times as much. It may be worth while to take some of the ores that are known to be high in potash and consider the possibilities in the light of Mr. Wysor's investigations.

For instance, there are certain red hematites in eastern Alabama (locally known as "gray ores") which are very high in potash. They are in large quantity, and are low in phosphorus for Southern ores; but have so far not met with general favor because rather siliceous. While shipments have been made of 50 per cent. metallic iron, they would ordinarily run nearer 45 per cent. with the correspondingly high siliceous contents. The average of the analyses of a number of samples, by well-known chemists, gives a K_2O contents of 2.08 per cent., or over seven times the potash contained in Mr. Wysor's mixture. The exact iron contents of his mixture is not disclosed; but if we assume it to be 52 per cent. it would mean that for each unit of iron there are 0.0054 units of potash. For the Alabama gray ores with a basis of 45 per cent. metallic iron there would be for each unit of iron 0.046 units of potash, or nearly nine times as much in proportion.

I am unable to find any typical analyses of potash in Southern cokes. As they commonly carry more ash than the coke used at South Bethlehem, it is quite possible that the potash is higher. Probably 75 per cent. more coke would be used in Alabama per ton of iron. The percentage of potash

TABLE 1

		Pounds
Charged into furnace per ton of iron		
From ore		87.7
From coke and stone		18.97
		106.67
Loss from		
Fumes, etc	0.79	
Dust-catcher	2.82	
Top furnace	4.76	
Slag	7.87	16.24
		90.43

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in the flux used at South Bethlehem is not uncommon. More flux would also be used in Alabama; probably twice as much. This would also serve to increase the potash. But it is very conservative to assume 50 per cent. more coke and 75 per cent. more flux, and if we assume the potash contents in the slag to be the same as at South Bethlehem and the same proportionate loss in the slag, there would be derived, from the coke and flux 11.10 lb. per ton of iron, above what would go out in the slag. Assuming a similar loss from leakage, we would get Table 1 as representing the potash in the gases that would go to the treaters.

How much can be saved?

I am advised that at the cement plant at Riverside, Cal., where the gases from the kilns are carried through a Cottrell precipitator, as much as 95 per cent. is commonly saved. At the Security Cement & Lime Co.'s plant at Hagerstown, Md., with which I am associated, the waste gases from the kilns are passed through a Cottrell precipitator. The balance shows that we commonly save 90 per cent. of the material that actually goes into the treater. As the volume and temperature of the blast-furnace gases are much less, it ought to be possible to do better than at a cement plant; but certainly 85 per cent. recovery is conservative. This would mean a saving of 76.86 lb. of K_2O per ton of iron produced.

It may be silly to note that at the present common and current prices for potash this would be worth $11 or $12 per ton of iron produced; but we cannot ignore the fact that with the destruction of shipping, and the high freight rates, potash must remain high for a number of years. Even at pre-war prices a furnace working on the gray ores of eastern Alabama, or a similar ore, with a byproduct recovery plant, would have a credit of from $2 to $3 on the cost of the iron from the potash saved. This would be in addition to the collateral advantage derived from cleaning the gases.

In addition to this, there are a number of furnaces which also carry zinc and other valuable materials in the escaping gases.

It is difficult to escape the conclusion that in future blast-furnace development consideration will be given to the percentage of potash in the raw material.

J. S. UNGER, Pittsburgh, Pa. (communication to the Secretary*).—In discussing Mr. Wysor's paper, I said that we were investigating the deposits around furnaces working on certain ferro alloys.

Since the meeting, we have made a number of investigations, and have found that the deposits in the second and third passes of the hot-blast stoves, and near the stack at the boilers of furnaces working on spiegel and ferromanganese, contain about 15 per cent. total potash, of which 10 per cent. is water-soluble. The potash occurs mostly as potassium sulphate, a very small amount being present as potassium carbonate.

* Received Apr. 28, 1917.

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S. G. VALENTINE, Oxford, N. J. (communication to the Secretary*).—Mr. Wysor's interesting paper is a valuable contribution to the important effort to conserve byproducts. The present world conditions make this particularly important. The investigation drew my interest, when I learned from him that it was being conducted, particularly because it included an incident that occurred some years ago when we all knew less about these more obscure incidents of blast-furnace operation than we do today. I related the occurrence to Mr. Wysor and as he refers to it in his paper, with the hope that it may be enlarged upon in the discussion, I am led to relate it more fully.

On one occasion at the Colebrook Furnaces at Lebanon, Pa., then owned by Robert H. Coleman, we were puzzled by the sudden tendency of all our gas burners at the boilers to clog with a deposit which rapidly closed them. We were accustomed to be troubled with the fumey gas which Mr. Wysor's paper refers to and which would at times hardly burn at all or only in a way which I can liken best to the appearance of worms of flame in a white cloud of fume. The gas would be lit and burn in small separate volumes about the size and shape of a man's finger curved, flash for a second and go out, giving place to others—a rather unsatisfactory procedure so far as keeping up steam was concerned, especially as those were the days of the long three-decked cylinder boilers. This clogging of the burners, however, was something entirely new. We worked for several hours to clear the gas passages with steel bars but the accumulation gained on us until we had to stop the furnace, take down the burners and chisel out the deposit. As it was removed it rapidly absorbed moisture and the odor of ammonia was very strong all around our cleaning operation. After several hours the material was all chipped out, the furnace started again, and thereafter, so far as my memory serves, we were never again troubled with the same thing. The material that clogged the boilers was chiefly alkaline cyanides, and the remarkable thing was that they appeared suddenly and in such large quantity. Just why this should have been is hard to say, but the interior condition of the furnace must have been such as to promote their formation in far greater quantities than normal, and so far greater that instead of being decomposed by chemical action in the upper part of the furnace they poured out with the gas, and condensed and collected in the gas burner passages.

* Received June 8, 1917.

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Dry-Hot versus Cold-Wet Blast-Furnace Gas Cleaning

BY LINN BRADLEY,* H. D. EGBERT† AND W. W. STRONG,‡ NEW YORK, N. Y.

(New York Meeting, February, 1917)

Introduction

MARKED differences of opinion have been expressed by engineers interested in cleaning iron blast-furnace gases for use in hot-blast stoves and under boilers, in reference to the advantages of a hot-dry method over a cold-wet method. One point at issue involves the sensible heat energy in the moisture contained in the gas. Some advocates of the cold-wet methods claim that the condensation and resultant removal of the greater portion of this contained moisture by wet scrubbing, spraying or similar methods, results in a saving of some of this sensible heat energy, by reason of the fact that water vapor has a high capacity for sensible heat energy and may carry from the exit of a hot-blast stove, for example, more heat units than are sacrificed or lost when the gas is cleaned by this cold-wet method. Other advantages claimed for the latter method are: That gas burns more readily when it is free from moisture in any form; that because gas is made denser by cooling and removing the moisture it has a higher calorific value than hot gas carrying moisture; and that higher flame temperatures are obtained when the gas is cleaned by the cold-wet method.

A search through the available literature fails to disclose any extensive calculations or records of conditions obtained in practice, bearing upon these phases of gas cleaning.

This paper deals with these problems from the standpoint of the economy of using a gas of higher flame temperatures, of improved stove design, and of economy in the gas-cleaning department. Calculations have been made which show that by cleaning the gases by a cold-wet method the sensible heat energy of the blast-furnace gases is greatly reduced; and, in general, this loss of heat energy is far greater than that lost from the stoves or boilers due to the sensible heat capacity of the water vapor if carried away by the exit gases. By making use of the

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latest values given for the mean specific heat of gases, by Kuzell and Wigton,[1] it will be shown that for a typical iron blast-furnace gas the sensible heat energy which is lost from the gas by the cold-wet cleaning process is in all cases much greater than the gain resulting from the removal of moisture, even when the moisture content of the furnace top gases is as great as 100 gr. per cubic foot of the gas, calculated at 32° F., due consideration being given to the quantity of sensible heat energy which such an amount of moisture is capable of carrying from a hot-blast stove or boiler throughout the usual range of exit gas temperatures.

Returning to the subject of the effect of water vapor on flame temperatures, the paper shows that it would be true, as claimed by some iron blast-furnace operators, that the removal of moisture from the gases prior to combustion results in a higher flame temperature, provided no sensible heat energy were removed from the gas along with the moisture, due to cooling the gas. It is shown, however, that for typical kinds of iron blast-furnace gas, the theoretical flame temperatures are considerably higher when the cleaning method allows both the sensible heat energy and the moisture to be kept in the gas. The ideal cleaning process would be that of removing the moisture without reducing the temperature of the gases, but as no process of this kind is available it will be obvious that, other things being equal, it is better to clean these gases hot and dry and leave the moisture in the gases than to cool the gases for the purpose of cleaning and of removing water.

The gain resulting from the conservation of the sensible heat energy of the gas entering the stoves also permits of other changes in practice that appear on the whole to be advantageous. By having hot gases enter the hot-blast stoves, it is possible to obtain higher flame temperatures in the stoves. This will result in a saving of coke in preparing the furnace charge, since a hotter blast will be obtainable in the same length of time with the consumption of the same quantity of dry top gas from the blast furnace. This hotter blast not only supplies additional heat energy, but it makes possible the attainment of higher temperatures within the furnace, and this in turn makes possible a reduction in the amount of coke charged. This effect is best understood by a reference to the excellent article by Walther Mathesius entitled "High Blast Heats in Mesaba Practice."[2]

Furthermore, by employing higher temperatures it should be possible to store up the same quantity of heat energy in the checker work of the stoves in less time than is now required. By carefully cleaning the gases hot-dry and by thoroughly mixing the air for combustion with the blast-furnace gases a minimum amount of excess air is required. To further increase the flame temperature, it may be feasible in some cases

[1] *Trans.*, vol. 49, p. 774 (1914).
[2] *Trans.*, vol. 51, p. 794 (1915).

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to utilize the heat in the stove exit gases for preheating the combustion air. It is obvious that the hotter the gas and the preheated air for combustion, the higher will be the flame temperature; and the same will also be true, the smaller the amount of excess air. The effect of water vapor, temperature of the gas, temperature of the combustion air, and the amount of excess air upon flame temperature is graphically shown by curves.

The curves have been plotted from a set of tables prepared from calculations based on the data on specific heats given by Kuzell and Wigton in the paper previously referred to. These tables can be applied to a large number of problems such as those in connection with hot gases coming from Portland cement kilns, smelter furnaces, cupolas, etc., where data on the sensible heat energy in the gases are required. It is possible that these tables may be submitted to the Institute for publication in another paper.

There appears to be so much room for improvement in the design and structure of hot-blast stoves that the subject has been discussed in a separate paper.

The total heat energy in a gas includes the heat which may be developed by combustion (chemical energy), the heat due to the temperature of the gas (sensible heat), and the latent heat of vaporization. The first type of energy may be called "the heat energy of combustion" and the second type "the sensible heat energy" of the gas. The heat energy of combustion is a function of the composition of the gases. For a definite composition of gases it is practically constant and can be readily calculated. The sensible heat energy of a gas depends upon the quantity of gas, the volume and temperature of the gas and the mean specific heat of the gas.

The specific heat of a gas in turn depends upon the temperature of the gas and its chemical composition.

In the present discussion 1 lb. of a typical dry, clean top blast-furnace gas is taken as the unit, and it is assumed to have the following percentage composition by weight:

	Per Cent.
CO_2	21.00
CO	24.00
H_2	0.25
CH_4	0.25
N_2	54.50

The presence of moisture, of dust and of excess air is measured in terms of the quantity of this foreign material per pound of such dry, clean top gas. The sensible heat energy of moist, top gas is, therefore, according to our method of calculation, the sensible heat energy of 1 lb. of dry top gas plus the heat energy of the moisture which accompanies and is in addition to 1 lb. of the gases which constitute the dry top gas; *i.e.*, the heat energies are added together.

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In practice, the amount of moisture or dust in a gas is frequently measured in grains per cubic foot of gas. This method of measuring the moisture or dust content of a gas usually assumes that the moisture or dust is computed at a certain temperature of the gas such as 32° F. Thus take 1 lb. of dry top gas with 20 gr. of dust per cubic foot of gas calculated at 32° F., and 50 gr. of moisture per cubic foot of gas calculated at 32° F. This unit of gas would contain 0.0349 lb. of dust and 0.0874 lb. of moisture. The unit of gas considered would consist of a total of 1.1224 lb. of matter, and a calculation of the sensible heat energy of 1 lb. of dry, clean, top gas with the above moisture and dust content would involve the calculation of the sensible heat energy of 1.1224 lb. of matter. The present paper, however, compares two methods of cleaning, the dry-hot and the cold-wet methods. Since both methods presuppose the removal of dust, it is not necessary to consider the sensible heat energy of the dust. In the above example the sensible heat energy would be calculated for 1.0874 lb. of matter, the dust being excluded.

In the above example the amount of dust and moisture per actual cubic foot of gas decreases with rise in temperature, for the reason that the gas expands. At atmospheric pressure 1 cu. ft. of gas at 32° F. will become 2 cu. ft. at 523° F. and in the example of moist, dusty gas given above, the dust content of 20 gr. per cubic foot of gas calculated at 32° F., will fall to 10 gr. per actual cubic foot of gas at 523° F., while the moisture content will fall from 50 gr. per cubic foot, calculated at 32° F., to 25 gr. per actual cubic foot at 523° F., though the percentage of dust and moisture per pound of dry, clean, top gas has remained the same. The measurement of the density of dust and moisture in a gas is, therefore, made by calculating how many grains of each 1 cu. ft. of gas would contain if reduced in temperature to 32° F., the pressure being standard at 760 mm. of mercury.

For purposes of combustion it is necessary to add a certain minimum weight of air per pound of the clean, dry top gas. After combustion the chemical composition and the specific heats of the gases have been completely changed. The datum point will be taken as 60° F., and the sensible heat energy of the exit stove gases will be the amount of heat energy that the products of combustion of 1 lb. of dry top gas plus the specified excess air plus the specified moisture entering the stove with the gas and air would emit when cooled from the specified temperature of the exit gases down to 60° F. In these calculations it will be assumed that the moisture content of the stove exit gases is not great enough at any time to result in condensation of any of the water vapor at the temperature at which the mixed gases actually leave the stoves. In practice, the moisture content would seldom if ever reach such an amount.

The latent heat of the water vapor need not be considered because it is lost in any cleaning method which can be adopted. In the cold-wet

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method the latent heat of the water vapor is absorbed by the water used during the washing process and is thus carried away by it; in the dry-hot method the latent heat of the water vapor is carried out with the stove exit gases and thereby lost.

Further, in using 60° F. as the datum point for calculations of the sensible heat energy of the exit stove gases or of the blast-furnace top gas after coming from the cleaner, it will be assumed that the gas contains 5 gr. of moisture per cubic foot of gas calculated at 32° F. This means that the sensible heat energy is referred to that of the same gas practically saturated with moisture at 60° F. Such a datum point is convenient because any kind of a gas cleaned by the cold-wet method usually comes out at about 60° F., and is practically saturated with moisture. As our comparison is made between a dry-hot and a cold-wet method of cleaning it is natural to assume conditions prevalent in the cold-wet method as the datum point.

Consider 1 lb. of dry, clean, top gas at 700° F., containing 20 gr. of dust and 25 gr. of moisture, both calculated per cubic foot of gas at 32° F. Our unit of dusty and moist gas weighs 1.0787 lb. It is found that the sensible heat energy in the 1 lb. of top gas and that in the moisture (which would be lost if the gas passed through a cold-wet cleaning apparatus, thus reducing its temperature to 60° F.) would be 174.69 B.t.u. = Qt. By the cold-wet method of cleaning that cools the gas to 60° F., the above Qt units of heat energy are lost for every pound of dry top gas, plus a specified moisture density.

In the dry-hot method of cleaning, no material lowering of temperature of the combustible gases for the stoves need take place. This condition is practically feasible when the electrical method of cleaning is used because the electrical precipitators need not be more than 15 or 20 ft. in length. The length of the gas mains and connections need not, therefore, be greatly increased, and, furthermore, they may be insulated so as to conserve the heat energy of the gases.

In the dry-hot method of cleaning the 25 gr. of moisture per cubic foot of gas, mentioned above, remain in the gas, and are carried into the stoves and then out with the products of combustion. The only difference between the exit gases from dry-hot cleaning and cold-wet cleaning in the present example is that in the one case there are 20 gr. of moisture per cubic foot of gas standard more than in the other case. Let us assume that the exit gases leave the hot-blast stoves at 600° F., which is a fair average. With the hot-dry method of cleaning, these gases will carry away 8.83 B.t.u. of sensible heat energy for every pound of dry top gas over and above what the same gases would have carried out had they been cleaned by a cold-wet method, due to the greater amount of moisture left in the gas when cleaned by the hot-dry method. Let this energy be Qe. The saving in sensible heat energy by the hot-dry method of cleaning as compared to the cold-wet method is:

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$Qt - Qe = 174.69 - 8.83 = 165.86$ B.t.u. per pound of dry top gas.

In the above comparison any energy changes due to expansion or contraction of the gases can be neglected because the exit gases are under practically the same condition of pressure and temperature for both methods of cleaning.

Assuming a ton of iron to represent a production of 12,000 lb. of such typical top gas, a hot-dry method of cleaning the gases would conserve:

$12{,}000(Qt - Qe) = 1{,}990{,}320$ B.t.u. per ton of iron in the above example.

Data and Theory of the Conservation of the Sensible Heat Energy of Gases

In the present article the term "grains of dust per cubic foot of gas calculated at 32° F., or 0° C." will be spoken of as "grains of dust" or as "grains of dust per cubic foot." "Grains of moisture" will mean grains of moisture per cubic foot of gas when the temperature of the gas is calculated at 32° F., and the pressure is 760 mm. of mercury. As previously stated, the moisture content is separate and in addition to the typical gas.

Cp is the specific heat of a gas at constant pressure and represents the ratio of the amount of the heat energy required to raise the temperature of a unit mass of the gas one degree compared with that required to raise the temperature of the same mass of water one degree (from the temperature of maximum density).

Cm is the mean value of Cp between the temperature t_2 and t_1.

The equations used for calculating the sensible heat energy of various gases are obtained from the following equations, where t is the temperature Fahrenheit. These equations were used by Kuzell and Wigton. For references as to the experimental determination of the constants in these equations the reader is referred to that part of the bibliography pertaining to specific heats.

Mean Specific Heats of Gases under Constant Pressure

(To 2,000° C. or 3,600° F.)

	Cm. (0 to t) for 1 lb. in B.t.u.
N_2	$0.2411 + 0.0000099t$
O_2	$0.2111 + 0.0000087t$
H_2O	$0.4701 - 0.0000118t + 0.0000000127t^2$
Air	$0.2342 + 0.0000096t$
CO	$0.2413 + 0.0000099t$
H_2	$3.3381 + 0.000124t$
CO_2	$0.1975 + 0.0000388t - 0.0000000043t^2$
CH_4	$0.5904 + 0.0000411t$

The term "flame temperature" in this paper will refer to the temperature that a gas will attain when one assumes that the energy of combustion is added to the sensible heat energy of the gas; all the resultant heat energy being used to raise the temperature of the gas, calculated on the basis of the values of the mean specific heat as given by the above

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equations. The actual flame temperature will be lower because some of this heat energy is conducted or radiated away before complete combustion takes place. The pressure of the gases is considered to be constant and standard. Slight changes in pressure would make only very small differences in the calculations and conclusions.

Some Conclusions Derived from Sensible Heat Data

1. The mean specific heat at constant pressure increases for all gases so far as known, as the temperature is increased. In some instances, values of the specific heats at constant pressure are assumed for temperatures as high as 3,600° F. Undoubtedly, the measurements of specific heats at these temperatures are not as yet very exact, but it is probable that the results are of the right order of magnitude and therefore do not invalidate any conclusions drawn from them which are essentially qualitative. The conclusions are also applicable to gases of quite a range of composition, the results being worked out in a similar manner to that used here in the typical example.

2. In general, the sensible heat energy lost in cooling iron blast-furnace gas by a cold-wet method of cleaning is much greater than that carried out with products of combustion by the moisture left in the gases in a hot-dry method of cleaning. Consider the curves in Fig. 1. In E and E' the typical blast-furnace top gas has a moisture content of 100 gr. From curve E it will be seen that if the top gas enters the wet cleaner at 300° F., it will lose 78 B.t.u. per pound typical top gas when cooled to 60° F. From curve E' it will be seen that if the same gas is burned without reduction of moisture content and the product of combustion leaves the stove at 1,000° F., the accompanying moisture will carry away 74 B.t.u. per pound of typical top gas. Even under these very exceptional conditions, a hot-dry method of cleaning would result in a saving of 4 B.t.u. per pound of typical top gas.

Consider the other extreme condition. Assume the blast-furnace gas to enter a hot-blast stove at 900° F. From curve E it will be seen that on cold-wet cleaning 284 B.t.u. would be lost per pound of typical top gas. Also assume that the gas leaves the stove at 1,000° F. Then from curve E' it will be seen that the moisture accompanying 1 lb. of typical top gas will carry away from the stove 74 B.t.u. In this case there will be a saving of 210 B.t.u. per pound of typical top gas. The other curves show results more favorable to the hot-dry method of cleaning, since the moisture contents are less. In practice the average moisture content seldom exceeds 35 gr. per standard cubic foot of typical top gas, even when the stock in the furnace has been watered to reduce dust losses.

3. The effect of moisture on flame temperatures is given in the curves in Fig. 2.* The effect of increasing the moisture content from 10 gr. to

* Based on calculations from the formulæ and curves of Kuzell and Wigton, *Trans.*, vol. 49 (1914).

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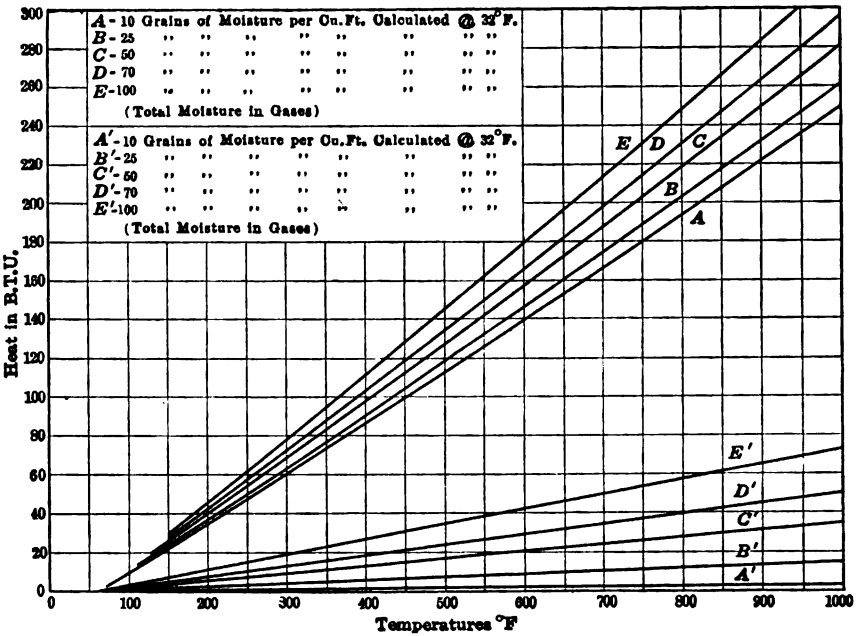

FIG. 1.—SENSIBLE HEAT CARRIED OUT BY MOISTURE IN STOVE EXIT GASES.

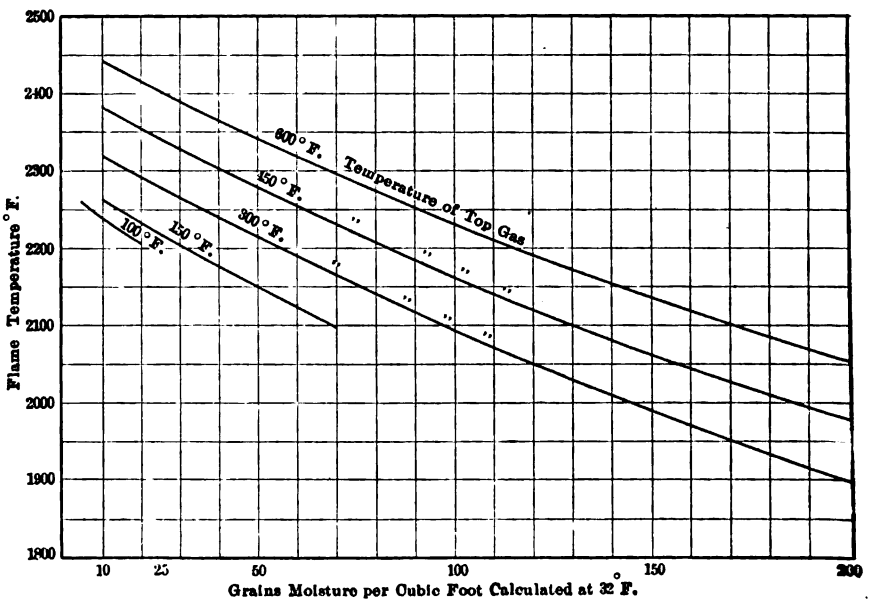

FIG. 2.—EFFECT OF MOISTURE IN FURNACE TOP GASES ON FLAME TEMPERATURE.

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100 gr. would be to lower the flame temperature produced by burning the typical gas from 2,320° F., to 2,090° F., for the gas entering the burner at 300° F., and from 2,460° F. to 2,230° F., for the gas entering the burner at 600° F. Now suppose that these gases had been cooled to 60° F. and the moisture thus reduced to 5 gr. The flame temperature would then have been about 2,250° F. In other words, a cold-wet method of cleaning would have resulted in slightly higher flame temperatures in the above cases provided the moisture content was 100 gr. or greater. For a 50-gr. moisture content the cold-wet method would give a higher flame temperature if the top gases were originally at 300° F., whereas if they were at 450° F., or 600° F., or over, the hot-dry method of cleaning would

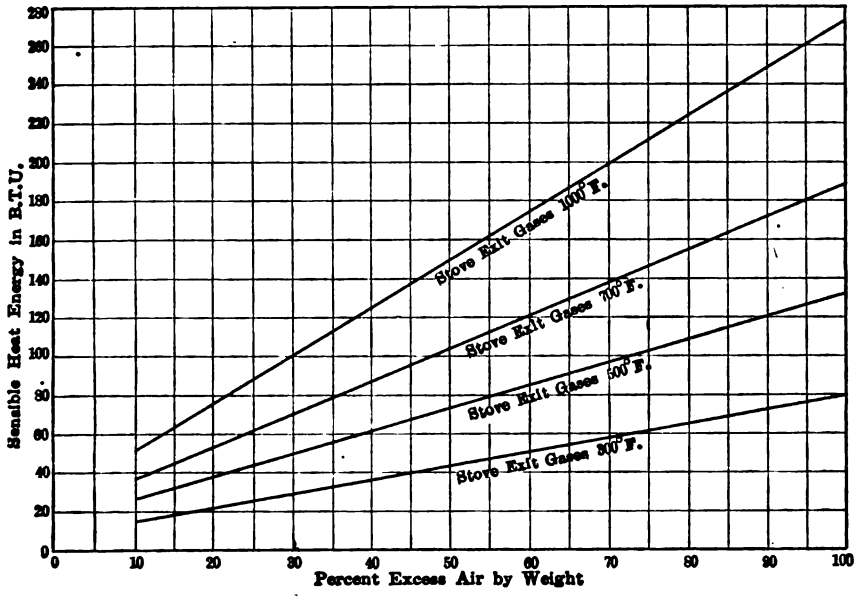

FIG. 3.—EFFECT OF EXCESS AIR ON AMOUNT OF SENSIBLE HEAT ENERGY ABOVE 0° F. CARRIED AWAY WITH STOVE EXIT GASES.

give the higher flame temperature. From the point of view of flame temperatures, there are certain temperature and moisture-content limits, on one side of which a hot-dry method of cleaning gives the higher flame temperature, while on the other side the cold-wet method of cleaning gives the higher flame temperatures. Operating conditions quite usually lie in the hot-dry region, as can be seen by a study of the curves.

4. The effect of excess air in increasing the amount of sensible heat energy carried out from the stoves and boilers by the exit gases at different temperatures is given in the curves of Fig. 3. These curves assume a moisture content for the top gas of 35 gr. (which is probably a fair average). The relative loss of heat energy due to excess air increases as the temperature of the exit gases is increased. The curves show how

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very important it is to keep the excess air down to a minimum. Thorough mixing of air and gas, and having the mixed gases hot on entering the stoves and boilers, are important essentials, and some automatic device for regulating the air input for variations (due to furnace changes) in the quantity and composition of the blast-furnace top gases should be used.

5. The sensible heat energy in the constituents of the stove exit gases resulting from the burning of 1 lb. of dry top gas, carrying 35 gr. of moisture with 60 per cent. excess air, the air carrying 5 gr. of moisture, is given in the curves of Fig. 4 for various temperatures of the exit gases.

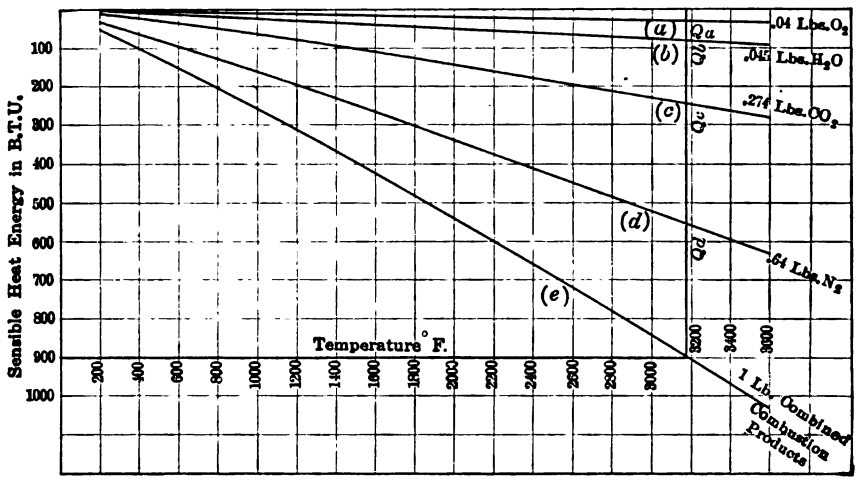

FIG. 4.—SENSIBLE HEAT ENERGY ABOVE 0° F. IN THE DIFFERENT PRODUCTS OF COMBUSTION FROM BURNING 1 LB. OF TYPICAL TOP GAS.

Although not plotted in that way, these curves could be made to show the sensible heat energy carried from the stoves and boilers by the moisture present in the original top gas and by the excess air. Such curves would give at various temperatures the loss of heat energy due to moisture and to excess air.

6. The curves of Fig. 5 show the effect of excess air on flame temperatures. A range of excess air from 20 to 60 per cent. lowers the flame temperature more than 300° F. As the amount of excess air is increased, the flame temperature depends less and less upon the temperature of the blast-furnace top gas. The great importance of reducing the amount of excess air, both as regards the conservation of sensible heat energy and the maintenance of a high flame temperature, is shown very positively by these curves.

7. To sum up, the statement frequently made, that the cold-wet method of cleaning gases is advisable because moisture is removed from the top gases, thereby permitting higher flame temperatures and obtain-

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ing a decrease in the loss of sensible heat energy, is not true under the conditions of operation assumed in the paper. It is shown herein that these conditions are fairly typical and representative. Unless the gas has a moisture content exceeding 100 gr. per cubic foot, or its temperature is extremely low, sensible heat energy is conserved by using a hot-dry method of cleaning. The conservation of this sensible heat energy by a hot-dry method of cleaning also permits of a higher flame temperature, if the moisture content is not too high.[3] The hot-dry method also makes it possible to operate with a minimum amount of excess air for combustion, this in turn promoting higher flame temperatures and conservation of gas.

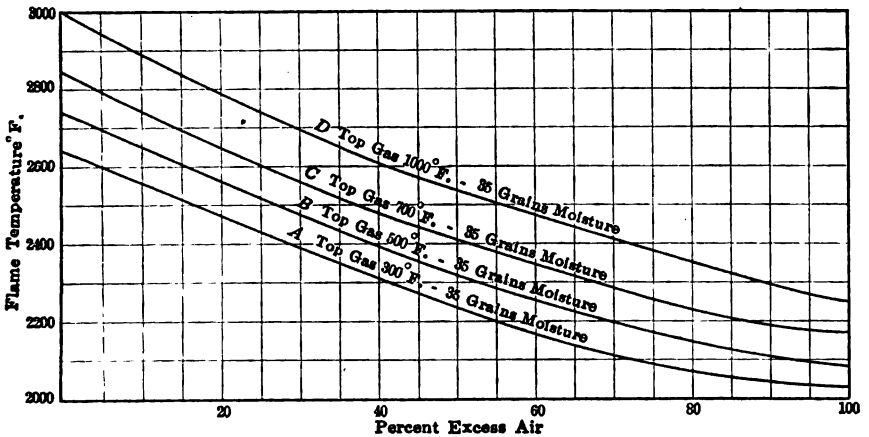

FIG. 5.—EFFECT OF EXCESS AIR ON FLAME TEMPERATURES.

In some ores a considerable amount of compounds of potash, zinc, lead, arsenic, antimony, etc., may accompany the compounds of iron, copper, etc., for which the ore is being smelted. Under present conditions this more volatile part of the ore may be carried away in the top gas. A dry method of cleaning may allow the recovery of certain of these volatile compounds, thus making commercially possible the treatment of a greater variety of ores. At the present time the application of the electrical method of cleaning blast-furnace gas from iron ore containing zinc is being developed and other problems similar to this are also under consideration.

[3] Dissociation of water vapor into hydrogen and oxygen is considered to be negligible under the above conditions. This would practically be the case if there was dissociation because under proper conditions the hydrogen and oxygen would again recombine. See Mallard and LeChatelier, *Annales des Mines*, 1883, vol. 4, p. 379; Hofman, *General Metallurgy*, p. 106. New York, McGraw-Hill Book Co., 1913.

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Hot-Dry Cleaning by Electrical Precipitation

Under practical operating conditions, many other conditions relating to the cleaning process must be considered. A few of these may be briefly considered. The cold-wet process of cleaning is usually more or less automatic in operation and requires comparatively little attention. Rather large quantities of water are used and considerable power is consumed in handling this water, and in forcing the gases through the system. The washers are comparatively large and the initial expense of installation is large. In some instances, a problem arises as to the disposal of the muddy water, it often being illegal to allow this contaminated water to run into the streams, while in other instances water is not abundant and its use for gas cleaning may be prohibitive.

There is a hot-dry method of cleaning that promises to be very advantageous for the purpose of cleaning these gases. The electrical precipitation processes make use of a high-tension electrical discharge which sweeps out the suspended matter from the surrounding gas. The electrical method does not cool the gas, it precipitates the suspended dust in a dry state, thus making it easy to reclaim this material. The operation is practically automatic and the energy consumption is small.

The cleaning power of the electrical method is practically complete, in many instances being as high as 99 to 100 per cent.

The degree of cleaning is greater than that usually obtained by wet methods in general and is ample for stoves and boilers. Indeed the results of recent tests have indicated the probability that even with a single pass precipitator the gases would be cleaned to the degree required by internal-combustion engines.

Summary

1. In general the sensible heat energy of the top gas from iron blast furnaces above 60° F., is much greater than the sensible heat energy of the moisture carried away from the stoves. A hot-dry method of cleaning the gases that conserves the sensible heat energy of the top gases is therefore more efficient as an energy saver than a wet method of cleaning that reduces the moisture content to 5 gr. (per cubic foot calculated at 32° F.), thus decreasing the sensible heat energy carried out by the moisture leaving the stoves.

2. By conserving the sensible heat energy of the top gas a dry-hot method of cleaning the gas (having a moisture content of not more than about 60 gr. of moisture) generally permits of a higher flame temperature than would be obtained by a wet process of cleaning that reduces the moisture content to 5 gr., excess air being considered constant.

3. Excess air lowers the flame temperatures. Curves are given that show this effect for various amounts of excess air.

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4. A dry-hot method of cleaning permits of the immediate recovery of the precipitated fume, dust or smoke in a readily usable condition.

5. Curves are given showing the flame temperatures with the typical top gases entering the furnace at 300°, 450° and 600° F., the gases containing variable quantities of moisture ranging from 10 to 200 gr. per cubic foot. Taking the top gas with a moisture content of 100 gr. and entering the stove at 600° F., the curve shows a lowering of the flame temperature of 220° F., from about 2,480° F. The wet method of cleaning would have cooled the entering gases from 600° F., to 60° F., thereby resulting in even a lower flame temperature.

6. Assuming a furnace to give 6,000,000 lb. of gas per 24 hr., the saving by the dry-hot method of cleaning of 140 B.t.u. per pound of gas (a representative case in practice) compared with the wet method would result in a saving of $69.67 per day, assuming coal to cost $2.50 net per ton and to contain 15,000 B.t.u. per pound. This equals about $25,000 per year.

7. The electrical method is unique in that it is the only known method that operates very efficiently for removing small suspended particles. For this reason the electrical method should be especially well adapted for Mesaba and similar ores, where the dust in the top gas is very fine.

8. The electrical method of cleaning the top gas should prevent the troubles of slow ignition and imperfect combustion often encountered when wet cleaning is used. These ignition troubles are due primarily to the loss of sensible heat energy.

9. The use of clean gas permits of a much more efficient type of stove construction since it eliminates cleaning. For a similar reason, boilers will operate more satisfactorily.

10. The dry method of cleaning furnace gases permits of the collection of such volatile constituents of the ore as potash, zinc, lead, tin, mercury, arsenic and antimony compounds. Ores that are lean in iron or copper, for instance, might contain enough zinc, for example, to make their smelting profitable, as the zinc might be recovered in a highly concentrated condition.

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Some Suggestions Regarding Construction of Hot-Blast Stoves

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(New York Meeting, February, 1917)

A HOT-DRY method of cleaning the gas from blast furnaces has been shown[1] to conserve the sensible heat energy of the gas, and in general it thus permits of a higher flame temperature. The electrical method of cleaning gases removes from 98 to 99 per cent. of the suspended matter in one operation when operating properly, thus making possible the use of a very clean gas for burning in the stoves. The use of such clean gas thus makes feasible a different type of hot-blast stove construction from that being used at present. It therefore seems advisable to construct the stoves from the viewpoint of getting the maximum amount of heat energy into the products of combustion.

By preheating the air added to the top gas and obtaining a better mixture of the air and top gas, less excess air will be required and a higher flame temperature obtained from the combustion of the top gas.

The interior of the stove should be provided with a kind of brick that will withstand a high flame temperature, will conduct the heat energy along certain directions, insulate the flow of heat along other directions and at the same time store heat energy so as to make the flow of such energy more uniform.

It is quite likely that these results can be best obtained by using several kinds of bricks: one type which is a good insulator of heat energy to be used for lining the stove and for making partitions through the stove to restrain the flow of heat; another type of brick or material with high refractory qualities; and a third type which will be a good conductor of heat energy.

The surface of the stove exposed to the atmosphere should be a minimum in comparison to the mass of checkerwork which it is possible to obtain, due regard being paid to other conditions.

With clean and hot furnace top gas it will be advisable to consider a rearrangement of the checker work in the stoves. One reason for the present size and shape of checker openings is to provide facility for

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[1] See preceding paper.

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periodical removal of coal dust. With clean gas the present wide spacing would not be required.

The mass of checker bricks should be as great as possible and still expose sufficient surface to the passing hot gases to absorb the greatest amount of heat, leaving the exit gases as cool as possible.

The checker openings should be small so as to bring the brick surfaces into intimate contact with the hot gases. The old rule that checkerwork openings must be 9 in. (due primarily to the necessity of cleaning the stoves of dust) can now be changed, and in fact has been changed in several plants.

The hot gases should be uniformly distributed across the cross-section of the passage and so maintained during their travel through the stove.

The hot gases should pass through the checker openings at a fairly high speed, thus increasing the heat transfer by the scouring action against the faces of the bricks.

The bricks should be so shaped and spaced as to insure the results mentioned above. It might even pay to have the bricks so close together as to require the installation of a fan above the stove to compensate for and overcome the additional friction of the gases through the smaller openings. This greater resistance to gas flow in the stoves could be taken advantage of in insuring the best distribution, high gas speed adjacent to brick surfaces and maximum heat absorption.

The bricks should contain the greatest mass or weight of material per unit of volume, combined with the highest mean specific heat and heat conductivity; *i.e.*, should be capable of holding the maximum heat energy consistent with speed of absorbing, storing and delivering such heat energy, and still possess the other qualities required of bricks for this purpose. Some experiments should be conducted and calculations made to determine the proper size, shape and composition of checker bricks, in the light of the points brought out in this paper.

Through improvements in design and the use of a hot-dry method of cleaning the top gas, it may be possible materially to lower the installation and the operating costs of the stoves, and obtain results equal to those obtained in present practice.

Mr. Mathesius'[2] paper has shown several advantages resulting from increasing the temperature of the blast. It would seem that effort along the lines indicated above should be made to increase the temperature of the blast by obtaining hotter stoves through the use of higher flame temperatures. Success by this method should show a further marked reduction in coke consumption in the furnace, an increased output of iron, smoother running conditions, and an increase in the calorific value of the gas from the furnace.

[2] High Blast Heats in Mesaba Practice, *Trans.*, vol. 51, p. 794, (1915).

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As for preheating the combustion air for the stoves, this is somewhat analogous to preheating the blast for the furnace, but it is unnecessary to employ so expensive a method. The combustion air could be preheated by the stove exit gases by means of suitably designed heat-exchanging apparatus placed above the stove, the preheated air being delivered to the stove burners by means of fan and duct. Calculations readily show the importance of adopting this suggestion and should be proven in practice.

It should be noted that a higher efficiency of absorption can be obtained in the stove when the flame temperature is highest; the exit gases will be only slightly, if any, higher in the one case than in the other. Furthermore, perhaps the greatest advantage to be obtained by preheating the combustion air and by hot-dry cleaning will be the ability to get a hotter blast in the same length of time, thus improving the ratio of "time on wind" to "time on gas."

A burner for intimately mixing the gas and air before burning (a modified Bunsen type) will aid greatly in giving a high flame temperature and the gas will be burned with a much less excess of air. Forced combustion, consisting of properly blowing the air into a gas burner with sufficient pressure, insures quicker combustion and a larger stove capacity. Radiation losses might thus be reduced, since a smaller number of stoves would be required, provided the efficiency of heat absorption by the checkerwork is not impaired. The question of long flames versus flameless combustion should be answered by thorough practical tests.

Application of Heat Data to Boilers

For use in boilers the importance of thorough cleaning may in some cases be more important even than it is in the case of the stoves. In using raw gas it is customary to blow the dust off the tubes every day by means of compressed air or steam. During blowing, a large amount of excess air enters the combustion chamber. Immediately after cleaning, the dust begins to deposit again and the efficiency of the boiler immediately decreases. The cleaner the gas, the more valuable it is for boiler purposes. The hot-dry method of cleaning the top gas for burning in boilers is therefore more efficient than the wet method for the following reasons: (1) the top gas is made cleaner than when the wet method of cleaning is used; (2) the sensible heat energy of the top gas is conserved; (3) the temperature of the gases in the boiler is increased, making possible the use of a gas of lower calorific value; (4) no slags or corrosion results from the dust settling on the furnace bricks or the boiler tubes.

The curves described in our other paper in this volume can be applied directly to calculations on the saving of heat energy in the boilers. In boiler operation there is the added advantage that the gas carried from

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the boilers is usually at a comparatively low temperature. For this reason the sensible heat energy of the unremoved moisture, carried away in the exit gases from the boiler, is even smaller than that from stoves.

The data on sensible heat energy given in the charts can be applied to a large number of problems: The hot gases coming from Portland cement kilns, smelter furnaces, cupolas, furnaces used to heat retorts for making illuminating, water and producer gas, coke ovens, etc. The electrical method of cleaning is now used in some of the above cases for removing the dust and fumes at high temperatures. At the Riverside cement plant and in sulphuric acid plants the electrical method is now being used to clean gases at about 900° F. Several other gases have been cleaned at this temperature. A cheap and efficient method of cleaning gases at a high temperature will thus introduce a new epoch in the saving of the sensible heat energy that is now being wasted in so many instances.

Discussion of the Two Preceding Papers

F. H. Willcox, Pittsburgh, Pa. (communication to the Secretary*). —We must keep in mind, in balancing the savings—to be anticipated by the most efficient combustion of gas, the best heat absorption by stoves or boilers, and by heat conservation of the thermal equivalent carried in blast-furnace gas—against the cost in operation, maintenance, depreciation and interest charges of the equipment required to realize the savings, that in boilers and hot-blast stoves we are basing our economies upon two different combustion phenomena.

In a boiler, evaporating water at a heat efficiency of 70 per cent., approximately 47,550 B.t.u. is consumed per boiler horsepower. Taking the blast-furnace gas cited in the above paper, we are using about 38¾ lb. of cold-washed gas per boiler horsepower. Using the hot-cleaned gas, the same equivalent heat is furnished by 34 lb. of the gas. This leaves of the 38¾ lb. required in the case of cold-washed gas, 4¾ lb. or 6,650 B.t.u. available for raising more steam when dry-hot gas is used, which means that an equivalent amount of coal firing can be dispensed with. Per ton of iron, this figure $\frac{2}{3} \times \frac{12{,}000}{38.75} \times 4.75 = 980$ lb. gas or 1,372,000 B.t.u. or 100 lb. coal. At $1.50 per ton of coal, the net saving per ton of pig iron produced by the use of dry-hot over wet-cold cleaning will, therefore, be about $0.06⅔.

At a stove heating 8,500 lb. of air per ton iron from 150° F., one-quarter of the dry-hot cleaned gas, or 3,000 lb., would heat the air to 1,450° F.

$$3{,}000 \times 1{,}400 \times 0.70 \div (8{,}500 \times 0.255) = 1{,}300.$$

* Received Feb. 20, 1917.

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The cold-wet gas would heat the same amount of air to 1,280° F.

$$3{,}000 \times 1{,}225 \times 0.70 \div (8{,}500 \times 0.255) = 1{,}130.$$

Assuming that in each case 150° is kept as reserve for times at which the stove is not used to best advantage, we have a temperature 1,300° for hot-dry against 1,130° for wet-cold cleaning. The difference, 170°, represents 8,500 × 0.255 × 170 = 368,500 B.t.u. delivered to the furnace per ton iron in excess of what would have been delivered using cold-cleaned gas. The mechanism of the saving this effects is interesting, for whereas in the case of the boiler every 13,500 B.t.u. saved and delivered in the gas to the boiler can be computed as equivalent to 13,500 B.t.u. in a pound of coal, 13,500 B.t.u. conserved and delivered in the gas to the stove delivers 70 per cent. of this to the hearth, or 9,450 B.t.u. Instead of this saving $\frac{9{,}450}{13{,}500}$ lb. of coke, as at the boilers, we save almost four times as much. Assuming 90 per cent. fixed carbon and 82 per cent. of the carbon burned at the tuyères to carbon monoxide, then the amount of heat generated at the tuyères per pound of coke is 4,500 × 0.90 × 0.82 = 3,320 B.t.u. So the 368,500 B.t.u. saved is equivalent to about 110 lb. of coke per ton iron rather than about 27 lb. coal, had the heat been used at the boiler. At $4 per ton coke, this saving is $0.22 per ton of iron.

I believe that the authors may have erred in assuming the dry, clean top gas to be at a temperature of 700° for the basis of their calculations, and think that 350° F. would be a fairer figure. The gas at boilers and stoves would correspond more closely to this lower temperature at the great majority of furnace plants in the country. This basis would roughly cut the above approximate savings in half, or we might expect a saving at the boilers of, say, 3 c. and at the stoves of 11 c. per ton of iron, by the use of dry-hot instead of cold-wet gas.

Under these circumstances, I question whether either cold-wet or dry-hot cleaning at boilers has any marked advantage over the use of hot uncleaned gas. At the stoves, we have an undisputed big saving which in reality and in practice may be greater than the estimate made above on account of the time and heat saved by not taking stoves off for cleaning, the elimination of the progressive decrease in heat efficiency due to bridging of the checker tops and dirt accumulations on the walls of the heating surface, and on account of the saving in labor to clean the well bottom. Further, in a stove there is an exceptionally long combustion chamber in which combustion of the gas can be effected even when the gas is at a low temperature, whereas in a boiler combustion must be completed in a relatively short length of travel, before the gas and air mixture reaches the tubes, if high efficiency is to be expected. Finally, in a stove there is no good reason to anticipate any marked excess of air

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because the usual stove tender is able to regulate his air inlet to give a good stack analysis, and with a modern type of stove, the stack temperature will not exceed 400° F. and may be as low as 200°. My observations lead me to believe that all good Northern and some Southern practice, and the trend of most other practice, places the temperature and moisture content of gas on the side, where, from the point of flame temperature, stack temperature at the stove and flame control, the cold-wet method of cleaning will give the best results. There are practices where magnetites comprise the burden that will undoubtedly indicate hot-dry cleaning of gas for the stoves, but they are likely to be a small proportion of the total operations.

Cold-wet cleaning can be carried to an extreme. During the month, I have been at a plant where the wet washer delivered gas so cold and wet that the gas would not burn in the stoves. I have been at another plant where the auxiliaries of the wet washer froze up. Elaboration of the washer can be carried to an extreme, increasing interest, maintenance and operating changes to a high figure and multiplying the operating difficulties both at the washer and stoves. I believe there has been tendency toward overestimation of the difficulties of washing gas for stoves, that is, in the ability to obtain clean and dry gas from a wet washer. Efficient wet cleaning can be accomplished at moderate expense and without intricate layout and I believe wet cleaning for stoves would be the choice of the majority of furnace men, their judgment being confirmed by paragraph three of Conclusions Derived from Sensible Heat Data. Another item to be considered in the use of dry-hot gas is the deterioration of the brickwork at the base of the combustion chamber by an exceptionally high flame temperature. With the use of a good mixing burner for the cold-wet method, wet cleaning gives excellent stove practice and no deterioration of brickwork. It is to be borne in mind that should furnace practice give a top heat of 600°, the application of dry-hot cleaning will give better stove heat, lower coke and eventually lower top temperature, so that eventually conditions may be reached when wet cleaning is indicated according to the tabulations of the paper.

At the boilers, we have a slight margin of savings due to the increased heat brought to the boiler in the gas, with the additional advantage that the gas is clean. There is no question in my mind but that dry-hot is more advantageous than wet-cold cleaning for boilers. In tests I helped run some years ago, we found the utmost difficulty in burning cold-wet washed gas in the boilers without an excessive amount of air. Hot gas burns more quickly and has a shorter flame; flame control is easier and we found that the increase in boiler efficiency due to clean gas was but little better than that of uncleaned gas, not enough increase in efficiency, in fact, to offset the loss in sensible heat and cost of washing. I am sure that with hot or cold gas, some type of aspirating burner is

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imperative, to obtain highest efficiency, so this cost goes for any system of gas burning in boilers for uncleaned, dry-hot or wet-cold gas. The question resolves down to relative pounds of steam evaporated by equivalent amounts of uncleaned and dry-hot gas.

We know that there are blast-furnace boiler plants operating with uncleaned hot gas at 68 to 70 per cent. efficiency and up to 130 per cent. overload. It is perfectly feasible to keep boiler tubes free from flue-dust deposition and to keep the flue temperature to about 600° F. with very little air dilution. Can the dry-hot cleaning better this work at an installation and operating cost that will do better than break even with the savings due to the better work obtained?

At plants where the dust evil is actually an insurmountable handicap on account of depositions on the tubes when it cuts the iron work of the burners and flues or destroys the brink, the dry-hot cleaning should be of more marked benefit and obviously superior to wet-cold cleaning. If the cost of operation and installation of the dry-hot method is less than that of mechanical or hand soot and dust blowers, and of the deterioration of boiler and settings due to flue dust, then it is bound to be the ultimate means of handling gas preliminary to use under boilers.

The suggestions in regard to stoves are timely and are apparently in line with the developments in hot-blast stove-design practice.

THE CHAIRMAN (JOSEPH W. RICHARDS, South Bethlehem, Pa.).—The flame temperatures, as calculated by the method of Kuzell and Wigton, are not determined upon the right principle. They are probably accurate enough for relative purposes, but would not be correct absolutely. The principle on which they were calculated was inherently a faulty principle.

The amount of heat which is saved as sensible heat in the hot gases, is probably larger than is usually estimated, and a rough calculation which I have made shows it to be equivalent approximately to the useful effect of 100 to 200 lb. of coke per ton of iron. The loss depends upon the temperature of the issuing gases. Taking the temperature at 600° F., the loss represents a considerable portion of all the heat generated in the furnace. The ultimate reason of that is that the gases are 4 or 5 tons in weight per ton of iron produced, and therefore what appears like a small amount of sensible heat in the gases amounts to a relatively large proportion of the heat generated in the furnace. Another point, the fuel seldom generates in the furnace more than 50 per cent. of its calorific power, so that if you estimate it in percentage of the heat generated in the furnace instead of in percentage of the calorific power of the coke used, then it figures up nearly double what it otherwise would. It thus makes a very respectable percentage of the total heat generated in the furnace, and if a considerable fraction can be returned into the furnace, in the place where it needs it most, around the tuyères, in the form of hot blast, the saving is even intensified. I would ask Mr. Bradley whether

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Fig. 1 should not have some explanation on it to show the difference between the curves *A* and *A'* and *B* and *B'*.

LINN BRADLEY.—That will have to be corrected. When the paper was prepared, these curves were very fully prepared and a great deal of information given, but that has been censored and left out and it needs explanation; it is not clear just how those curves were prepared.

CHAIRMAN RICHARDS.—I agree with Mr. Willcox that 60 per cent. excess of air in the stacks from the stoves is too liberal an allowance.

LINN BRADLEY.—That is too liberal on the principle of practice, but there are some furnaces where the laborers really need to be shown in a graphical way how important it is to cut down.

CHAIRMAN RICHARDS.—It is poor practice; it should not be so.

C. P. PERIN, New York, N.Y.—At one of the furnace plants with which I am familiar, we recently installed eight deep cast-iron gas washers, located in front of the gas valves of each stove.

At this furnace the ores decrepitate more or less, as does also the dolomite, and this dust has given more or less trouble in the stoves. These washers cost in the neighborhood of $400 each, exclusive of the piping connections, and use the waste water from the stoves. They cause a drop of about 100° in the temperature of the air coming from the stove. They have not been in operation long enough to prove how valuable they are. The reduction in temperature obtainable, however, shows itself very plainly.

My present state of mind is one of extreme doubt as to the value of the elimination of that amount of dust in the stoves, if it results in cold, wet gas and consequent lower heats. A prolonged period of use may show that the diminution of loss of time on the score of dust may offset the disadvantages due to lower heats.

F. G. BREYER, Palmerton, Pa.—I heartily agree with all Mr. Bradley has said with regard to the value of hot-dry gas versus cold-wet gas. There is one point, however, on which I do not agree with the speaker, namely, that "the electrical process is unique and that it is the only known method that operates very efficiently for removing small suspended particles." I am connected with the New Jersey Zinc Co. and we have two blast furnaces making spiegeleisen from the residue from the zinc-oxide furnaces. In the residue there is always considerable zinc left behind, so that we get a very dirty gas which is partially settled out in an elaborate system of dry dust catchers. Formerly we burned this gas after passing through the dry dust-catcher system, directly under return-tubular boilers. We recently installed modern water-tube boilers and it became desirable to clean the gas more thoroughly from this standpoint

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as well as for the zinc recovered in the dust. The question of a proper method of cleaning came up and we decided on the wet method and finally adopted Theisen washers. At the present time I believe we are, perhaps, the only plant in the country that is scrubbing with Theisen washers, not only the gas that goes to the boilers but also that which goes to the hot-blast stoves.

I mention this because I believe zinc oxide certainly comes under Mr. Bradley's heading of "small suspended particles." The fineness of zinc fume is of a different order of magnitude from that of fine ore particles or coke dust, yet our Theisen washers are now removing that without any preliminary scrubbing whatever. We run the gas through a large dry cooling tower, then into the Theisen washers at a temperature between 150° and 200° C., where it is scrubbed down to below 0.05 grain of dust per cubic foot; the average is 0.02 grain. We of course have no trouble at all with boilers and none with our stoves on such gas.

We have had the difficulty mentioned by one of the gentlemen present, that a gas after a wet washing gives more trouble at first than is met when burning hot gas. When we changed over from using the dry-hot gas to the wet-scrubbed gas, our stove men reported that they could not get sufficient temperature in the stoves. We found that was largely due to the fact that the stove men were judging their combustion entirely by the appearance of the flame in the lower part of the stove, and that the cold-wet gas from the scrubbers at this point looked as if it were giving a cold flame, *i.e.*, as if there were not enough gas going in. The furnace men kept crowding more gas in, and less air, in an effort to get the lower part of the combustion chamber hot; whereas, as a matter of fact, they already had a large excess of gas. As soon as we began putting the gas machine behind the stoves, admitting more air and regulating the CO_2 content of the gas, we soon got back to our proper temperatures and have had no trouble since. The stove men now have learned that it is not necessary to have the combustion chamber hot right where the gas goes in. They used to have it very hot there, through the higher initial flame temperatures and greater velocity of combustion of the hotter gas. Now the combustion is slower, the flame longer and the hottest zone well up in the combustion chamber.

To come back again to paragraph 7 of Mr. Bradley's report: Perhaps we are in a better position than is the average furnace man to make some comparison of this dry-hot versus cold-wet cleaning, on account of the fact that we have abundant experience with dry filters. We have a pretty good idea of the cost and efficiency of filtering gas through bags as compared with the cost of taking out the suspended particles with Theisen washers, and I take exception to Mr. Bradley's statement that the electrical method is the only efficient method, because, with bag houses, we get practically 100 per cent. efficiency. The only loss is a

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very small one on shaking, certainly less than 1 per cent., and I venture to say less than 0.5 per cent. Similarly with the Theisen washers working on a very fine fume such as ours, the efficiency is as good as any published results for electrical precipitation. From our experience with the electrical method, we have never been able to get as good recovery of fume or dust of any sort as we do with the bag house.

There have been a good many unsatisfactory experiments made, in Germany, especially, cleaning blast-furnace gas by the dry method, but I do not believe that the automatic bag house had been improved up to its present state when these experiments were made. Leaving aside the question of explosion, which I think can be taken care of just as readily as we can take care of it on a wet-washing system, I believe that the method of cleaning blast-furnace gas dry and hot with the automatic bag house is going to be the final solution. We may not be able to keep our gas up as high as 450° or 500° F., as by the electrical method, but we certainly can get them up to 250° or possibly 300° F. We operate houses now where the gas is filtered regularly at 400 and possibly goes up as high as 500, but that would not be a regular operation because the bag consumption would be excessive.

L. E. Riddle.—I would like to ask Mr. Bradley if there has been any successful electrical precipitation in connection with the iron blast furnace? I understood that they have been experimenting with direct blast-furnace gas, and that moisture, precipitating with the dust, caused the precipitate to clog up very rapidly. I am much interested to know if that has been overcome, because we have furnaces in which the gas temperature is 1,000° and I would like to save that if I could.

Linn Bradley.—In a test of the hot process on the cleaning of blast-furnace gas, about 1,000 to 2,000 cu. ft. of gas were cleaned continuously over a period of several hours, under furnace operating conditions. The temperatures varied from 100° to 500° F., depending on the care taken for conserving the sensible heat. If the temperature gets low, the moisture will precipitate and if there is a large amount of moisture compared with the amount of dust, you get a sticky precipitate. To overcome clogging the temperature must be kept up so that the moisture will not be condensed. It is merely a question of temperature and dust, even in the presence of sulphuric acid.

Chairman Richards.—The statement of Mr. Breyer that in the modern baghouse gases can be filtered up to 400° F. is extremely interesting and points to the possibility of fireproofing or heatproofing the bag materials, or using some sort of material which is a little more resistant to heat than wool, so as to filter gases at the ordinary temperatures of blast-furnace gas.

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F. G. BREYER.—We have used asbestos bags successfully; the question is their relative economy as compared to the ordinary muslin bags. I think we have some asbestos bags that have lasted about 8 months to a year, may be longer than that. Still, their initial cost is very high and we do not as yet feel satisfied that we would be justified in putting them in. If the gases that we are filtering with those asbestos bags were subsequently used to heat a boiler or for some such purpose where the heat was of value to us and it was desirable to run these bag houses at a high temperature, the heat saved by running at the higher temperatures would undoubtedly justify the use of asbestos bags and I do not doubt that asbestos bags will eventually come into use in an ordinary bag house.

C. P. PERIN.—Frazier and Chalmers, in England, are building gas washers with a coarse washing bag. It is expected that a cotton bag, treated with some fireproof preparation, will bring about an extension of the use of this type of washer. So far, they have installed a number of this type of dry gas-cleaning machinery. It is thought in England that this is the coming method for gas cleaning.

I should like to ask whether anybody here has ever tried with a blast furnace, the experiment which is now being carried on in the case of heating furnaces. In Chicago there are two continuous-heating furnaces equipped with something resembling the old-fashioned cast-iron pipe stove. The air intake passes through these "U" tubes and is admitted with the gas at the point of combustion. It has been suggested that a similar arrangement might be installed in the chimney flue back of hot-blast stoves.

CHAIRMAN RICHARDS.—A basic principle which is often sinned against in the operation of hot-blast stoves is to consider that the waste gases must be hot enough to operate the chimney. If a chimney works by the heat of the waste gases, enough heat must be supplied to operate the chimney, but that is an extremely wasteful way of drawing air through the stoves. The chimney, as a heat engine, has an efficiency of not more than 0.5 per cent. of the heat which it consumes. Therefore, if by any means you can save the heat in the gases and substitute mechanical fan draft or suction for the operation of the stoves, you may save many times as much energy as it takes to run the fan or blower. If the fan is run by a steam engine from steam raised by the boilers, the mechanical power required to run the fan, or the heat equivalent required to run it, is only a small fraction of the heat which might be saved from the gases. Therefore, a stove should be designed to take as much heat as possible from the gases. If you could make the gases go to the chimney at ordinary temperature and get the heat that is in them back into the hot blast by some means of methodic operation, you would stand to save nearly all

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the heat which otherwise goes up the chimney, and, besides that, you would not need a chimney.

C. P. PERIN.—We had a most interesting problem in India, in operating open-hearth furnaces. The draft was seriously affected by the climatic conditions. The period of low temperature is at 8:00 a. m., and the period of high temperature between 2:00 and 2:30; the temperature in the shade frequently rises to 120°. The air, being diathermous to the direct rays of the sun, seems to be very quickly heated and pushed up by the reflected rays, so that by 10:00 o'clock the draft stack had very much less effect than at 8:00 o'clock, and stacks that were ample in this country required an addition of, in certain instances, 50 ft. to make them effective. In several instances, blowers were added to increase the efficiency.

In reply to Prof. Richards, I thought the question of the mechanical efficiency of a blower inducing draft had been worked out adversely, and that many engineers were of the opinion that a fan was not the cheapest way to induce draft.

CHAIRMAN RICHARDS.—My impression was the other way. We build a chimney and use an immense amount of heat to operate it; whereas, if we have mechanical power at hand, we can devise means of saving the heat which is in the gas and usefully putting it back into the furnace. There is a margin of saving which is well worth looking at.

LINN BRADLEY.—Is it a question of saving heat? Is it not a question of getting heat where you want it and at the temperature you want it, rather than that you can utilize high blast? It seems to me that is the way to work it so that you get a high blast, high temperature, as well as conserve heat.

F. G. BREYER.—The question of utilizing further the waste heat of the products of combustion and the advisability of putting in a fan, instead of using a stack and gases hot enough to give the draft, has been rather forcibly brought to our attention since we put up the Theisen washers to clean our gas for the stoves. We have installed a new type of stove in which the checkerwork and heat-absorbing surface are considerably greater than formerly. We made this change because we could afford to do it with our clean gas. We could cut down our flues to whatever we thought a reasonable sized opening and operate these stoves at the same temperature, that is at 1,200° F., as before. We find that they are so much more efficient than our old stoves that our exit flue temperatures are down as low as 200° C. We only have one stove out of nine built that way, after we get all nine of them changed and if the temperature continues around 200° C., we may be forced to use a fan to pull the gases out of our stoves. Those stoves, I might say, not only have smaller

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checkers, but they are also insulated with insulating brick around the outside, conforming on the whole somewhat closely to Mr. Bradley's ideal stove construction.

CHAIRMAN RICHARDS.—Perhaps I did not make clear my conception of what should be done. Suppose you have a stove with two passes that allows the gas to go to the chimney at 300° C., and you can put in an extra set of checkerwork that will reduce the temperature to 150° C., which is not enough temperature to pull the gases through the stove. Assuming that the 150° C. which you are saving goes back into the blast and increases the temperature of the blast by about the same amount, it seems to me many times the saving may be effected by using a fan to help bring the gases out of the stoves, than the power necessary to drive the fan. If the temperature of the gases going into the stack could be reduced to the outside temperature, 60° F., a fan or suction blower would have to be used to pull the gases out of the stoves; but if one-half or three-fourths of that heat is sent back into the blast, a large saving will certainly be made.

R. J. WYSOR, So. Bethlehem, Pa. (communication to the Secretary*) —In addition to the printed discussion† (of Huessener's paper) which was prepared a year ago, the following remarks are offered in discussion of the papers by Messrs. Bradley, Egbert and Strong.

The authors have made an interesting theoretical comparison of the relative merits of the dry and wet methods as applied to the cleaning of blast-furnace gas. Their suggestions, in general, are in line with actual practice and endeavor in the construction of modern hot-blast stoves.[1] Preheating the combustion air, as suggested by the authors, is in advance of present practice; for three-pass stoves this would be a more difficult proposition than for two- or four-pass units.

In a separate paper[2] I have stated that in this plant almost perfect cleaning of raw gas leaving the dust catcher was accomplished in a small experimental Cottrell treater. It is reasonable to suppose that time and experience will be required to develop the electrical-precipitation method on a full-size scale for blast-furnace conditions. However, there seem to be no insurmountable difficulties in the way of such development. Certainly much time and money have been expended in developing wet-washing processes, and there is still room for improvement.

In the event of the ultimate installation of large electric precipitators in connection with new blast furnaces, in plants where gas fuel is at a

* Received Feb. 18, 1917.

† See p. 337.

[1] A. J. BOYNTON: Progress in Hot-Blast Stove Design. *Yearbook of the American Iron and Steel Institute*, 1914, p. 76.

[2] Potash as a Byproduct from the Blast Furnace. See p. 257.

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premium, the logical plan would be to insulate all mains, etc., leading from the furnace to boilers and stoves and from the latter back to the furnace; this would include downcomers, dust catcher, precipitators, connecting mains, boilers, stoves and hot-blast system.

From a practical operating standpoint, however, the difference in B.t.u. saving or maintenance of a slightly higher flame temperature as effected by one system of gas cleaning over the other is of less importance than the relative cleanliness of gas produced. Especially is this true in hot-blast stove practice. The glazing or destruction of brickwork, or the blocking of checker openings, due to unwashed or imperfectly washed gas, is responsible for a far greater loss in stove efficiency than is a slight difference in thermal value of the fuel gas. The maintenance of brickwork in stoves for regenerative purposes is destined to receive much more attention in the future than in the past. Furthermore, in some blast-furnace plants, chiefly isolated merchant furnaces, there is an excess of fuel gas available, and hence the thermal value within ordinary ranges is of little moment.

Whether the wet or dry system of blast-furnace gas cleaning for the average plant finally predominates, of course, depends upon various factors, such as first cost, operating cost, space required for units, dependability and safety of operation and cleanliness of gas produced. Other conditions being about equal, the superior system will be the one which cleans gas to an appreciably lower average dust content than the other. If there is no appreciable difference, other conditions being about equal, the balance of favor will be with the dry process.

K. HUESSENER, Pittsburgh, Pa. (communication to the Secretary*).—I agree with the authors on the main point that as far as economy is concerned dry-hot cleaning is always preferable to cold-wet cleaning. One strong point in favor of this argument has apparently, however, been overlooked; namely, that the sensible heat is all velvet to the stoves and boilers. The sensible heat is always completely utilized, because it has not to be developed by combustion as in the case of the latent heat. In other words, hot-blast furnace gas of a latent heat of 100 B.t.u. per cubic foot and a sensible heat of 5 B.t.u. per cubic foot is more valuable than cold gas of a latent heat of 105 B.t.u., without any sensible heat. In order to develop the additional 5 B.t.u. latent heat, a corresponding amount of air has to be admitted and the volume and weight of the flue gas will be correspondingly increased, so that for the same stack temperatures the heat losses in the case of the cold gas are of necessity larger than in the case of the hot gas where the weight of the flue gas is not affected by the sensible heat. If both gases are burned with an efficiency of 70 per cent. calculated on the latent heat, 75 B.t.u. are

* Received Feb. 28, 1917.

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utilized in the case of the hot gas as against 73.5 B.t.u. in the case of the richer cold gas. If 75 B.t.u. per cubic foot are to be utilized under a 70 per cent. efficiency, while using cold gas, the latent heat of such cold gas would have to be 107 B.t.u. This difference increases inversely to the efficiency. At a 60 per cent. efficiency the 5 B.t.u. sensible heat are equivalent to 8 B.t.u. latent heat. If the temperature of the gas entering the washer is 500° F. and is cooled down in the washer to 100° F., approximately 8½ B.t.u. per cubic foot are lost in sensible heat. At an efficiency of 60 per cent. these 8½ B.t.u. sensible heat will correspond to over 14 B.t.u. latent heat. This is in my opinion the strongest argument against wet cleaning.

On the other hand, I think that a comparison of hot-clean gas at temperatures of 600° F. with wet-cold gas at 60° F. favors the dry-hot cleaning and is not quite fair to the cold-wet cleaned proposition. In the first instance top temperatures of 600° F. are now always the exception rather than the rule. The average blast-furnace operator always attempts to keep the top temperatures low and if he cannot do it otherwise he will use water on top of the charge. 350° F. for hot gas appears to me to be a fairer average. As far as the cold-wet cleaned gas is concerned, very few plants are in a position to get temperatures of 60° F. even if they try to and I believe I have conclusively shown in my paper on the combustion of blast-furnace gas that the most economical temperature for cold-wet washed gas is 100° F. The comparison should, therefore, be made between hot gas of 350° F. and cold gas of 100° F. The loss in sensible heat is in that case about 5½ B.t.u. per cubic foot of average gas.

I also differ from the authors in regard to what they say about the importance of higher flame temperatures in hot-blast stoves; at any rate in so far as it is claimed that a higher initial temperature will *de facto* result in a higher blast temperature. Experience has abundantly shown that the final blast temperature is only very little dependent on the temperatures developed in the combustion chamber. These blast temperatures depend nearly exclusively on the heat storage capacity of the stove. Assuming that the checkerwork is clean, the heat storage capacity in its turn depends on the number of square feet of heating surface available per cubic foot of blast blown per minute. It has been shown time and time again that where wet-washed stove gas is used the same temperatures are continuously available, which were obtained with hot unclean gas as long as the stoves were clean.

An excellent illustration of the argument of heat-storage capacity as against combustion temperatures is given by the Larimer four-pass stove which is now in operation at the Illinois Steel Co., Joliet, Ill. In this plant cold-wet washed gas is utilized of a calorific value of not more than 94 B.t.u. The combustion temperatures rarely exceed 2,200° F.

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and yet blast temperatures of 1,850° F. are obtained, which is more than any furnace could probably utilize under present circumstances. A heat-balance sheet of the Larimer stove shows that only 6 per cent. of the total heat is absorbed in the combustion chamber as against 64 per cent. in the second pass. The average temperature of the combustion chamber was 2,150° F. and the average temperature in the second pass 1,445° F. which in my opinion shows conclusively that an increase in the combustion chamber temperatures of 100° F. or 150° F. could hardly, if at all, have affected the blast temperature. The difference between the combustion-chamber temperatures with hot-gas temperatures at, say, 350° F. and the cold-dry gas at 100° F. would be only a little over 100° F.

I am also of the opinion that it might have been preferable to deal with stoves and boilers separately, as the two are really different propositions. In the case of the boilers, the combustion temperatures are the most important factor, as stack temperatures usually fall and rise inversely to the combustion temperatures. As far as boilers are concerned, the moisture in the products of combustion will seldom, if ever, reach such an amount that condensation in the exit gases could take place. Not so in the case of stoves. The Larimer stove, for which most detailed figures are available, shows that the temperature of the exit gas is usually in the neighborhood of the compression temperature of the blast. While this temperature when leaving the blowing engines will usually be in excess of 140° F., it was shown at the Joliet plant that in a number of cases it was between 110° F. and 130° F. when reaching the stoves. Temperature charts showed that the exit gas very frequently did not reach a temperature of 140° F. until after the stove had been on gas for ¾ hr. If dry-hot washed gas was used in such a case, it would surely result in sweating taking place in the last pass if the dew point of the top gas is in the neighborhood of 130° F. This is, of course, a condition that cannot possibly be countenanced in stove practice, as it would invariably ruin the fire-brick work in the shortest possible time.

As far as the cleaning of boiler gas is concerned, it must be kept in mind that boilers do not appreciably drop in efficiency on account of the dust in the boiler gas. S. K. Varnes, Experimental Engineer of the Pennsylvania Steel Co. calculated the drop in efficiency over 6 weeks from the date of starting on the clean boiler to the time of laying off the boiler for cleaning and found it an average of 1¼ per cent. owing to increase in the temperature of the exit gas. In the case of a 500-ton furnace making 3½ tons of steam per ton of coke and assuming the value of steam at 20 c. per ton, this drop of 1¼ per cent. in efficiency only represents $1,600 per year to which one would have to add the wages of two men, say $2,000, who have to blow the boilers. There is, of course, also a drop in efficiency while the boilers are being blown, but as this is not more than 1 hr. per day, the total loss is only 4 per cent. of the total operation time,

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so that even if the boiler efficiency dropped by 25 per cent. during blowing, it would mean a loss of only 1 per cent. on the total operation, which causes a yearly loss of $1,280. This will show that it would require a very cheap cleaning arrangement to make it pay to wash boiler gas.

LINN BRADLEY, H. D. EGBERT and W. W. STRONG (communication to the Secretary*).—In the discussion of the paper, a request was made for a further explanation of the curves given on Chart I.

In preparing the curves given on this chart, a typical blast-furnace top gas having the following composition by weight was assumed:

	Per Cent.
CO_2	21.00
CO	24.00
H_2	0.25
CH_4	0.25
N_2	54.50

For any given top-gas temperature, Curves *A*, *B*, *C*, *D*, and *E* indicate in British thermal units the sensible heat lost in cooling the top gas to 60° F. by wet-cleaning methods. Top-gas temperatures are plotted as abscissæ, sensible heats above 60° F. in British thermal units are plotted as ordinates, and separate curves are drawn for top gases carrying various amounts of moisture.

For any given stove exit-gas temperature, curves A^1, B^1, C^1, D^1, and E^1, indicate in British thermal units the sensible heat carried out of the stoves by the moisture which is left in the top gas when it is dry-cleaned and enters the stoves hot. This moisture is removed when the gases are wet-cleaned and the sensible heat which it carries out of the stoves is therefore saved. As before, separate curves are drawn for top gases carrying various amounts of moisture.

The curves in Fig. 1, therefore, indicate graphically how much more sensible heat is conserved by cleaning the top gases by the dry-hot method than is conserved by cleaning the top gases by a cold-wet method and thus removing the greater part of the moisture carried by them. They also show of how little moment it is from a heat conservation standpoint whether or not the moisture carried by the top gases is removed before they enter the stoves. That wet cleaning secures the removal of this moisture and is thus to be recommended from a heat conservation standpoint is an argument often advanced by advocates of wet cleaning. The curves of Fig. 1 show how fallacious such an argument is.

The following example will explain the use of the chart:

Assume the top gas leaving the furnace to have a temperature of 400° F. and to have a moisture content equivalent to 25 gr. of water per cubic foot, calculated at 32° F. Now assume this gas to be cooled to 60°

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F. before entering the stoves and to carry 5 gr. of moisture per cubic foot, calculated at 32° F. at that temperature. Then from Curve *B* on Fig. 1 it will be seen that the sensible heat lost is approximately 95 B.t.u.

Now assume the gas to enter the stoves at 400° F. and to carry the original amount of moisture. Also assume that the gas leaves the stove at a temperature of 400° F. in both of the above cases. Then from Curve *B*-1, Fig. 1, it will be seen that the sensible heat carried out by the moisture in the gases in excess of 5 gr. per cubic foot, calculated at 32° F. is approximately 5.0 B.t.u. Thus the difference (95 − 5 = 90 B.t.u.) represents the conservation of sensible heat per pound of the gas when the top gas is dry-cleaned as compared to when it is wet-cleaned. Even should the gases carry 100 gr. of water per cubic foot calculated at 32° F. having a temperature of 200° F. at the burner and the exit from the stoves be as before, 400° F., there would still be a conservation of sensible heat obtainable by dry cleaning. This latter case obviously represents an extreme condition.

As has been brought out in the discussion of the paper, there are two factors to be taken into account when considering heat economy in connection with blast-furnace gas cleaning. There is, first of all, the question of heat saving, and then that of getting the heat where it is needed so that an increase in the temperature of the blast can be obtained. In addition, Mr. Wysor brings out an important point in his discussion of the use of unwashed gas; *i.e.*, that the relative cleanness of the gas produced by different systems of cleaning is of great importance, inasmuch as the cleaner the gas the less will be the destruction of the brickwork and the blocking of checker openings with the attendant loss in stove efficiency. In brief, an efficient hot-dry cleaning method preserves the efficiency of the stoves, saves a large amount of sensible heat which is lost in wet-cleaning operations and enables the heat which is thus conserved to be utilized where it is most needed; *i.e.*, in raising the temperature of the blast.

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Blast-Furnace Gas.

Discussion of the paper of K. HUESSENER, (*Trans.*, vol. 53, pp. 402 to 433), and of the papers of LINN BRADLEY, H. D. EGBERT and W. W. STRONG, pp. 303, 319.

R. J. WYSOR, So. Bethlehem, Pa. (communication to the Secretary*). —At our plant (Bethlehem Steel Co.), we have had several Bradshaw burners under course of construction and erection for some time, but up to the present we have not had opportunity to put them in service. I regret that I can not take part in the discussion with some actual results from these burners. Mr. Huessener properly emphasizes the importance of obtaining the highest combustion temperature practicable in boiler practice, which demands a near approach to perfect combustion conditions. The ideal burner is the one which, while meeting the various mechanical requirements, fulfills the above condition under a wide range of gas pressure.

The question as to the relative value of hot raw gas and cold washed gas for boiler use is a very interesting one. I think that in the past the importance of low temperature of washed gas sometimes has been overemphasized. Mr. Huessener has pointed out that so far as temperature is concerned, no advantage accrues in washing gas below 100° F. In many plants, there is a notable operating economy in using furnace cooling water, which is appreciably warmer than the supply water. However, in a given type of washer, other conditions being constant, the lower the exit gas temperature the lower will be the dust content; this is equivalent to saying that the more washing water is used, other conditions being constant, the lower will be the dust content of the exit gas. For each type of washer there is a certain desired average temperature difference between inlet water and exit gas, concomitant with known average dust and moisture contents, and with economical operation. With the present tendency toward smaller stove checkers, it is generally agreed that gas for stove use should be cleaned to a dust content of 0.20 grain or less.

The method employed and extent of gas cleaning are clearly problems for individual furnace plants. As a general proposition, wet washing is decidedly more attractive for furnaces smelting soft, fine Lake ores than for those reducing chiefly hard, lumpy ores, such as the foreign and domestic magnetites. In the former case the average moisture content of the gas is probably 35 grains per cubic foot, the dust content is relatively high and the top temperature relatively low. In our plant, the ore mixtures consist almost exclusively of hard magnetites. The average dust content of the gas leaving the dust catcher will average only about 2 grains and the moisture about 12 grains per cubic foot, corresponding to saturation at about 83° F. The temperature is fully 100° F. higher

* Received Nov. 6, 1916.

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than in Western practice. Owing to the large percentage of gas consumed in gas engines and under boilers in our plant, gas economy is of prime importance. Clearly, dry cleaning under such circumstances is an attractive issue.

With the possible exception of the Halberger-Beth system, the only dry cleaning method of any promise for blast-furnace gas is the Cottrell process.

We installed a small experimental Cottrell electric dust precipitator at one of our furnaces over a year ago, but have had opportunity to begin tests only recently. While we are not yet in a position to publish any results, we have obtained some that are promising, and we are hopeful of the outcome. We have in mind the possibility of fine gas cleaning for gas engines, though at present we are working for primary cleaning only. I see no reason why this process, successful elsewhere, cannot be adapted to the peculiarities of blast-furnace conditions. If successful, the chief advantages which might be expected, in our case the maximum, would be as follows: First cost and operating cost should at least be no greater than with the present cheapest wet scrubbers, for equal dust elimination; conservation of about 10 B.t.u. of sensible heat per cubic foot; higher combustion temperature; probably lower total moisture content; conservation of more flue dust for sintering, and elimination of undesirable sludge basins.

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Calculations with Reference to the Use of Carbon in Modern American Blast Furnaces

BY HENRY PHELPS HOWLAND,* SOUTH CHICAGO, ILL.

(New York Meeting, February, 1917).

INTRODUCTION

DURING the last decade no topic has created more interest or received more thought among blast-furnace men than *coke*.

One reason for this is, undoubtedly, the remarkable increase in the use of byproduct coke. Formerly our coke was made at a distance from the furnaces and by a distinct organization. There was little coöperation or development possible under these conditions.

Today the situation is greatly changed. Much of our coke is made by the organization operating the furnaces and the coke oven is operated with the primary object of obtaining the best results in the blast furnace.

The effect of changing the method of coke manufacture, even in some minor detail, is often felt almost immediately at the blast furnace. There naturally results one of the most valuable assets of the byproduct coke oven, *i.e.*, the possibility of increased blast-furnace efficiency due to close observance and correct operation of the ovens. The results are evident, but often it is difficult correctly to account for them.

In attacking the solution of a problem of as much importance as why one coke is better than another, there are facts which should be kept clearly in mind. The author hopes, in this paper, to bring out some points relative to the use of carbon in the blast furnace which will be of value in solving this question.

GRÜNER'S IDEAL WORKING

Years ago Grüner made a statement regarding the use of carbon in the blast furnace, which has been very generally accepted by metallurgical and blast-furnace men. I quote this theory as given by Prof. Richards on p. 248 of *Metallurgical Calculations:*

"From the standpoint of the generation of the maximum quantity of heat in the furnace, Grüner was right in formulating his dictum of the ideal working of a blast furnace, viz.: All the carbon burnt in the furnace should be first oxidized at the tuyères to CO and all reduction of oxides above the tuyères should be caused by CO, which thus becomes CO_2."

* Superintendent of Blast Furnaces, Wisconsin Steel Co.

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This quotation shows clearly that Grüner was speaking from the standpoint of the heat development in the blast furnace. The blast furnace and metallurgical world in general have corrupted this theory and applied it to fuel consumption. We have come to believe that a low-coke furnace burns a larger percentage of coke at the tuyères than a high-coke furnace.

Prof. Richards has well stated the commonly accepted view on the following page of the same book where he says:

"This proportion, or percentage, will not necessarily express how efficiently the furnace is running as regards fuel used per unit of iron made, but it will tell what proportion of the calorific power of the fuel used is being generated at the tuyères and in possibly *nine cases out of ten* this proportion indicates the general efficiency of the furnace as regards fuel consumption."

The following example illustrates this theory:

Take a furnace using 2,100 lb. of coke of 88 per cent. carbon, or 1,848 lb. of carbon per ton of iron: Assume that 100 lb. of this carbon is not gasified, but is used to supply the carbon for the iron and flue dust, thus leaving 1,748 lb. that could be burned at the tuyères. If we find that 85 per cent. of this, or 1,486 lb., was burned at the tuyères we must then say that this furnace is 85 per cent. efficient as regards fuel consumption. This line of reasoning would lead us to believe that when we reached 100 per cent. efficiency the carbon gasified at the tuyères would be 1,486 lb. and that would be all that was gasified. We would then require in this perfect furnace 1,486 + 100 = 1,586 lb. of carbon or

$$\frac{1,586}{0.88} = 1,800 \text{ lb of coke.}$$

Again, when a furnace was using 2,500 lb. of coke the efficiency would be

$$\frac{1,486}{(2,500)\ 0.88 - 100} = \frac{1,486}{2,100} = 70.8 \text{ per cent.}$$

and only 70.8 per cent. of the carbon should be burned at the tuyères.

The only reason for the belief in this commonly accepted theory must be that no one has ever made any calculations along this line, on furnaces of high- and low-coke consumption.

Calculations Showing Percentage of Carbon Gasified at Tuyères

Somewhat over a year ago our low-coke furnace was found to be gasifying no greater percentage of coke at the tuyères than a previous furnace using 2,100 lb. of coke. This fact being proved for our individual furnaces, it seemed well to compare the results with those on other furnaces. With this in view, data from other plants were obtained and

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the results of the calculations are shown in Table I. The method of calculation is as follows:

All figures are based upon the ton (2,240) of iron as a unit. All volumes are corrected to 62° F. The air blown is assumed to be dry at 62° F. which, of course, while not exactly true, is assumed thus to simplify the problem.

The total carbon charged in the coke minus the carbon in the pig iron, and the carbon in the flue dust, leaves as a remainder the carbon that is gasified and can leave the furnace only in the form of gas. The carbon in the limestone can also leave only in the furnace gas.

The sum of these amounts of carbon gives us the pounds of carbon in the furnace gas that can appear only in the form of CO, CO_2 and CH_4. The pounds of carbon in 1 cu. ft. of either CO, CO_2 or CH_4 is the same, due to Avogadro's law. Hence, having the total pounds of carbon going to the gas, we obtain the cubic feet of CO, CO_2 and CH_4.

The gas analysis shows what per cent. of the total gas these carbon gases constitute and thus enables us to obtain the cubic feet of gas per ton of iron. The gas analysis also gives the percentage of nitrogen and thus we arrive at the cubic feet of nitrogen per ton of iron, and as this can come only from the air, we determine the air actually delivered to the furnace per ton of iron. Dividing this latter figure by 75.8[1] gives us the pounds of carbon burned at the tuyères.

This can then be expressed as may be desired, either in per cent. of total carbon charged or of carbon gasified in the furnace. The method is outlined in a specific case as follows:

Furnace No. 1. (*Plant*) *Wisconsin Steel Co.* (*Date*) *October*, 1914

1. Tons per day, 542.
2. Coke, 1,716 lb.
3. Limestone, 912 lb.
4. Flue dust, 89 lb.
5. Carbon in coke, 87.08 per cent.
6. Carbon in limestone, 12 per cent.
7. Carbon in flue dust, 10 per cent.
8. Carbon in Fe, 4 per cent.
9. Gas, CO_2, 15.7 per cent.
10. Gas, CO, 22.8 per cent.
11. Gas, per cent. CH_4, 0.
12. Gas, N_2, 57.8 per cent.
13. Cu. ft. air per minute = 35,385.
14. Temp. of air to engines, 65° F.
15. Time lost, minutes per day, 21.
16. Pounds carbon to gas = (2)(5) + (6)(3) − (2,240)(8) − (4)(7) = 1,504.5.
17. Per cent. carbon gases in gas = (9) + (10) + (11) = 38.5.

[1] Cubic feet of air at 62° F. required to burn 1 lb. of C to CO.

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TABLE I

Furnace No.	Month Year	Lb. Coke per Ton Iron	Tons Iron per Day	Carbon in Coke, Per Cent.	Kind of Coke		Gas Analysis—Per Cent. by Volume						Pounds Carbon to Gas	Calculated Quantities per Ton Iron			Pounds Carbon per Ton Iron			Per Cent. Carbon Burned at Tuyères	
					Method of Manufacture	Operation	CO_2	CO	CH_4	$CO_2+CO+CH_4$	N	H		Cu. ft. Gas 62°F.	Cu. ft. Air 62°F.	Cu. ft. Air per Lb. Coke	Total Charged	Gasified in Furnace	Gasified at Tuyères	Per Cent. Total Carbon	Per Cent. Gasified Carbon
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22
1	1911	2,615	301	86.3	BH	Stonega	10.3	27.6	0.2	38.1	59.8	2.1	2,251	187,200	141,600	54.2	2,254	2,110	1,868	82.8	88.6
2	1912	2,551	272	84.4	BP	Solvay	11.3	26.9	0.6	38.8	59.0	2.2	2,182	178,200	132,800	52.0	2,153	2,049	1,751	81.4	85.6
3	1913	2,472	482	86.1	BH	Conn.	12.3	25.8	0.0	38.1	58.4	3.5	2,133	177,300	131,000	53.0	2,128	1,996	1,728	81.2	86.6
4	1914	2,247	450	87.1	BH	Conn.	11.8	26.2	0.1	38.1	58.5	3.4	1,978	164,440	121,600	54.2	1,957	1,846	1,605	82.0	87.0
5	1914	2,198	499	86.9	BH	Conn.	12.5	26.8	0.3	39.6	58.0	2.4	1,933	154,600	113,200	51.6	1,908	1,813	1,494	78.7	82.4
6	1914	2,123	541	88.3	BH	Conn.	12.8	26.0	0.2	39.0	58.0	3.0	1,905	154,800	113,500	53.4	1,875	1,764	1,498	79.8	84.9
7	1913	2,115	360	84.3	BP	Solvay	14.2	25.3	0.0	39.5	57.9	2.6	1,826	146,500	108,200	51.2	1,782	1,683	1,427	80.1	84.8
8	1914	1,996	490	86.3	BP	Koppers	13.8	26.0	0.2	40.0	57.4	2.6	1,718	136,100	98,800	49.5	1,722	1,616	1,303	75.6	80.6
9	1915	1,936	376	85.7	BP	Solvay	14.6	24.7	....	39.3	57.4	3.3	1,689	137,000	98,900	51.1	1,659	1,557	1,305	78.8	83.7
10	1915	1,905	393	88.7	BP	Solvay	14.5	26.0	....	40.5	56.4	3.1	1,700	132,900	94,900	49.8	1,690	1,575	1,252	74.1	79.5
11	1915	1,901	517	85.5	BP	Koppers	13.6	24.9	0.2	38.5	58.0	3.2	1,600	131,700	96,600	51.8	1,625	1,514	1,274	78.4	84.1
12	1914	1,863	504	86.6	BP	Koppers	14.0	25.3	0.2	39.5	57.4	2.6	1,602	128,500	90,500	50.0	1,614	1,513	1,230	76.2	81.3
13	1914	1,780	426	84.9	BP	Koppers	14.2	26.0	....	40.2	56.2	3.6	1,519	120,000	85,200	48.0	1,511	1,414	1,124	74.4	79.5
14	1915	1,742	503	84.6	BP	Koppers	15.4	22.8	1.2	39.4	57.1	3.7	1,480	119,000	85,925	49.3	1,474	1,382	1,133	76.9	82.0
15	1914	1,716	542	87.1	BH	Benham	15.7	22.8	....	38.5	57.8	3.7	1,505	123,800	93,200	54.2	1,494	1,396	1,194	80.0	87.0
16	1915	1,715	585	84.6	BP	Koppers	15.4	22.6	1.4	39.4	57.1	3.5	1,456	117,300	84,530	49.3	1,451	1,357	1,114	76.7	82.2
17	1914	1,702	543	87.5	BP	Koppers	14.2	25.2	0.2	39.6	57.3	3.1	1,476	127,500	85,600	50.3	1,490	1,388	1,130	75.9	81.5
18	1914	1,699	572	87.0	BP	Koppers	14.9	23.5	0.2	38.6	57.3	4.1	1,472	120,800	87,600	51.6	1,479	1,376	1,155	78.2	83.9
19	1915	1,673	580	88.6	BH	Benham	15.1	23.6	....	38.7	58.6	2.8	1,477	121,000	89,600	53.4	1,482	1,384	1,182	79.9	85.0
20	1915	1,658	590	88.3	BH	Benham	15.5	22.9	0.1	38.5	58.7	2.8	1,467	120,600	89,600	54.0	1,464	1,366	1,182	80.8	86.5
21	1915	1,636	442	89.5	BP	Koppers	14.2	25.2	0.2	39.6	57.6	2.8	1,464	117,100	85,200	52.0	1,463	1,369	1,124	76.8	82.1
22	1915	1,635	593	88.5	BH	Benham	15.5	23.3	0.0	38.8	58.3	2.9	1,434	117,000	85,200	52.1	1,447	1,349	1,124	77.7	83.4
23	1915	1,624	592	87.3	BH	Benham	14.8	23.6	0.2	38.6	58.3	3.1	1,401	115,000	84,800	52.2	1,417	1,317	1,118	79.0	85.0
24	1915	1,623	457	89.6	BP	Koppers	15.3	24.8	0.2	40.3	57.0	2.7	1,453	114,700	82,600	50.9	1,454	1,360	1,090	75.0	80.2
25	1915	1,589	608	88.3	BH	Benham	15.3	23.5	0.0	38.8	58.2	3.0	1,390	103,400	83,300	52.4	1,403	1,307	1,100	78.5	84.2
26	1915	1,584	466	89.2	BP	Koppers	15.7	24.6	0.2	40.5	56.9	2.6	1,423	111,300	80,100	50.6	1,413	1,324	1,057	74.8	79.9

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TABLE II. (*Data Additional to that of Table I*)

Furnace No.	Dimensions: Heights: Total	Heights: Tuyères to Top Bosh	Diameter: Hearth	Diameter: Bosh	Diameter: Stock Line	Capacity Cu. Ft. Tuyères to Stock Line	No. Tuyères	Date Last Blown In	Per Cent. Silicon in Iron	Per Cent. Carbon: Iron	Stone	Flue Dust	Coke Dried	Screenings	Per Cent. Moisture in Coke	Pounds per Ton Iron: Stone Used	Flue Dust Made	Engine Room Reports: Cu. Ft. Air per Min.	Temp. Air to Tubs	Grains Moisture	Per Cent. Coke Screened Out
1	84′	13′4½″	13′	19′3″	14′	14,060	8	9–09	1.62	4.0	11.8	25.0	88.7	80.0	2.25	1,192	225	30,040	34°	1.83	0.5
2	80′	11′	11′0	16′	12′	9,200	8	9–09	1.59	4.0	11.6	5.4	88.5	80.0	4.0	1,148	275	25,295	.....	.....	0.75
3	89′	13′10″	15′6	20′6″	16′	18,500	12	9–10	2.13	4.0	12.0	11.7	88.1	.....	2.25	1,144	370	45,560	62°	1.55	.....
4	85′	13′0″	16′0	22′0	16′	19,500	12	.....	1.56	4.0	12.0	8.2	89.8	82.0	2.5	1,097	461	44,213	82°	.....	0.56
5	85′	13′0″	16′0	22′0	16′	19,500	12	.....	1.40	4.0	12.0	8.2	89.4	82.0	2.5	1,003	388	45,839	92°	6.44	0.39
6	89′	13′10″	15′6	20′6″	16′	18,500	12	9–10	1.93	4.0	11.7	12.0	90.7	80.0	2.25	1,208	180	46,090	52°	3.15	0.5
7	80′	12′11″	12′0	18′6	13′6	12,375	8	9–12	1.53	4.0	12.05	11.6	89.1	81.5	4.00	1,190	116	32,390	70	4.28	1.5
8	94′	11′7″	17′0	21′3	16′3	20,666	12	2–14	1.03	4.2	11.75	9.0	88.5	.....	2.50	870	140	39,210	38	1.05	.....
9	80′	11′11″	13′6	18′6	14′0	12,459	8	5–15	0.94	4.1	12.05	4.8	89.4	84.0	3.85	1,101	224	30,799	90	5.9	0.3
10	80′	11′11″	13′6	18′6	14′0	12,459	8	5–15	1.25	4.4	12.0	8.5	91.6	84.0	2.9	1,042	192	31,120	85	6.6	0.3
11	Same as		No. 8	.....	.....	.....	.....	.....	0.90	4.25	11.93	11.45	88.3	.....	3.07	719	140	36,824	37	2.58	.....
12	Same as		No. 8	.....	.....	.....	.....	.....	0.95	4.20	11.75	7.55	88.85	.....	2.50	755	98	38,220	54	2.04	.....
13	78′	19′10″	14′	19′6	15′0	15,584	9	3–14	1.26	4.00	11.70	.....	88.00	.....	3.50	897	60	29,670	62	.....	.....
14	85′	14′3″	15′	20′6	15′5″	.....	12	1–15	1.00	4.00	12.00	2.00	89.1	87.0	4.32	828	135	36,368	60	4.0	1.7
15	90′	14′	16′6	21′6	16′0	20,222	12	9–14	1.62	4.00	12.00	10.0	92.0	80.0	1.55	912	89	35,385	65	.....	3.2
16	90′	14′3″	16′6	21′4	16′6	.....	12	9–13	0.89	4.00	12.00	2.1	89.1	87.0	4.32	833	200	42,765	60	3.9	1.6
17	90′	14′6″	17′8	22′0	17′0	20,630	10	2–14	1.23	4.20	11.75	6.2	89.5	.....	2.25	752	95	42,950	49	2.5	.....
18	Same as		No. 17	.....	.....	.....	.....	.....	1.35	4.20	11.75	8.25	89.0	.....	2.30	804	82	48,970	75°	5.5	.....
19	Same as		No. 15	.....	.....	.....	.....	.....	1.56	4.00	12.00	9.5	93.3	80.0	1.85	780	94	35,615	62°	2.2	3.8
20	Same as		No. 15	.....	.....	.....	.....	.....	1.41	4.00	11.9	9.1	92.6	80.0	2.23	849	90	35,820	62°	1.7	2.8
21	Same as		No. 13	.....	.....	.....	.....	.....	1.36	4.1	12.0	4.8	91.8	.....	2.5	792	31	30,050	70	3.8	.....
22	Same as		No. 15	.....	.....	.....	.....	.....	1.62	4.0	11.9	8.0	92.2	80.0	1.4	709	99	35,210	56	3.8	3.0
23	Same as		No. 15	.....	.....	.....	.....	.....	1.48	4.0	11.7	8.8	91.2	80.0	1.6	714	125	35,680	62	1.8	3.0
24	Same as		No. 13	.....	.....	.....	.....	.....	1.19	4.1	12.0	6.9	91.9	.....	2.5	775	.....	29,910	75	5.0	.....
25	Same as		No. 15	.....	.....	.....	.....	.....	1.67	4.0	11.7	7.1	92.3	80.0	1.5	712	94	35,570	62	3.4	3.3
26	Same as		No. 13	.....	.....	.....	.....	.....	1.20	4.1	12.0	7.6	91.5	.....	2.5	828	.....	30,210	80	6.4	.....

Furnaces No. 24 and No. 26 Used More Flue Dust than Produced

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18. Pounds carbon gasified (16) − (3)(6) = 1,395.5.
19. Cubic feet gas at 62° F. = $\frac{(16)}{(17)} \times 31.69$ = 123,839.
20. Cubic feet actual air at 62° F. = $\frac{(16)}{(17)}$ per cent. N 40.06 = 90,492.
21. Pounds carbon gasified at tuyères, $\frac{(20)}{75.8}$ = 1,194.
22. Carbon gasified at tuyères per cent. gasified carbon = 85.5 per cent.
23. Carbon gasified at tuyères per cent. total carbon = 80.0 per cent.
24. Cubic feet of air per pound of coke actual = 52.7 at 62° F.
25. Cubic feet of air per pound of coke, engine reports = 53.7 at 62° F.
26. Cubic feet of air, engine room reports $\frac{(13)(1,440 - (15))521}{(1)(459 + (14))}$ = 92,111 at 62° F.
27. Blowing efficiency = $\frac{(20)}{(26)}$ = 98.2 per cent.

All figures above equation (16) are data for the period of a month; equation (16) is derived thus:

The carbon in the coke (2) × (5) plus the carbon in the stone (3) × (6) minus the carbon in the pig iron (2,240) × (8) minus the carbon in the flue dust (4) × (7) = 1,504.5

(2) × (5) 1,716 × 0.8708 = 1,494 carbon in coke

(3) × (6) 912 × 0.12 = $\frac{109}{1,603}$ carbon in stone

2,240 × 0.04 = 89.6 carbon in pig iron

(14) × (7) 89 × 0.10 = $\frac{8.9}{98.5}$ carbon in flue dust

1,603.0

98.5

(16) 1,504.5 lb. of carbon to the gas.

Equation (18) is obtained by subtracting from the above the carbon in the stone, thus 1,504.5 − 109 = 1,395.5 lb. of carbon gasified in furnace.

Our next step is to find the cubic feet of gas produced per ton of iron. The weight of carbon in 1 cu. ft. of either CO, CO_2 or CH_4 being the same, we derive this value as follows:

Avogadro's law says:

(1 oz.) (molecular weight) CO = 22.4 cu. ft. at 32° F.

(1 oz.) (28) (CO) = 22.4 cu. ft.

1 cu. ft. of CO = $\frac{28}{22.4}$ = 1.25 oz. = 0.07813 lb. at 32° F.

Molecular weight of carbon being 12 and of CO, 28, the weight of carbon in CO will be $\frac{(12)}{(28)} \times 1.25$ oz. = 0.5357 oz. = 0.03348 lb. at 32°.

Weight of carbon in 1 cu. ft. of carbon gas at 62° is thus:

$$\frac{0.03348 \times 491}{521} = 0.031552 \text{ lb.}$$

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Expressing this in the form of its reciprocal we have thus:

1 lb. of carbon going to the gas will make $\frac{1}{0.031552} = 31.69$ cu. ft. of carbon gas at 62° F. Hence if we multiply the pounds of carbon going to the gas (16) by 31.69 we have the cubic feet of carbon gas, thus:

$$1{,}504.5 \times 31.69 = 47{,}678 \text{ cu. ft. carbon gases.}$$

Dividing this by the per cent. of carbon gases in the gas (17) will give the total gas produced at 62° F., thus:

$$\frac{47{,}678}{0.385} = 123{,}839 \text{ cu. ft. gas.} \tag{19}$$

The general formula for gas at 62° F. thus becomes

$$G = \frac{\text{Lb. of carbon to gas}}{\text{Per cent. } (CO + CO_2 + CH)_4} \times 31.69 \text{ cu. ft.}$$

Having obtained our gas, the air is obtained by multiplying G by the per cent. of N in the gas which gives the total cubic feet of N per ton of iron (which is assumed to come only from the air) and dividing this product by 0.791 which is the per cent. of N in air by volume, thus:

N in gas = 57.8 per cent.

$$\text{Air} = \frac{G\,0.578}{0.791} = \frac{123{,}839 \times 0.578}{0.791} = 90{,}492 \text{ cu. ft. air at 62° F.}$$

The general formula for the air direct without obtaining the cubic feet of gas is

$$A = \frac{\text{Lb. of carbon to gas}}{\text{Per cent. } (CO + CO_2 + CH_4)} (31.69) \frac{\text{Per cent. N}}{0.791}$$

$$= \frac{\text{Lb. of carbon to gas}}{\text{Per cent. } (CO + CO_2 + CH_4)} \text{ per cent. N } (40.06) \text{ at 62° F}$$

Our next step is to find the pounds of carbon burned to CO by the air. Referring again to Avogadro's law,

1 oz. (32) O = 22.4 cu. ft. at 32° F.

1 oz. $\quad O = \frac{22.4}{32}$ cu. ft. at 32° F.

1 lb. $\quad O = \frac{(22.4)}{(32)} 16$ cu. ft. at 32° F.

$= 11.2$ cu. ft. at 32° F.

$= 11.2 \times \frac{521}{491} = 11.88$ cu. ft. at 62° F.

CO is composed of 12 parts carbon and 16 parts oxygen, that is, 16 lb. of O is required to burn 12 lb. of C to CO, or 1 lb. of C requires $\frac{16}{12} = 1.333$ lb. of O.

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1 lb. C requires (1.333) (11.88) = 15.84 cu. ft. O at 62° F.
Air is 20.9 per cent. O by volume
Hence

$$1 \text{ lb. C burned to CO requires } \frac{15.84}{20.9} = 75.8 \text{ cu. ft. air at } 62° \text{ F.}$$

That is, it requires 75.8 cu. ft. of dry air at 62° F. to burn 1 lb. of carbon at the tuyères.

As we have found above that we have delivered at the tuyères 90,492 cu. ft. of air at 62°, it is evident that the carbon burned is $\frac{90,492}{75.8} = 1,194$ lb. that is

$$\frac{1,194}{1,494} = 80 \text{ per cent. of carbon of coke.} \quad (28)$$

$$\frac{1,194}{1,396} = 85.5 \text{ per cent. of carbon gasified.} \quad (27)$$

If it is desired to find the blowing efficiency, it may be done as follows:

Time lost per 24 hr. is 21 min., leaving 1,440 − 21 = 1,419 min. of allowing time. This, multiplied by 35,385, gives the cubic feet of air supposed to be delivered by the engines in 24 hr.

Dividing this by the tons produced in 24 hr. and correcting for temperature, we find that our engines were supposedly blowing 92,111 cu. ft. of air at 62°F. per ton of iron. The furnace actually got only 90,497; the blowing efficiency is

$$\frac{90,492}{92,111} = 98.2 \text{ per cent. efficiency.}$$

The actual wind per pound of coke is

$$\frac{90,492}{1,716} = 52.7 \text{ cu. ft. of air.} \quad (24)$$

The wind per pound of coke from engine reports

$$\frac{92,111}{1,716} = 53.7 \text{ cu. ft. of air.} \quad (25)$$

The above line of figuring has been used in calculating the results in table No. 1.

Aside from the fact that there may be some nitrogen from the coke and that we have assumed the air to be dry at 62° F., the method is exact. In my opinion, neither of these points is of enough value to counteract the complications introduced by using them. It is realized, of course, that the value of such a table depends primarily upon the accuracy of the data upon which the calculations are based. When the data were requested from the several different plants, the desirability of having it accurate was given the utmost emphasis.

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Comments on Data

The greatest stumbling block was gas analysis, and I am becoming more and more convinced that the average analysis of blast-furnace gas is almost worthless for calculation purposes. The reason for this lies primarily in its sampling, which, to obtain correct results, needs to be closely followed up by someone who realizes its importance and will see that the sampling is correctly done.

We have followed up this matter quite persistently at our plant for several years. There are two things which, if watched, will, generally speaking, show the accuracy of gas analysis:

First: Per cent. of hydrogen.

Second: The sum of the carbon gases.

Hydrogen.—This figure should be different for different periods of the year. The moisture in the ores and coke gives about 2 per cent. hydrogen in the gas. Five grains of moisture in the blast means about 1 per cent. hydrogen in the gas. Thus, in the winter months, our hydrogen should be about 2.3 per cent. and increase gradually up to 3 per cent. and, possibly, more in the more humid days. Of course, with a dry-blast plant this figure would remain practically a constant at, say, 2.0 per cent.

Regarding the sum of the carbon gases, there is no reason, with a given condition of furnace operation, why this should not remain a practical constant. It certainly should not vary much from day to day and should never be much below 38 per cent. or much above 40.0 per cent.

In some of the data sent me, the hydrogen was as low as 1 per cent. and the sum of the carbon gases varied from 36 per cent. to 41 per cent.

By careful observance of these two points one can be sure of getting as accurate analysis as is practicable to obtain. A gas analysis incorrect as to the sum of the carbon gases will make impossible correct determinations of the wind blown. The degree to which this error affects the wind blown may be illustrated as follows:

Referring to our general formula for air

$$A = \frac{\text{lb. of carbon to gas}}{\text{per cent. of carbon gases}} \text{ per cent. N } (40.06)$$

We will assume three different gas analyses, only one of which is correct:

(1) CO_2, 15 per cent.; CO, 24 per cent.; CH_4, 00; H, 2.5 per cent.; N, 58.5 per cent.

(2) CO_2, 15.5 per cent.; CO, 24.5 per cent.; CH_4, 00; H, 2.5 per cent.; N, 57.5 per cent.

(3) CO_2, 15 per cent.; CO, 26 per cent.; CH_4, 00; H, 2.5 per cent.; N, 56.5 per cent.

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The sum of the carbon gas is 39 per cent., 40 per cent., and 41 per cent. respectively, the 39 per cent. being assumed as correct. Representing all the constants in our equation by K, the equation may be written

$$A = K\frac{\text{per cent. N}}{\text{per cent. sum of carbon gases}} = K\frac{100\text{—per cent. H—per cent. N}}{\text{per cent. sum of carbon gases}}$$

(1) $A = K\frac{0.585}{0.39} = K(1.50)$—Correct analysis used.

(2) $A = K\frac{0.575}{0.40} = K(1.44)$—Incorrect analysis used.

(3) $A = K\frac{0.565}{0.41} = K(1.39)$—Incorrect analysis used.

This indicates that for each 1 per cent. error in reporting the sum of the carbon gas analyses, there will result 4 per cent. error in the calculated wind.

The column No. 5 in Table I headed "Carbon in Coke" may need a word of explanation. The way this figure is obtained at various plants differs. For example, at our plant while we screen out from our pockets about 3 per cent. of the coke, the coke figure per ton of iron is the total coke unloaded from cars into the pockets, divided by the tons of iron produced. In this case the car weights are those of the coke plant located several hundreds of miles from the furnace. At some of the by-product coke plants of the Chicago district the coke is screened at the ovens and the furnace is charged only with the screened coke. The method of arriving at the carbon in the coke is illustrated as follows:

Coke.—Analysis, dried: Carbon, 92 per cent; Moisture, 1.5 per cent; Screenings, 3 per cent. Carbon in screenings, 80 per cent.:

$(0.92 \times 0.985) - (0.80 \times 0.03) = 88.4$ per cent. = carbon in coke.

If the coke unloaded into the pockets divided by the production gives a coke consumption of 1,700 lb. then $1,700 \times 0.884 = 1,503$ lb. would give the carbon charged.

If it is not the practice to charge the furnace with the screenings then the coke consumption would be $1,700 \times 0.97 = 1,649$ lb. and the carbon in the coke $(0.92 \times 0.985) = 90.62$ per cent. The carbon charged would be $1,649 \times 0.9062 = 1,494$.

This latter figure is lower than the correct figure because it is really incorrect to credit the screenings as a certain per cent. of the coke because the carbon of the screenings is lower than the carbon of the coke.

I am confident that on the whole the data used in these calculations are as accurate as could be obtained. Any blast-furnace man who is inclined to question the accuracy of the results obtained has only to apply the method to his own furnaces.

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Conclusions from Table I

Table I shows clearly that, at least in these furnaces, there is no law governing the relation between coke consumption and the per cent. of the carbon which is burned at the tuyères. For example, compare furnaces Nos. 5 and 17:

No. 5. Coke consumption, 2,198 lb., per cent. gasified at tuyères = 82.4.

No. 17. Coke consumption, 1,702 lb., per cent. gasified at tuyères = 81.5.

There is a difference of about 500 lb. of coke per ton of iron, with practically no difference in the per cent. burned at the tuyères.

Starting with the high-coke furnaces at the top and glancing down the table, we find several such instances. Many of the furnaces toward the bottom of the table might be classed as ideal, from the standpoint of coke consumption, yet none of them approach the point of burning 100 per cent. of the carbon at the tuyères.

The conclusion is plain, therefore, that none of the above furnaces (furnaces which I believe to be fairly representative of American blast-furnace practice using Mesaba ores) are working on the basis commonly called "Grüner's Ideal."

The question very naturally arises whether any blast furnace does or ever has gasified 100 per cent. of its gasified carbon at the tuyères. As far as I am aware, no such instance based on accurate data has ever been cited. Furthermore, I believe that there is no reason why this condition should be desired.

Cubic Feet of Air per Pound of Coke

The conclusion just stated vitally affects our conception of the amount of air used to burn a pound of coke in the blast furnace.

Few figures in metallurgical writing are subject to more abuse in their quotation and use than that of cubic feet of air per pound of coke. We hear it quoted all the way from 45 to 70 and usually with no specification as regards temperature. We hear it used as a basis of comparison of blowing equipment and even to prove that Greenawalt sinter is of great value, as compared to some of our fine Mesaba ores.[2]

There are, of course, two values of this figure; the one almost universally used is the wind per pound of coke as quoted in our daily and monthly reports; the other, which is rarely used, the actual value as shown in Table I.

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[2] J. E. Johnson: Increased Fuel Economy in Smelting Iron Ores, *Iron Trade Review*, vol. 54, p. 239 (1914).

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The first value, which we will speak of as the "approximate wind per pound of coke" of course varies greatly. The "approximate" value is generally obtained by multiplying the engine revolutions for 24 hr. by the air supposedly blown per revolution, and dividing this product by the number of pounds of coke supposedly charged during the 24 hr. This gives the usual "approximate wind per pound of coke" for 24 hr. No allowance being made for temperature, this figure cannot be quoted at any standard temperature.

It is only in recent years that any allowance has been made for variations in temperature. Many plants are at present using outside air and varying the revolutions of the engines with the thermometer. At many of these plants the wind per pound of coke is not reported at any standard temperature.

Another error is due to a varying coke unit. At the majority of plants the coke unit varies, as used from day to day, and at the end of the month it is found necessary to adjust the amount of coke charged to the furnace. The approximate wind per pound of coke for the month is, however, generally an average of the daily figure without any such correction being made.

The correct method of deriving this figure is, first of all, to know the temperature of the air entering the air tubs. The total cubic feet of air for the entire 24 hr. (based upon the engine displacement and allowing proper corrections for delays) corrected to 62° F. should be carried forward each day and added for a total for the month. By using this sum as a dividend, and the correct shipper's weight of coke for the month as a divisor, our quotient will be the "approximate air per pound of coke" delivered by the engines at 62° F.

The above method is the way to report our cubic feet of air per pound of coke as delivered by our engines and yet I believe that there are few plants figuring it as carefully as outlined above.

Aside from the two errors in method of calculation mentioned, there are the variables of air tubs, leaks in mains, valves and stoves, all of which tend to make the approximate wind per pound of coke unreliable.

Remarks on Approximate Value

Table III shows some values of this figure which are on record and which are fair samples of the way this figure is quoted. It may be but natural that furnace men have come to look upon much of the variation which has occurred in the quotation of this figure as due to conditions inside of the furnace.

In attempting to account for these variations we have assumed that they were due to the kind of coke, the way the coke was being burned, an easily reduced ore, or an ore difficult to reduce, etc.

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When the facts in the case are realized, we find that practically all of the variation is due to our methods of figuring, variations in temperature, leaks in stoves, mains, engine efficiency, etc.

TABLE III.—*Variation in Quoted Figures of Wind per Pound of Coke*

Period	Number of Furnaces	Average		Individual Furnace Figures							
				Lowest Coke		Highest Coke		Lowest Air		Highest Air	
		Coke	Air per Lb. Coke	Coke	Air per Lb. Coke	Coke	Air per Lb. Coke	Air per Lb. Coke	Coke	Air per Lb. Coke	Coke
Year—1906.....	91	2,343	64.9	1,989	56.8	3,238	60.0	53.1	2,137	82.0	2,419
Year—1907.....	94	2,362	65.0	1,945	68.6	3,212	63.0	48.3	2,473	87.8	2,337
Month—1908...	41	2,321	59.4	1,994	71.6	3,378	52.2	50.5	2,337	71.6	1,994
Week—1913....	10	2,030	67.3	1,703	73.1	2,264	68.7	57.6	2,056	73.1	1,703
Month—1907...	11	2,464	72.6	2,245	85.6	2,996	90.4	60.8	2,396	90.4	2,996
Month—1908...	6	2,375	57.2	2,041	53.5	2,843	62.3	52.0	2,585	65.2	2,287

REMARKS ON ACTUAL VALUE

Turning now to our other value, the actual wind per pound of coke, we find that there are only two causes for variation:

First: The carbon in the coke.

Second: The per cent. of carbon which reaches the tuyères.

We have seen that it takes 75.8 cu. ft. of air at 62° F. to burn 1 lb. of carbon at the tuyères.

Let us assume for illustration a furnace using 2,000 lb. of coke per ton of iron; the coke is charged having 87 per cent. carbon and 100 lb. of carbon being used to supply the iron and flue dust.

It follows that if every pound of coke that went into the top of the furnace reached the tuyères and was burned to CO, the maximum wind possible per pound of coke would be:

$$75.8 \times \frac{87}{100} = 65.95 \text{ cu. ft. air per pound of coke.}$$

This amount of air never could be used, simply because 100 lb. of the carbon was used for iron and flue dust, thus

$2{,}000 \times 0.87 = 1{,}740$ lb. of carbon charged,

$1{,}740 - 100 = 1{,}640$ lb. of carbon gasified,

$$\frac{1{,}640}{1{,}740} = 94.3 \text{ per cent. of total carbon gasified in furnace.}$$

We therefore see that in reality the maximum air that can be used is:

$$75.8 \times \frac{87}{100} \times \frac{94.3}{100} = 62.2 \text{ cu. ft. air per pound coke.}$$

This furnace would be working along the lines of Grüner's Ideal furnace.

We have seen from Table I that under actual operating conditions

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the blast furnace burns at the tuyères more nearly 80 per cent. of the total carbon charged than 94.3 per cent., and on this assumption the actual figure becomes:

$$75.8 \times \frac{87}{100} \times \frac{80}{100} = 52.76 \text{ cu. ft. of air per pound of coke at } 62° \text{ F.}$$

This figure will be a little higher as the coke consumption increases, due to the fact that the carbon necessary for the iron and flue dust is practically the same in pounds, but naturally a lower percentage of the total, thus leaving a higher percentage which can be gasified.

Relation between Wind, Coke and Production

A clear conception of the fact that the actual wind per pound of coke is a practical constant should tend to emphasize in our minds the value of low wind where we are seeking primarily low coke consumption.

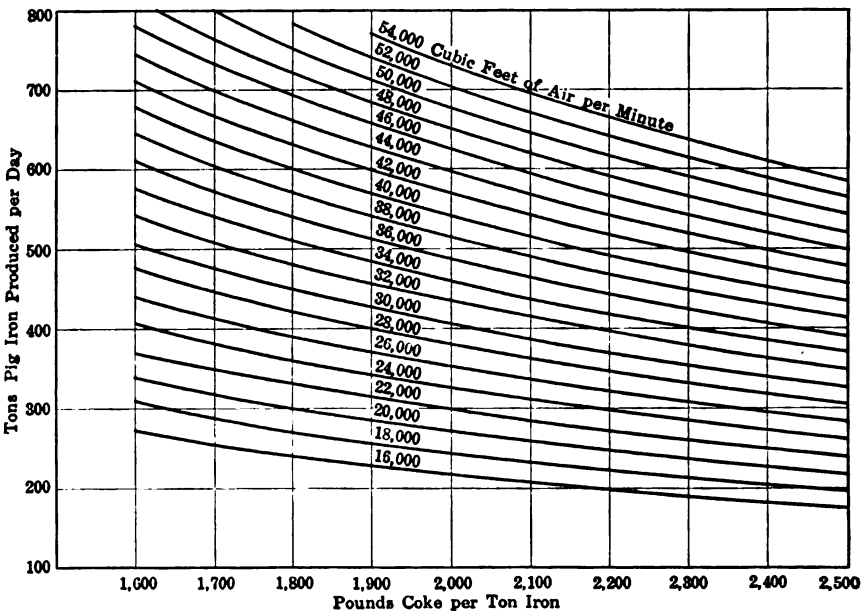

Fig. 1.

Tables IV and V are inserted and Fig. 1 plotted with the idea of emphasizing this point.

These are based upon the following assumptions which, aside from the blowing efficiency, are believed to be practically exact for nearly all furnaces:

First: 52.5 cu. ft. of dry air at 62° F. per pound of coke.

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Second: Engines delivering blast 1,420 min. out of 24 hr., thus assuming a delay of 20 min.

Third: Blowing efficiency 100 per cent.

Let us assume that we have a furnace that should make 540 tons per day. By blowing various amounts of air we obtain the following results:

Cubic Feet Air per Minute	Coke Consumption	Tons per Day
34,000	1,700	541
36,000	1,800	540
38,000	1,900	540
40,000	2,000	540
42,000	2,100	541

On the other hand, if our coke consumption is to stay the same with the increased wind, then our tonnage will have to increase as follows:

Cubic Feet Air per Minute	Tons per Day	Coke Consumption
34,000	541	1,700
36,000	572	1,700
38,000	604	1,700
40,000	636	1,700
42,000	668	1,700

Tables IV and V show with reasonable accuracy the wind we are blowing per minute for any particular furnace. One who is operating a furnace producing 542 tons per day on 2,100 lb. of coke is probably actually blowing about 42,000 cu. ft. of air at 62° F. If, according to engine reports, the figure is 45,000, then the blowing efficiency is $\frac{42,000}{45,000} = 93.4$ per cent.

Fig. 1 shows us at once that it is practically impossible to obtain low-coke consumption unless we keep our wind low.

TABLE IV.—*Relation between Cubic Feet of Air per Minute, Tons of Iron Produced, and Coke per Ton of Iron, Assuming Wind per Pound Coke = 52.5 Cu. Ft. Blast Delivered to Furnace 1,420 Min. per Day*

Cu. Ft. Actual Air per Min. at 62° F.	16,000	18,000	20,000	22,000	24,000	26,000	28,000	30,000	32,000
Coke per Ton Iron									
1,600	271	304	338	372	406	440	474	507	541
1,700	255	287	318	350	382	414	446	477	509
1,800	240	270	300	330	360	390	420	450	480
1,900	228	256	285	314	342	371	399	428	456
2,000	217	243	271	298	325	352	379	406	433
2,100	206	232	258	284	310	336	362	387	413
2,200	197	221	246	271	296	321	345	370	396
2,300	188	212	235	258	282	305	329	352	376
2,400	180	203	226	248	271	294	316	339	361
2,500	173	195	216	238	259	281	303	325	348

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TABLE V.—*Relation between Cubic Feet of Air per Minute, Tons of Iron Produced, and Coke per Ton of Iron, Assuming Wind per Pound Coke = 52.5 Cu. Ft. Blast Delivered to Furnace 1,420 Min. per Day*

Cu. Ft. Actual Air per Min. at 62° F.	34,000	36,000	38,000	40,000	42,000	44,000	46,000	48,000	50,000	52,000	54,000
Coke per Ton Iron											
1,600	575	608	642	677	711	744	778	812	846	879	913
1,700	541	572	605	637	669	701	732	764	796	828	859
1,800	510	540	570	600	630	660	690	720	750	780	811
1,900	484	512	541	570	599	627	655	684	713	741	768
2,000	460	487	514	541	569	595	623	649	677	704	730
2,100	438	463	489	515	542	568	593	618	645	670	695
2,200	418	442	467	493	517	542	566	590	615	640	664
2,300	400	423	447	471	495	518	541	565	589	612	635
2,400	383	406	429	451	474	496	519	541	564	586	609
2,500	368	389	411	433	455	477	498	519	542	563	584

ANALYSIS OF USES OF CARBON

Table I shows a wide range of furnaces considered from the standpoint of coke consumption. The furnace at the top of the list burned 1,868 lb. of carbon at the tuyères, whereas the best furnace burned only 1,057 lb. The work accomplished was approximately the same in both cases. We are not so much interested in the question why the high-coke furnace took 1,868 lb. as we are in how the other furnace was able to get along with 1,057 lb.

It has occurred to me that one help toward solving this problem is to analyze as simply as possible the work done by the carbon in the furnace. I have therefore taken a furnace which, while not the lowest in the list, possibly serves better for comparison because of that fact—furnace No. 19, using 1,673 lb. of coke.

This furnace is No. 1 Furnace of the Wisconsin Steel Company for the month of February, 1915. The data are as follows:

Produced:	580 gross tons of iron per day.
	94 lb. of flue dust per ton of iron.
Consumed:	Coke 1,673 lb. per ton of iron.
	Stone 780 lb. per ton of iron.
	Scrap 75 lb. per ton of iron.

ANALYSIS

	Per Cent.
Iron: Silicon	1.560
Manganese	0.750
Phosphorus	0.075
Carbon	4.000
Sulphur	0.035
Iron	93.580

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		Per Cent.
Coke—as charged	88.6	Carbon
Stone	12.0	Carbon
Flue dust	9.5	Carbon
Gas: CO_2	15.1	
CO	23.6	
H	2.7	
N	58.6	
Ratio, CO/CO_2	1.49	

By the same methods used in calculating Table I, we obtain the following results:

Pounds carbon charged	1,482
Pounds carbon gasified in furnace	1,384
Pounds carbon gasified at tuyères	1,182
Cubic feet of gas at 62° F.	120,950
Cubic feet of air at 62° F.	89,600

	Pounds	
Carbon pig iron	89.6	
Carbon flue dust	8.9	
	98.5	
Carbon in stone	93.6	
CO_2 in stone	$93.6 \times \frac{44}{12}$	= 343 lb.

The carbon in the stone does not, of course, interest us, except that it furnishes 343 lb. of CO_2 which either goes to the gas, or attacks the carbon of the coke.

The Fe per ton of pig iron is: $2,240 \times 0.9358 = 2,095$ lb.

The excess scrap furnishes Fe as follows: $75 \times 0.85 = 64$ lb.

Thus leaving $2,095 - 64 = 2,031$ lb. of Fe to come from Fe_2O_3. This means

$$\frac{2,031}{0.70} = 2,901 \text{ lb. of } Fe_2O_3.$$

A certain amount of carbon is used in reducing the manganese, phosphorus and silicon.

The equations of these reactions are taken from Prof. Richards' book, quoted previously.

$$P_2O_5 + 5C = 2P + 5CO$$
$$Mn_3O_4 + 4C = 3Mn + 4CO$$
$$SiO_2 + 2C = Si + 2CO$$

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The atomic weights used in these equations are as follows:

Fe	56.0
H	1.008
O	16
C	12
P	31
Mn	55
Si	28

$P_2O_5 + 5C = 2P + 5CO$

and produces $\frac{70}{31}$ lb. of CO

1 lb. of P requires $\frac{30}{31}$ lb. of C

Per ton of iron we have—

$2{,}240 \times 0.00075 = 1.7$ lb. of phosphorus,

requiring $\frac{1.7 \times 30}{31} = 1.6$ lb. of C

producing $\frac{1.7 \times 70}{31} = 3.8$ lb. of CO

Manganese reaction—

$$Mn_3O_4 + 4C = 3Mn + 4CO$$

1 lb. Mn requires $\frac{16}{55}$ lb. of C and produces $\frac{112}{165}$ lb. of CO.

Per ton of iron we have:

$$2{,}240 \times 0.0075 = 16.8 \text{ lb. of Mn}$$

requiring $\frac{16.8 \times 16}{55} = 4.88$ lb. of C,

producing $\frac{16.8 \times 112}{165} = 11.4$ lb. of CO.

Silicon reaction— $SiO_2 + 2C = Si + 2CO$

1 lb. of Si requires $\frac{24}{28}$ lb. of C. Produces $\frac{56}{28}$ lb. of CO.

Per ton of iron we have:

$$2{,}240 \times 0.0156 = 35 \text{ lb. of Si}$$

requires $-35 \times \frac{6}{7} = 30$ lb. of C

produces $-35 \times 2 = 70$ lb. of CO

SUMMARY

	Carbon Required, Pounds	CO Produced, Pounds
Phosphorus reduction	1.6	3.8
Manganese reduction	4.9	11.4
Silicon reduction	30.0	70.0
Total	36.5	85.2

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We originally had 1,384 lb. of carbon gasified in the furnace and we have used 36 lb. as shown above, leaving (1,384 − 36) = 1,348 lb. available for reduction of Fe_2O_3.

Of this we know that 1,182 lb. is burned to CO at the tuyères, leaving 166 lb. of carbon which can be used in direct reduction. We will therefore have available for reduction of 2,901 lb. of Fe_2O_3, 166 lb. of solid carbon and $1{,}182 \times \frac{28}{12} = 2{,}758$ lb. of CO.

The amount of Fe_2O_3 we can reduce by means of these agents depends entirely upon how efficiently the work is done. We are told that there are a great many angles to this question of reduction. The temperatures and composition of the reducing gases may so interact upon one another that either oxidation or deoxidation may take place. There are many points involved in this question regarding which the author frankly admits his ignorance aside from what he reads in technical literature, and which he has no desire to discuss here.

Certain it is that our opinions on this topic change from time to time. Sir Lowthian Bell, upon whose researches most of our conclusions are based, remarks that while it might be possible, he doubts the probability of our ever being able to attain a ratio less than 2 to 1, by volume of CO to CO_2 in our top gases.

Our analysis of the top gas on our small hand-filled furnace of yesterday (Dec. 11, 1915) showed a ratio of 1.23.

I shall assume for the purpose of our discussion that the reduction of the Fe_2O_3 by either CO or C takes place under such conditions as to produce a ratio of 1 to 1 in the resulting gas. Any CO in excess of the amount required to reduce the ore under these assumptions must be considered as unused, and as passing to the gases as CO, thus, of course, raising the ratio in the top gases above that of the reducing gases.

In the same way we will have to assume that any solid carbon not necessary for reduction is dissolved by the CO_2 of the gas, thus raising the CO percentage in the top gas.

These assumptions are made merely for the purpose of analyzing the work of the furnace, and as long as this point is kept clearly in mind, no confusion need arise.

The equations of reduction when written to meet these specifications will be as follows:

(*A*) *Indirect:*

$$Fe_2O_3 + 6CO = 2Fe + 3CO + 3CO_2$$

(*B*) *Direct:*

$$Fe_2O_3 + 2C = 2Fe + CO + CO_2.$$

It will be noticed that in the case of the "indirect" reduction we employ 6C to reduce the same amount of Fe_2O_3 that is accomplished by 2C in the equation of the "direct" reduction. According to this line

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of reasoning, direct reduction is three times as efficient, as regards carbon consumption, as is the indirect.

Returning now to the furnace under discussion, we can say that regardless of how the equations are written, we are sure of the following facts:

First.—2,901 lb. of Fe_2O_3 went into this furnace as ore and came out as 2,031 lb. of Fe in the iron and 870 lb. of oxygen in the gases.

Second.—1,384 lb. of carbon went into the furnace as solid carbon and came out as either CO or CO_2 in the gas.

Our problem, as previously stated, is: given 2,901 lb. of Fe_2O_3 to be reduced by means of 166 lb. of solid carbon, and 2,758 lb. of CO. We will make three assumptions:

Assumption A

That 166 lb. of carbon is used in direct reduction of the ore, the remaining ore being reduced by CO, all excess CO passing to the top gases.

The equation for direct reduction—

$$Fe_2O_3 + 2C = 2Fe + CO + CO_2$$

tells us that 166 lb. of C reduces (166)6.667 = 1,107 lb. of Fe_2O_3 and produces

$$(166)(1.167) = 194 \text{ lb. of CO}$$
$$(166)(1.833) = 304 \text{ lb. of } CO_2$$

We have left 2,901 − 1,107 = 1,794 lb. of Fe_2O_3 which must be reduced indirectly. This requires $(1{,}794)\frac{168}{160} = 1{,}884$ lb. of CO for its reduction, and at the same time produces—

$$1{,}884 \times 0.50 = 942 \text{ lb. of CO}$$

and

$$1{,}884 \times \frac{11}{14} = 1{,}480 \text{ lb. of } CO_2$$

We have left—

2,758 − 1,884 = 874 lb of CO which passes to the gases without doing any reducing:

Summary—Assumption A

	Iron Produced	Carbon Used, Pounds	Pounds Produced	
			CO	CO_2
Si, Mn, and P reduction	.....	36	85	0
Direct reduction of Fe_2O_3	775	166	194	305
Indirect reduction of Fe_2O_3	1,256	807	942	1,480
Unused CO	.....	375	874	.....
Stone	.....	.....	.....	343
Totals	2,031	1,384	2,095	2,128

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Assumption B

Assume that all CO made at tuyères reduces ore and that the excess carbon is used up by CO_2.

2,758 lb. of CO will reduce

$$2{,}758\left(\frac{160}{168}\right) = 2{,}627 \text{ lb. of } Fe_2O_3$$

and produce

$$2{,}758 \times \frac{5}{10} = 1{,}379 \text{ lb. of CO}$$

and

$$2{,}758 \times \frac{11}{14} = 2{,}167 \text{ lb. of } CO_2$$

We have left 2,901 − 2,627 = 274 lb. of Fe_2O_3 which must be reduced by direct reduction.
This requires—

$$274 \times \frac{3}{20} = 41 \text{ lb. of C}$$

produces

$$274 \times \frac{7}{40} = 48 \text{ lb. of CO}$$

and

$$274 \times \frac{11}{40} = 75 \text{ lb. of } CO_2$$

We have left 166 − 41 = 125 lb. of carbon for which there is no way of accounting except on the basis of its having been dissolved by CO_2, thus:

$$CO_2 + C = 2CO$$

125 lb. of C will dissolve

$$(125)\frac{(11)}{3} = 458 \text{ lb. of } CO_2$$

and produces

$$(125)\frac{14}{3} = 583 \text{ lb. of CO}$$

It should be noted that this furnace did not produce enough CO at the tuyères to reduce all the Fe_2O_3.

Summary—Assumption B

	Iron Produced	Carbon Used, Pounds	Produced	Pounds
			Pounds CO	CO_2
Si, Mn, and P reduction..	.....	36	85	0
Direct reduction, Fe_2O_3...	192	41	48	75
Indirect reduction.....	1,839	1,182	1,379	2,167
Stone.....	.....	.....	.....	343
		1,259	1,512	2,585
Dissolved by CO_2.....	.....	125	583	−458
Total.....	2,031	1,384	2,095	2,127

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We thus see that in whichever way we assume the reduction to take place the ultimate gas analysis is the same.

The gas analysis obtained by analyzing the gas of this furnace was 23.6 per cent. CO and 15.1 per cent. CO_2, and the cubic feet of gas was 120,950.

$$120{,}950 \times 0.236 = 28{,}544 \text{ cu. ft. of CO}$$
$$120{,}950 \times 0.151 = 18{,}263 \text{ cu. ft. of } CO_2$$
$$(28{,}544)(0.07363) = 2{,}102 \text{ lb. of CO}$$
$$(18{,}263)(0.1157) = 2{,}113 \text{ lb. of } CO_2$$

which shows that our data check fairly well.

Assumption C

Let us suppose for the purpose of comparison that this furnace had consumed the same amount of coke but that the total of 1,348 lb. of carbon had all been burned to CO at the tuyères. Under this assumption the furnace would be working along the lines of Grüner's Ideal. This would require $1{,}348 \times 75.8 = 102{,}178$ cu. ft. of air at 62° instead of 89,600 originally blown.

We will now produce $1{,}348 \times \frac{28}{12} = 3{,}145$ lb. of CO.

We have 2,901 lb. of Fe_2O_3 to reduce which requires $(2{,}901)\frac{21}{20} = 3{,}046$ lb. of CO, and produces

$$3{,}046 \times \frac{5}{10} = 1{,}523 \text{ lb. of CO}$$

and

$$3{,}046 \times \frac{11}{14} = 2{,}393 \text{ lb. of } CO_2.$$

We have left 99 lb. of CO unused.*

Summary Assumption C

	Iron Produced	Carbon Used, Pounds	Pounds to Gas	
			CO	CO_2
Si, Mn, and P reduction	.....	36	85	
Direct reduction, Fe_2O_3	.....	0	0	0
Indirect reduction, Fe_2O_3	2,031	1,304	1,523	2,393
Unused	.....	44	103	0
Dissolved by CO_2	.....	0	0	0
Stone	.....	.....	.....	343
Total	2,031	1,384	1,707	2,736

*Capable of reducing 94 lb. of Fe_2O_3.

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Our gas analysis by volume will therefore become:

$$CO = \frac{1707}{0.07363} = 23{,}183 \text{ cu. ft. } CO$$

$$CO_2 = \frac{2736}{0.1157} = 23{,}647 \text{ cu. ft. } CO_2$$

$$N = (102{,}178)(0.791) = 80{,}823 \text{ cu. ft. of N.}$$

$$CO + CO_2 + N = 127{,}653 \text{ cu. ft.}$$

The hydrogen being 2.7 per cent. of the total, the above sum is 97.3 per cent. of the total gas, or the cubic feet of gas produced at 62° is—

$$\frac{127{,}683}{0.973} = 131{,}195.$$

The gas analysis is:

	Per Cent.
CO	17.7
CO_2	18.0
H	2.7
N	61.6
	100.0

The analysis of the working of this furnace under the three conditions outlined above, is shown in Table VI. In looking over this table we see that had this particular furnace operated as shown in the Column "C" (that is, according to Grüner's law) the net result to the furnace plant would be a distinct loss. While the furnace would have used the same amount of coke per ton of iron the B.t.u. available in the furnace gas is greatly lowered. The result shows that for each pound of carbon gasified in the furnace, we would have used 1,256 B.t.u. more in the furnace when operating as "C" than when operating as the furnace really did. As this furnace gasified 1,384 lb. of carbon per ton of iron, and made 580 tons of iron per day, the net loss would be $\frac{1{,}384 \times 580 \times 1{,}256}{11{,}000}$ = 91,600 lb. of coal in 24 hr., assuming coal to have a heat value as fired in the B.F. boiler of 11,000 B.t.u. per pound.

Comparing the gas analyses on these two furnaces, we see at once that they give no indication of the coke consumption, but only of the way the reduction takes place in the furnace. If we had been able to operate this furnace along the lines of "A" or "B" and produce a gas with the same $\frac{CO}{CO_2}$ ratio as "C" the coke consumption would have been far below that shown in "C."

It is difficult for me to see how this furnace, as operated, would have been improved by endeavoring to operate it along Grüner's ideal, even assuming this to be a possibility.

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TABLE VI.—*Use of Carbon under Various Assumptions*

Per Ton Iron (2,240 Lb.)	Wisconsin Furnace No. 1		
	"A"	"B"	"C"
1. Coke, pounds	1,673	1,673	1,673
2. Carbon charged, pounds	1,482	1,482	1,482
3. Carbon gasified, pounds	1,384	1,384	1,384
4. Carbon reduction Mn. P, Si, pounds	36	36	36
5. Carbon direct reduction Fe_2O_3, pounds	166	41	0
6. Carbon indirect reduction Fe_2O_3, pounds	807	1,182	1,304
7. Carbon in CO not needed for reduction, pounds	375	0	44
8. Carbon dissolved by CO_2, pounds	0	125	0
9. Carbon gasified at tuyères, pounds	1,182	1,182	1,348
10. Carbon used in reducing Fe_2O_3, pounds	973	1,223	1,304
11. Cubic feet air per pound coke, 62° F.	53.4	53.4	61.08
12. Analysis of Top Gas			
13. CO per cent. by volume, per cent	23.6	23.6	17.7
14. CO_2 per cent. by volume, per cent	15.1	15.1	18.0
15. H per cent. by volume, per cent	2.7	2.7	2.7
16. N per cent. by volume, per cent	58.6	58.6	61.6
17. Total B.t.u. per cubic foot top gas at 62°	84.0	84.0	64.8
18. Cubic feet gas per ton iron	121,000	121,000	131,200
19. B.t.u. per pound C gasified	7,344	7,344	6,143
20. B.t.u. in CO per cubic foot top gas	76.6	76.6	57.4
21. B.t.u. in CO per pound carbon gasified	6,697	6,697	5,441
22. B.t.u. lost to top gas per pound carbon gasified	.....	.....	1,256

J. E. Johnson, Jr.,[3] has just finished a very complete analysis of the question of heat development in the blast furnace, and has found himself unable to depart from his original belief in the correctness of Grüner's ideal working. I have not as yet had time to give his article the study it deserves and it may be entirely possible that when I do, my ideas along this line will change.

In Mr. Johnson's article he has attempted to bring into line the results obtained on our No. 1 Furnace with those obtained by his figuring. In so doing, he has quoted our blast heat as 1,250° F. and the carbon in the coke as 93 per cent. The correct figures are 1,100° and 89 per cent.

His whole case has been presented almost entirely from the theoretical standpoint. It would have carried much more weight with blast-furnace men had he presented a few examples based upon accurate data of furnaces, operating along lines he claims to be essential for maximum fuel economy, namely, "Grüner's Ideal." Had such furnaces been doing consistently better work than those shown in Table I, much force would be added to his argument.

[3] Thermal Principles of the Blast Furnace, *Metallurgical and Chemical Engineering*, vol. 13, No. 16 (Dec. 15, 1915).

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Value of Direct Reduction

All the furnaces in Table I are doing direct reducing. All those that are burning less than 1,350 lb. of carbon at the tuyères are not making enough CO to reduce all the Fe_2O_3 and hence, some Fe_2O_3 must be reduced directly by carbon, and, as Prof. Richards points out, this is done three times as efficiently from the standpoint of carbon required, as when done by the indirect method. Certainly as we approach the end of the list, we find that our direct reduction is absolutely essential for there are furnaces toward the end which are not gasifying enough carbon, all told, to reduce the ore, had this carbon first been burned to CO.

I believe that Prof. Richards is absolutely right in his statement on p. 253 of *Metallurgical Calculations* when he says:

> "The ordinary furnace produces at the tuyères, in order to get heat enough to melt down the charges, more CO gas than is needed to abstract all the oxygen from the charges; under these conditions it is uneconomical to oxidize any carbon at all above the tuyères. The exceptional furnace, because of pure ores, small amount of slag, pure fuel, high temperature of blast, or dry blast, gives heat enough at the tuyères to melt down the charges without producing enough CO gas to reduce all the charges; under these conditions, more or less reduction is effected by solid carbon and with the greatest economy in quantity of carbon required in the furnace."

I am sure that the best furnaces, of which I have knowledge, persistently violate Grüner's ideal. Furthermore, the way we are getting our lowest coke records today is by doing a great deal of direct reduction.

Even if we were to admit that the "corrupted" Grüner's theory is correct, it is of little use unless the furnace is acquainted with the theory and willing to work along this theoretical line.

We might illustrate the case by relating the old story of the boy, the dog and the man—the furnace being in this case the dog:

The dog was barking loudly at the boy and giving every evidence of being about to attack him. The man remarked to the boy, "Oh, go ahead, he won't bite you!" The boy said, "I know it." The man asked "Well, why don't you go ahead?" and the boy replied "Well, you see I know it and you know it, but the question is—does the *dog* know it?"

Conclusion

This article has developed along lines which will, undoubtedly, be criticised by some as being of but little practical value. We are, however, able to draw conclusions which will eliminate some of the factors in the question—why does one coke do better work than another? The conclusions would be that it is *not* because:

First: There is any difference in the percentage gasified at the tuyères; or

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Second: There is any difference of wind required per pound of coke; or

Third: There is any great difference in the carbon content of the coke.

Having eliminated these three points from consideration, we must look elsewhere in endeavoring to answer our question.

As noted before, all of our lowest coke furnaces are doing some direct reduction. The heat to enable this direct reduction to be carried on efficiently must come from the carbon burned to CO at the tuyères.

It seems clear, therefore, that in low-coke furnaces, one of the most important, if not the most important, function of the carbon burned at the tuyères is to produce heat to enable the carrying on of the direct reduction, rather than to produce CO for indirect reduction.

On this basis, it becomes very essential that our carbon shall burn instantaneously to CO in order that the resulting heat may be localized where needed. This should not be a question of seconds but of a fraction of a second. If our carbon is of such a nature that this burning to CO is a comparatively long process, more of it will be required than of the quick burning carbon in order to obtain the same concentration of heat at the desired point.

We would, therefore, say that the most desirable thing about a coke is that quality in the carbon which will allow of its being instantaneously burned to CO and thus result in the maximum concentration of heat where needed.

Practically all of the furnaces in Table I using less than 1,700 lb. of coke were using a coke made either wholly or largely from low ash, high volatile coal. While the carbon content is high, it evidently is not the quantity but the quality of the carbon that produces the low-coke consumption.

The low-coke consumption goes hand in hand with low wind. The lower volume of gas resulting means, in a hearth of a given area, a lower velocity of the gases, thus tending toward concentration of heat.

Following the same line of reasoning, we see the value of our big hearth areas and steep bosh angles, both of which tend to decrease the velocity of the gases and concentrate the heat where we need it.

The low wind and large hearths are comparatively easy of obtainment. The great problem will be to operate our coke ovens in such a manner as to obtain coke from inferior coal, the quality of whose carbon shall approach that of the carbon in the coke made from better coals. Judging from the advancement made in byproduct oven operation during the last 10 years, we have every reason to anticipate the correct solution of this problem.

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Discussion

A. H. Lee, Buffalo, N. Y. (communication to the Secretary*).—The statements and results recorded in Mr. Howland's paper can be accounted for by the use of coke with a low sulphur content (0.60 and under). This allows the furnaces to be run on a small volume of acid slag, which it is presumed was done in Mr. Howland's practice. H. A. Brassert has admitted that this was the largest factor in low fuel consumption in his practice. Where the coke contains from 1.00 to 1.25 per cent. of sulphur the situation is quite different and I doubt whether the results mentioned by Mr. Howland could be obtained under such circumstances.

N. M. Langdon, Mancelona, Mich. (communication to the Secretary†).—Grüner's ideal working (which is but a statement of the law first enunciated by Bell) as given in Gordon's *Translation of Studies of Blast-Furnace Phenomena*, "supposes that the reduction of the oxide of iron is effected by the CO only, without intervention of solid carbon."

This ideal, if reached, would mean theoretically perfect or 100 per cent. efficiency of reduction. The workings of the large number of furnaces as given in Bell's and Grüner's great works do not show that this standard of perfection ever was reached. This problem has been worked out by the writer for a number of furnaces, and his results, as well as those given by others, all show a failure to reach perfection. Some furnaces, however, approach nearer to perfection than others. The "ideal" as given by Grüner was not stated as a solved problem in blast-furnace practice, but as an ideal to the attainment of which furnace managers should strive. A study of the factors involved from every angle is interesting and instructive and the paper on this subject contributed by H. P. Howland is no exception to the rule. This article is fairly open to criticism, not because of the unique method of presenting the subject, but for the reason that, as the writer conceives, erroneous assumptions are taken which lead naturally to incorrect conclusions.

Referring to the example of two furnaces compared to show the efficiency of the fuel and, for better illustrating the comparison, rearranging the data as follows:

Exhibit 1

	Coke	Total Carbon	Carbon Used in Iron and Flue Dust	Carbon Burned at Tuyères	Carbon Used for Direct Reduction
1	2,100	1,848	100	1,486	262
1*a*	1,800	1,586	100	1,486	0
2	2,500	2,200	100	1,486	614

* Received May 9, 1916.
† Received May 1, 1916.

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$$1. \quad \left(\frac{1{,}486}{1{,}848 - 100}\right) \times 100 = 85 \text{ per cent. efficiency.}$$

$$1a. \quad \left(\frac{1{,}486}{1{,}586 - 100}\right) \times 100 = 100 \text{ per cent. efficiency.}$$

$$2. \quad \left(\frac{1{,}486}{2{,}200 - 100}\right) \times 100 = 70.8 \text{ per cent. efficiency.}$$

In the above figures no account has been taken of the carbon required for reduction of the metalloids, which is always direct. The general context of the article, as well as the tables and the one case when the amount of carbon required for the reduction of the metalloids is stated, shows it to be small. Therefore, without vitiating the conclusions to be drawn, it may be neglected.

Case (1) shows 85 per cent. efficiency, (1*a*) 100 per cent. and (2) 70.8 per cent. This is correct for the conditions assumed, but a further examination shows 100 per cent. efficiency of carbon burned at the tuyères and the whole of the inefficiency located at the top or reducing zone. Had the reduction of the ore been 100 per cent. inefficient, that is, had the reduction been made with direct carbon only, it would have required 677 lb. of carbon. Letting a equal the amount of solid carbon actually used in the reducing zone, the equation $\frac{677 - a}{677} \times 100$ gives the percentage of efficiency in this zone. Applying to the above we have as the efficiency of reduction:

Exhibit 2

$$\text{Case 1.} \quad \frac{677 - 262}{677} = 61.3 \text{ per cent.}$$

$$\text{Case 1}a. \quad \frac{677 - 0}{677} = 100 \text{ per cent.}$$

$$\text{Case 1.} \quad \frac{677 - 614}{677} = 9.3 \text{ per cent.}$$

These percentages are instructive and show that, in order to secure maximum efficiency in the combustion of carbon in the blast furnace, attention should be directed to the reducing zone.

Quoting, "Table 1 shows clearly that, at least in these furnaces, there is no law governing the relation between coke consumption and the per cent. of the carbon which is burned at the tuyères." For comparison and discussion, the data of the two examples 5 and 18, are stated and some additional data inserted.

Exhibit 3

	Coke	Carbon of Coke Gasified in Furnace	Carbon Gasified		Per Cent. Burned at Tuyères	Efficiency of Reduction	Stone Used
			At Tuyères	Reducing Zone			
5	2,198	1,813	1,494	319	82.4	52.9	1,003
18	1,699	1,376	1,155	221	83.9	67.4	804
24	1,623	1,360	1,090	270	80.2	60.1	775
19	1,673	1,384	1,182	202	85.0	70.2	780

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While the carbon burned at the tuyères has a direct reference to coke consumption, yet a comparison of percentages is misleading, for the reason that this carbon is only a part of the total carbon burned and this other part, burned in the reducing zone, is variable according to the efficiency of reduction. The fact that a furnace is working with low coke consumption does not by any means indicate that it is even closely approaching Grüner's ideal. This is shown by 24 in Exhibit 3. It will be noted that the coke, total carbon gasified, and carbon gasified at tuyères are lower than 18, while the carbon gasified in the reducing zone is higher, showing a lower efficiency of reduction. The carbon required at the tuyères of any furnace is governed entirely by the quantity, composition and character of the materials entering the furnace and the pig iron produced. The carbon required in the reducing zone depends upon the character of the metallic oxides, the composition of the pig produced and largely upon the degree of efficiency of reduction. From what has been said, it is obvious that if this dictum is accepted, under similar conditions that furnace which shows the highest percentage of efficiency of reduction and thus approaches Grüner's ideal will show the lowest coke consumption, and naturally that is the condition to be desired.

The high coke of No. 5 is due to a lean ore mixture as disclosed by the amount of stone (1,003 lb.) used per ton of iron, necessitating a larger amount of carbon burned at the tuyères than in the case of either No. 18 or No. 24, also to the low efficiency of reduction (52.9 per cent.) in the reducing zone. The lower coke requirement of No. 18, as compared with No. 5, is due to a better ore mixture and higher efficiency of reduction, 67.4 per cent., as compared with 52.9 per cent. The still lower coke requirement of No. 24 is due to a still better ore mixture as disclosed by the smaller amount of stone used, permitting a decrease in carbon required at the tuyères to only 1,090 lb. as compared with 1,155 lb. in No. 18; and this decrease is sufficient to more than overbalance the increase of carbon due to lower efficiency, 60.1 per cent., as compared with 67.4 per cent., in the reducing zone.

For discussion of the work of furnace No. 19, the following tabulation is made:

Exhibit 4

	Coke	Fe	Total Carbon	Carbon in Pig	Carbon Required	
					At Tuyères	Reducing Zone
19 A	1,673	2,031	1,482	98	1,182	202
19 B	1,673	2,031	1,482	98	1,182	36
20	1,658	....	1,464	98	1,182	184

Whether the equation is correct or not as stated in Exhibit 5, column

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A, the carbon actually used for reduction and the resulting gas is the same as shown in Column B and this latter equation is simpler.

Exhibit 5

		A	B
Reduction by CO	1	$Fe_2+O_3+6CO=2Fe+3CO+3CO_2$	$Fe_2O_3+3CO=2Fe+3CO_2$
Reduction by C	2	$Fe_2+O_3+2C=2Fe+CO+CO_2$	$Fe_2O_3+3C=2Fe+3CO$

The equation 1 A shows 6CO taken for reduction, only one-half of which was actually used. This unused half, like the bridesmaid at a wedding ceremony, may have performed a very interesting and important part, but, itself, forms no part of the union. This 3CO remains the same after the reaction as before, therefore only 3CO can be considered as having been used, the equation 1 B correctly representing the actual ultimate result.

The reduction of oxide of iron in the blast furnace by solid carbon takes place at a point where the temperature is sufficient in the presence of the surplus of carbon from the fuel to convert any CO_2 which may have formed to CO. The ultimate gas from the reduction by solid carbon is CO only and the ultimate reaction is represented by the equation 2 B.

Reconstructing the summary for "assumption A" which is to illustrate reduction by CO and referring to Exhibits 4 and 5, we have according to equation 1 B the results shown in Exhibit 6.

We see that 166 lb. of solid carbon reduced 517 lb. of Fe, and that 487 lb. of carbon from the total of 1,182 lb. burned to CO at the tuyères reduced 1,514 lb. of Fe. The total CO and CO_2 gases are the same as shown in the original "assumption A." The ratio by volume of $\frac{CO_2}{CO}$ is 0.646, which is very much lower than the ratio 1CO to $1CO_2$ popularly though mistakenly supposed to be the point of saturation by CO_2 at which reduction by CO ceases.

Exhibit 6

		Fe, Pounds	C, Pounds	CO, Pounds	CO_2, Pounds
19 A	In pig	.....	98	.....	.....
	Direct reduction of metalloids	.....	36	89	.....
	Direct reduction of Fe	517	166	387	.....
	Indirect reduction of Fe	1,514	487	.....	1,786
	Unused	.....	695	1,622	.....
	Stone	.....	.....	.....	343
		2,031	1,482	2,098	2,129

$$\frac{CO_2}{CO}=\frac{2,129}{2,098}=1.015 \text{ by weight or } 0.646 \text{ by volume.}$$

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Below is "assumption B" reconstructed according to equation 2 B.

Exhibit 7

		Fe, Pounds	C, Pounds	CO, Pounds	CO_2, Pounds
19 B	In pig	.....	98		
	Direct reduction of metalloids	.....	36	89	
	Direct reduction of Fe	0	0	0	
	Indirect reduction of Fe	2,031	653	.....	2,394
	Unused	.....	529	1,234	
	Stone	.....	.....	.....	343
		2,031	1,316	1,323	2,737
	Carbon saved	.....	166		
			1,482		

$$\frac{CO_2}{CO} = \frac{2,737}{1,323} = 2.069 \text{ by weight or } 1.32 \text{ by volume.}$$

The original "assumption B" is supposed to show the result when "all the CO made at tuyères reduces ore and that the excess carbon is used up by CO_2," as applied to the same data as "assumption A." Assumption B is not tenable because, under the conditions, we must assume the same amount of carbon burned at the tuyères as in assumption A. Now, to reduce the Fe wholly by carbon from CO requires only 653 lb., leaving the balance of 529 lb. of carbon to pass off as CO, and as all of the Fe has been reduced there is an excess of 166 lb. of carbon in the reducing zone which cannot be burned to either CO or CO_2 because there is no oxygen there to unite with it.

The correct statement of the case, assuming the reduction to have been effected with CO only, is shown in Exhibit 7, which shows a saving of 166 lb. of carbon. It may be observed, however, that this saving is probably impossible because of the dilution of the gas by CO_2 which, as shown in the Exhibit, is 1.32. Bell practically states that reduction stops when the proportions are $\frac{CO_2}{CO} = 1.00$.

Assumption G is not well taken as illustrating Grüner's ideal because, in addition to assuming that reduction was by CO only, another unnecessary assumption is injected into the consideration, viz., that 166 lb. more carbon is burned at the tuyères than actually was burned.

Records of heat equations for actual practice of a number of furnaces, collected by the writer, show that for the character of iron given for No. 19, the lowest amount of carbon that can be expected in direct reduction is 156 lb. Assuming this amount for furnace No. 19, we have:

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Exhibit 8

		Fe, Pounds	C, Pounds	CO, Pounds	CO_2, Pounds
19 C	In pig	.....	98		
	Direct in metalloids	.....	36[1]	89	
	Direct in Fe	373	120	280	
	Indirect in Fe	1,658	533	.....	1,954
	Unused	.....	649	1,514	
	Stone	.....	.....	.....	343
		2,031	1,436	1,883	2,297
	Saved	.....	46		
			1,482		

$$\frac{CO^2}{CO} = \frac{2,297}{1,883} = 1.22 \text{ by weight or } 0.777 \text{ by volume.}$$

[1] Equal 156 lb.

This shows a ratio $\frac{CO_2}{CO}$ of 0.777 by volume, and is probably as near to Grüner's ideal as could be obtained in actual practice.

The statement that "all the furnaces in Table 1 are doing direct reducing" is correct, while the statement that "all those that are burning less than 1,350 lb. of carbon at the tuyères are not making enough CO to reduce all the Fe_2O_3" is incorrect. It is also incorrect to say that reduction is three times as efficient when done by the direct method. The oxygen from the iron requires precisely the same amount of carbon for its reduction whether taken from the solid carbon of the fuel or from the carbonic oxide of the gas. When the carbon can be taken from the carbonic oxide produced by the fuel necessarily burned at the tuyères, it obviously saves an equal amount of carbon from the fuel that, otherwise, would be needed and, therefore, is more efficient than solid carbon. The extent to which this can be done is the measure of the approach to the ideal.

All furnaces do not reach Grüner's ideal. In fact, it is very doubtful if any do or can. Some, however, come nearer to it than others. They may show very low coke consumption and yet be far from reaching the ideal. The reason for this is not because of a violation of or considerable variation from Gruner's ideal, but because of some other favorable condition. This is shown in Exhibit 9.

It will be noted that the last 16 furnaces show a decrease of 416 lb. of carbon required, and a decrease of 40 lb. in the reducing zone due to a nearer approach to Grüner's ideal. The great decrease of 375 lb. is in carbon burned at the tuyères evidently due to a better ore mixture, requiring less flux, as shown by the stone used. Less slag is produced, therefore less carbon is required to be burned at this point.

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Exhibit 9

	Total Carbon Gasified, Pounds	Carbon Gasified at Tuyères, Pounds	Carbon Gasified in Reducing Zone, Pounds
Average of first 10 furnaces	1,800	1,522	278
Average of last 16 furnaces	1,384	1,146	238
Difference.....	416	376	40

From what has preceded, it will be evident that the factors governing coke consumption will be found in the quantity and quality of the materials entering the furnace, coke, ore, limestone and blast; also in the method of reduction—that method tending to the highest economy, which approaches nearest to Grüner's ideal.

W. H. Blauvelt, Syracuse, N. Y. (communication to the Secretary*).—I have read Mr. Howland's paper on Calculations with Reference to the Use of Carbon in Modern American Blast Furnaces with much interest, and believe that his method of analysis of the subject will be of material assistance in our study of the action of coke in the blast furnace.

Mr. Howland quotes Professor Richards' statement that from the standpoint of the generation of the maximum quantity of heat in the furnace, Grüner was right in saying that all the carbon burned in the furnace should be first oxidized at the tuyères to CO and all reduction of oxides above the tuyères should be caused by CO, which thus becomes CO_2. It is clear that the maximum quantity of heat is generated in the furnace when the greatest amount of carbon is burned to CO_2 irrespective of whether it is burned at the tuyères or further up the furnace. So it would appear that Grüner's theory is sound regarding the maximum production of heat only, because if all the carbon is burned to CO at the tuyères the reduction of the ores will have to be effected by the further oxidation to CO_2. Whereas, if the reduction of the ores is effected in part by solid carbon, part of this carbon might be oxidized only to CO, resulting in a higher percentage of CO in the escaping gases than from Grüner's "ideal conditions." But in studying the combustion of coke in the furnace, it is clear that the production of the maximum quantity of heat is not of the first importance in blast-furnace operation, or in the utilization of the fuel charged into the furnace. To my mind the production of a high thermal head at the tuyères is of the first importance, and the best coke is that which reaches the tuyères in proper condition to produce the highest temperature at the tuyères, and in just sufficient quantity to do the amount of work required there under the conditions

* Received June 30, 1916.

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produced by this maximum temperature. The combustion of a much larger amount of fuel at the tuyères, under conditions that will fall short of producing the highest possible temperature, cannot produce as good results, either in fuel economy or output. If this statement is correct, does it not correspond with Mr. Howland's statement that too much wind is inimical to low coke consumption? Nothing is more fatal to obtaining the highest temperatures than an excess of air for combustion. In the blast furnace an excess of air dilutes and cools the products of combustion, reducing the maximum thermal head at the tuyères, and the larger volume carries the high-temperature zone too high in the furnace.

There has been a great deal of discussion as to what are the qualities which make the best furnace coke. The wider use of the byproduct oven has brought the control of the coke closer to the furnace man, and makes it possible to modify its structure in many ways, so that the question "what are the best specifications?" has become a very live one. It will probably be generally admitted that furnace coke should be of nearly uniform size, and many furnace managers are eliminating all coke below ¾ in. and above 4 or 4½ in. Also that the best coke is that which is sufficiently strong to resist undue abrasion and crumbling by attrition with the stock, and of an open porous structure that will permit the most rapid combustion when it reaches the tuyères. Many large users agree that the coke should never be overcoked, beyond the point of producing a sufficiently strong structure, as overcoking quickly reduces the combustibility.

If Grüner's ideal gives the best furnace operation, we should want a coke that is resistant to the oxygen in the ore but easily combustible at the tuyères, which is a contradiction of qualities. If my argument is correct, that the furnace man wants the greatest thermal head at the tuyères rather than the production of the greatest quantity of heat in the furnace as a whole, then he is willing to sacrifice some coke by solution in the oxidizing gases in the upper part of the furnace, provided he can obtain a sufficient quantity of coke at the tuyères, of a quality that will permit rapid combustion with the minimum amount of air, thereby giving him the maximum thermal head.

J. W. RICHARDS, South Bethlehem, Pa. (communication to the Secretary*).—Mr. Howland's paper, data and conclusions have interested me intensely, and have led me to study carefully the record of his 26 furnaces therein given.

Mr. Howland is undoubtedly right in his conclusion that when expressing the amount of carbon burned at the tuyères in percentage of the total carbon gasified in the furnace, there is no apparent relation between this percentage and the fuel consumption of the furnace. In the diagram ac-

* Received Nov. 8, 1916.

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companying this discussion the broken line represents the complement of this percentage; that is, the percentage of the carbon gasified which is gasified above the tuyères, or the difference between Mr. Howland's percentages and 100. The irregular character of this percentage is at once evident, showing that there is no simple relation between the percentage of carbon gasified above the tuyères and the fuel consumption of the furnace. It proves conclusively that Grüner's law does not apply either directly or inversely to the working of the blast furnace when the amount of carbon burned at the tuyères and gasified above the tuyères is expressed in *percentage* of the total carbon gasified in the furnace. On this base, therefore, and with this manner of expression, Grüner's ideal working is proved to be neither right nor wrong, but simply no guide at all to the question of fuel efficiency in the furnace.

After contemplating for some time this knockdown for a very much discussed theory, the writer was led to plot diagrammatically Mr. Howland's 26 furnaces, and the accompanying diagram shows the amount of fuel charged, the carbon charged, the carbon gasified in the furnace, the carbon gasified at the tuyères and the carbon gasified above the tuyères (+1,000 in order to get it on the diagram), all expressed in pounds of carbon per ton of iron made.

The conclusion to be drawn from this diagram is evident at a glance. While the carbon gasified at the tuyères varies from 1,868 down to 1,057 lb., a variation of 811 lb. per ton of iron made, the line representing carbon gasified above the tuyères, in pounds per ton of iron made, is nearly horizontal, the maximum variation being from 323 to 184, or 129 lb. Of course, if these variations were expressed in percentages of the carbon gasified above the tuyères, they would be enormous; but such an expression is without significance, just as expressing them in percentage of the total carbon gasified appears to be without significance. The really significant fact is, that in these 26 furnaces with a variation in carbon gasified at the tuyères of 811 lb., the variation in carbon gasified above the tuyères is only 129 lb., that is, the latter item is relatively constant in amount, when expressed per ton of iron made.

Furthermore, the variation in pounds of carbon gasified above the tuyères does not coincide with the variations in amount of fuel used in the furnace, but, as inspection of the lines on the diagram shows, is a wavy line about as high at one end as at the other, having its maximum at furnace No. 10 and its minimum at furnace No. 20, but with no observable regularities in rise or fall paralleling even remotely the consumption of fuel in the furnace. It is very evident that Mr. Howland's figures prove that the amount of carbon gasified above the tuyères, expressed in pounds per ton of iron made, is practically constant in all his 26 furnaces, averaging 250 lb. per ton of iron, and that it is unrelated to and independent of the total fuel consumption of the furnace.

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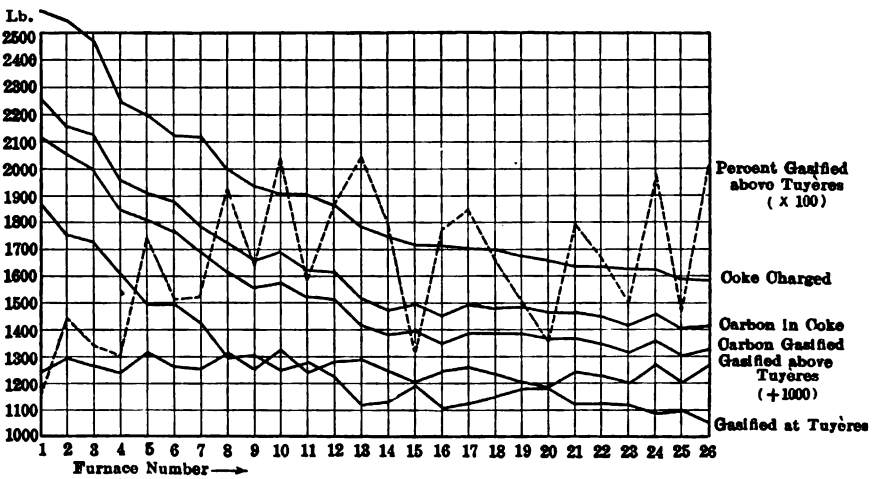
A further conclusion from this diagram is that the amount of carbon gasified at the tuyères is closely parallel to the amount of carbon charged into the furnace. We therefore arrive at the comparatively simple view that in all these 26 furnaces, varying immensely in fuel consumption, the weight of carbon gasified above the tuyères is practically constant, while the weight of carbon gasified at the tuyères is practically a function of the total weight of carbon charged; that is, it is necessarily the total carbon gasified minus the relatively constant amount gasified above the tuyères. We therefore have before us the consideration of a *constant* factor in the fuel consumption, namely, the constant weight dissolved or gasified above the tuyères; and the variable amount burned at the tuyères is simply proportional to the variable amount of carbon charged minus the above-mentioned constant.



This statement would require that the *percentage* of carbon gasified above the tuyères would decrease as the total amount of fuel consumed increased, or would increase as the weight of fuel charged per ton of iron decreased. This is the reverse of Grüner's ideal working, yet it is borne out in a roughly approximate way by furnaces 1 to 10 where the fuel consumption drops rapidly from 2,615 to 1,905 lb. and the percentage of carbon gasified above the tuyères increases rapidly from 11.4 to 20.5. In furnaces 10 to 26, in which the fuel consumption still further decreases, but slowly, no such regularity, even approximate, can be observed; there is practically no observable relation between fuel consumption and this percentage. It would appear from the above discussion that expressing the *amounts* of carbon gasified at the tuyères and gasified above the tuyères, in *percentages* of the total carbon gasified in the furnace is a useless and misleading operation, and that no definite conclusions of value

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can be attained by doing it. On the other hand, for furnaces working under approximately similar conditions of ore, fuel, etc., if these amounts are kept in pounds or kilograms per ton of iron made, a very simple relation appears; namely, that the weight of carbon gasified above the tuyères is practically constant, while the weight of carbon gasified at the tuyères varies in parallel with the total carbon gasified in the furnace, that is, approximately with the fuel consumption.

From an inspection of Mr. Howland's data, the amount of carbon gasified above the tuyères in his 26 furnaces does not appear to vary with size of the furnace or the amount of pig iron made per day or with the kind of coke used. It appears to be a practically constant amount for the whole 26 furnaces, and independent of any of the detailed conditions of operation scheduled by Mr. Howland. It is possible that furnaces run with different kinds of ore of different sizes and with entirely different fuel, such as charcoal, would show greater variations in the amount of carbon dissolved or gasified above the tuyères. Such a comparison would be instructive and bring out the factors which really control the solution of carbon above the tuyères. Such a study is much needed to throw further light upon this interesting discussion of blast-furnace phenomena. But, as far as we have gone, we can say that Mr. Howland's splendid paper has proved this one important and illuminating point, viz.: that the solution or gasification of carbon above the tuyères is independent of the fuel consumption, is practically a constant amount in weight per ton of iron made, and does not condition or determine the fuel efficiency of the furnace in usual furnace practice.

THE CHAIRMAN (JOSEPH W. RICHARDS, South Bethlehem, Pa.).—The record of the 26 furnaces given by Mr. Howland shows conclusively that there is nearly a constant amount of carbon used up in direct reduction above the tuyères of the blast furnace. It cannot be accidental that the amount of carbon consumed above the tuyères in those 26 furnaces varies very little, from the furnace using the most coke to the furnace using the least. As far as these furnaces are concerned, Grüner's ideal working does not apply. There is a constant amount of carbon burned *above* the tuyères, and then all the rest of the carbon which is gasified is burned *at* the tuyères, because that is the only place it can be burned. As far as American furnaces are concerned, I think this conclusion simplifies considerably the discussion of the reduction of iron ore in the furnace.

Abstract of the Discussion by Professor Mathesius (p. 376), *Prepared by Joseph W. Richards*

Prof. Mathesius analyzes the running of Howland's furnace No. 19, according to the method described by him in *Stahl und Eisen*, 1916,

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Nos. 30 and 31, and in his work "Die physikalischen und chemischen Grundlagen den Eisenhüttenwesens" (Spamer, Leipzig).

By assuming 500 Cal. lost from the hearth by radiation and cooling water per kilogram of pig iron made, a throat-gas temperature of 600° C., and taking the ratio of CO_2/CO as 0.64, he finds that his diagram would show 0.68 kg. of carbon oxidized per kilogram of pig iron, while Howland gives 0.66 kg. He expresses himself as well satisfied with this test of the accuracy of his theory. Making the further assumption that the heat losses from the shaft are 1,000 Cal. per kilogram of pig iron (twice as great as from the hearth), he calculates that 9.2 per cent. of the iron oxide charged is reduced in the hearth, and 90.8 per cent. in the shaft. Of the latter 15.8 per cent. is also "direct reduction" leaving 75 per cent. indirect reduction, in the shaft, by CO gas, and a total of 25 per cent. direct reduction.

He compares this result with ordinary blast-furnace practice, saying that in foundry iron furnaces three-fourths of the direct reduction ordinarily takes place in the *shaft* and one-fourth in the *hearth*, while in Thomas Basic pig-iron furnaces about 70 per cent. of the direct reduction takes place in the *hearth* and 30 per cent. in the *shaft*. He ascribes this difference to the shaft being much warmer in the foundry iron furnaces.

By rather involved and strained reasoning, Prof. Mathesius then attempts to show that if the blast temperature were raised to 800° C., the furnace could get no advantage from this, using the same ore; the only advantage would be that a more difficultly reducible ore could be used, and worked as efficiently as the more easily reducible ore.

WALTHER MATHESIUS, Charlottenburg, Germany (communication to the Secretary*).—Howland bases his conclusions on the data of Grüner[1] who in his blast-furnace calculations makes the rather idealistic statement that the best results are obtained in a furnace in which the iron ore is reduced solely by the rising gases in the shaft.

Howland's paper contains a table compiled from carefully collected data in which a review is given of the results and conditions of 26 American blast furnaces. These furnaces use mostly Mesaba ore and the results are in most cases very good.

His calculations refer especially to furnace No. 19 in the table, a furnace of the Wisconsin Steel Co. which excels the others in efficiency.

In his investigation he draws the following conclusions: There is no law which governs the relation between coke consumption and the percentage of carbon burned at the tuyères. None of the furnaces in the table work according to the so-called ideal method as stated by

* Received Dec. 19, 1916.

[1] M. L. Grüner: Analytische Studien über den Hochofen (1875), Wiesbaden, C. W. Kreidels Verlag.

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Grüner. As far as the author is informed, there is no record of a blast furnace anywhere in which 100 per cent. of the gasified carbon was

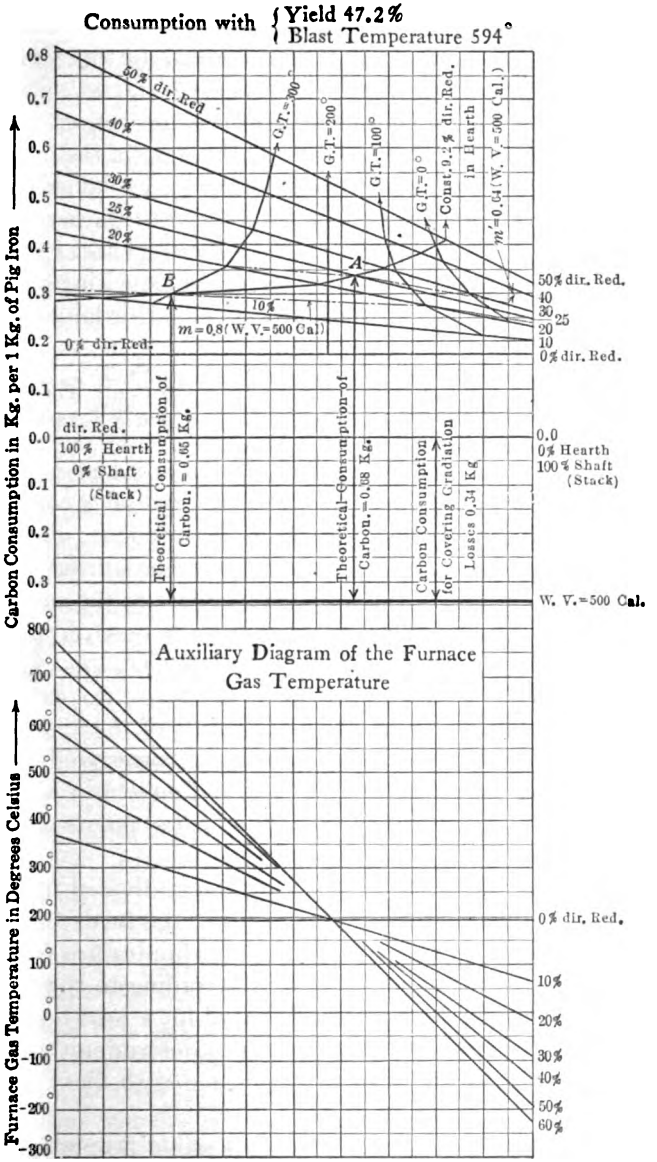

FIG. 1.

gasified at the tuyères. Moreover, the author does not consider such a condition desirable.

He reaches these conclusions by comparing the actual working conditions of the furnace with three theoretical working assumptions which

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he develops in detail in his paper. The furnace, however, does not work according to either theory, as the author tells us, and yet the results are throughout better than they possibly could be according to his theories.

It seemed interesting to me to calculate the working conditions of the furnace of the Wisconsin Steel Co. according to the complete theories of the blast furnace that I have developed.[2]

The calculated results are appended to this discussion. The results are plotted in Fig. 1, which clearly shows all the steps of the process. The development and construction of such a diagram is explained in the above cited references to my papers on this subject. For those of my readers who are not familiar with them I give the following short explanation:

The diagram assumes the blast at 600° C. and smelting pig iron of the following composition (the same as furnace 1 in Howland's table):

Si, Per Cent.	Mn, Per Cent.	P, Per Cent.	C, Per Cent.	S, Per Cent.	Fe, Per Cent.
1.560	0.750	0.075	4.000	0.035	93.580

A heavy line in the middle of the diagram is taken as abscissa, on the left side of which have been recorded the working conditions that would result if 100 per cent. of the Fe_2O_3 were reduced directly in the hearth. On the right side of the abscissa are recorded the working conditions when the total direct reduction takes place in the shaft, in such a manner that the CO_2, formed by the reduction of Fe_2O_3 by CO, is again completely reduced by the carbon of the coke to CO.

All possible intermediate steps between these two extremes mentioned are recorded on some intermediate points of the abscissa.

The ordinates of the diagram represent the corresponding consumption of carbon from coke (exclusive of ash and moisture) which is necessary for the working of the furnace at the indicated points of the abscissa line.

The consumption of carbon from the coke is divided into two great parts, one of which makes up for heat losses of the furnace due to water cooling and radiation. The corresponding ordinates are carried downward from the zero line in the center and according to the heat balance of the furnace under discussion are calculated for a heat loss of 500 Cal. per kilogram pig iron in such a manner as to determine the quantity of carbon from the coke which has to be burned with blast at 600° C. in order to furnish the required calories.

Since this condition holds good in all possible proportions of direct reduction between hearth and shaft, the consumption of carbon from the coke must be represented in a line passing the diagram parallel to the

[2] *Stahl und Eisen*, vol. **30**, p. 31 (1916), and *Die physikalischen und chemischen Grundlagen des Eisenhüttenwesens*, Verlag Spamer, Leipzig, Germany.

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axis of the abscissa and at a proportionate distance from it. That line has been marked W. V. (which means heat loss) = 500 Cal.

The second large part of the consumption of carbon from the coke arises from the necessity for covering the heat requirements of the chemical and physical reactions taking place in the furnace. The ordinates corresponding to this heat consumption are carried upward from the abscissa. The upper ends of these ordinates start from a straight line and pass through the diagram from right to left. They are marked 0, 10, 20, 30, 40 and 50 per cent. direct reduction, respectively.

To better characterize the working, some other lines have been plotted on the diagram which were computed according to my theory of blast-furnace practice as mentioned above.

From Howland's article it appears that the furnace gases had a volumetric proportion of $CO_2/CO = 0.64$. A dotted line in the diagram shows how this proportion must form itself in the furnace according to the working conditions and especially according to the distribution of the direct reduction between hearth and shaft, taking into account a heat loss of 500 Cal. per kilogram of pig iron. A comparison of this dotted line with the lines indicating the percentage of the direct reduction brings out that in this particular working the total direct reduction is within 20 to 30 per cent.

The working data that we have been using do not, however, enable us to determine how the direct reduction is divided between hearth and shaft. The only data for that purpose concern the temperature of the furnace gases.

Howland does not give any such temperatures, and I had, therefore, to get such data from other American blast-furnace reports. I took these from a paper by Brassert[3] which was abstracted in *Stahl und Eisen*. He gives 160° C. as the average temperature of the furnace gases.

The diagram of my theory of blast-furnace practice makes it possible to determine on the basis of this gas temperature how the direct reduction in the blast furnace is distributed between the shaft and the hearth. This is accomplished by putting into the diagram all those gas temperatures that must result at a working yield of 47.2 per cent., a blast temperature of 600° C., and a heat loss of 500 Cal. per kilogram pig iron. The plotting of these lines showing the temperature of the furnace gases is facilitated by a guide diagram at the bottom of the large diagram, the construction of which has been developed in my previously mentioned publications.

Since the lines of the furnace gas temperatures cut the line of the $m - (CO_2/CO)$ proportion at certain points, it is easy to determine a point corresponding to a temperature of 160° C. by means of interpola-

[3] Brassert: Neuzeitliche Entwiebelung des amerikanischen Hochofenbetriebes, *Stahl und Eisen* vol. **36**, No. 3, p. 61 (January, 1916).

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tion between the points of intersection giving the temperatures of 100° C. and 200° C. respectively. The point so found corresponds exactly with the actual working condition of the furnace under consideration. Accordingly, we can see on the diagram that the total direct reduction is 25 per cent., 36.5 of which takes place in the hearth and 63.5 in the shaft; in other words, 25 per cent. is the total direct reduction, 9.2 per cent. takes place in the hearth and 15.8 per cent. in the shaft. This conclusion gives us a clear conception of a furnace using Mesaba ore, showing that with the prevailing temperatures and composition of gases, and the working speed of this furnace of the Wisconsin Steel Co., only 9.2 per cent. of the Fe_2O_3 charged at the furnace entered the hearth unreduced. It must be remembered, however, that the theoretical calculation could determine only the percentage of Fe_2O_3 charged that entered the hearth unreduced. There is at least a strong probability that those parts of the ore are no longer in the unchanged form of Fe_2O_3 but have already undergone a gradual decomposition to Fe_3O_4 or FeO. The statement that 9.2 per cent. of the burden enters the hearth unreduced consequently implies that the quantities of oxygen to be directly reduced in the hearth by carbon and carbon monoxide are as great as if 9.2 per cent. of the ferric oxide entered the hearth unchanged. It is not to be doubted that more heat units are necessary for the direct reduction where the oxygen to be combined with the carbon is present in the form of ferrous oxide instead of ferric oxide. But these differences of energy can be neglected, as they are not large enough to cause an appreciable fault in the calculations.

It can be concluded that this percentage would not change appreciably if the percentage of the total direct reduction of the ore was changed, provided, however, the quality of the ore, the blast temperature and the working speed remained the same. This offers the possibility of plotting a new line in the diagram which up to now I had not considered in my theoretical investigations of blast-furnace practice. The new line would indicate under which condition the percentage of direct reduction in the hearth would remain 9.2 per cent. even if the total direct reduction should change within wide limits. This line is marked on the diagram: "Const. 9.2 per cent. dir. reduction in hearth," and is the locus of all points with direct reduction in the hearth of 9.2 per cent. It is to be considered as a standard line for the degree of reducibility of the smelted ore under the given rate of production or working speed.

From the position of the working point of the Wisconsin Steel Co. furnace in the diagram, it becomes possible to read without further computation that the actual consumption of carbon in the operation of this furnace must be 0.68 kg. of carbon per kilogram of pig iron in accordance with the conclusions arrived at in my blast-furnace theory.

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Now according to Howland, the consumption of carbon per kilogram of pig iron is really 0.6615 kg.

The concurrence of these two figures can be considered unexpectedly satisfactory, especially so in consideration of the fact that in the framing of my theory several necessary assumptions could not be avoided (as, for instance, the apportioning of radiation losses between hearth and shaft at ⅔ to ⅓, etc.), the admissibility of which as before conceded would have to be established by considerable number of carefully computed corroborative furnace operations.

Under detailed investigation of the stated assumption in the single case under consideration, the precise establishment of a hearth balance as given in the appendix shows the presence of a heat loss of 322 Cal.; this figure approximates sufficiently the value of ⅔ of 500, namely 333⅓.

The actual working conditions of the furnace under discussion are completely elucidated by the above data.

In spite of these conditions, the question is still permissible, whether, and in what measure, a saving of coke could yet be effected by an eventual change of procedure.

Since the percentage of direct reduction in the hearth is as low as 9.2 per cent. of charged Fe_2O_3, much further curtailment could not be expected to bring practical results. The direct reduction could in no case be appreciably lowered and no noteworthy decrease of coke consumption could result from such diminution.

On the contrary, it is worth mentioning that in this particular operation 15.8 per cent. of the direct reduction takes place in the shaft.

The further discussion, therefore, is directed to the task of determining whether, and by what means, in further diminishing this portion of the direct reduction, a saving of coke could be effected.

From the diagrams in my publications on the operation of furnaces for foundry and Thomas iron conducted on equal output and blast temperatures, it becomes evident that in the production of foundry iron generally the apportioning of the direct reduction between hearth and shaft is as follows: About 75 per cent. of it belongs to the shaft and 25 per cent. to the hearth, while in the majority of Thomas iron furnaces the direct reduction taking place in the hearth is 70 per cent. to only about 30 per cent. in the shaft. This noticeable difference is exclusively due to the fact that in the production of foundry iron the temperatures in the hearth must be maintained at a higher degree to obtain a considerable reduction of silicic acid, while in the Thomas iron process, on the contrary, it is necessary to keep the temperature of the hearth low enough to avoid an undesirable reduction of silicic acid. The consequence is that at the present rate of production for foundry iron the shaft is considerably better than for producing Thomas iron. From this

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results necessarily a considerably increased transformation into CO of the CO_2 formed in the lower portion of the shaft by the ore reduction, therefore greater direct reduction in the shaft. When the diagram established

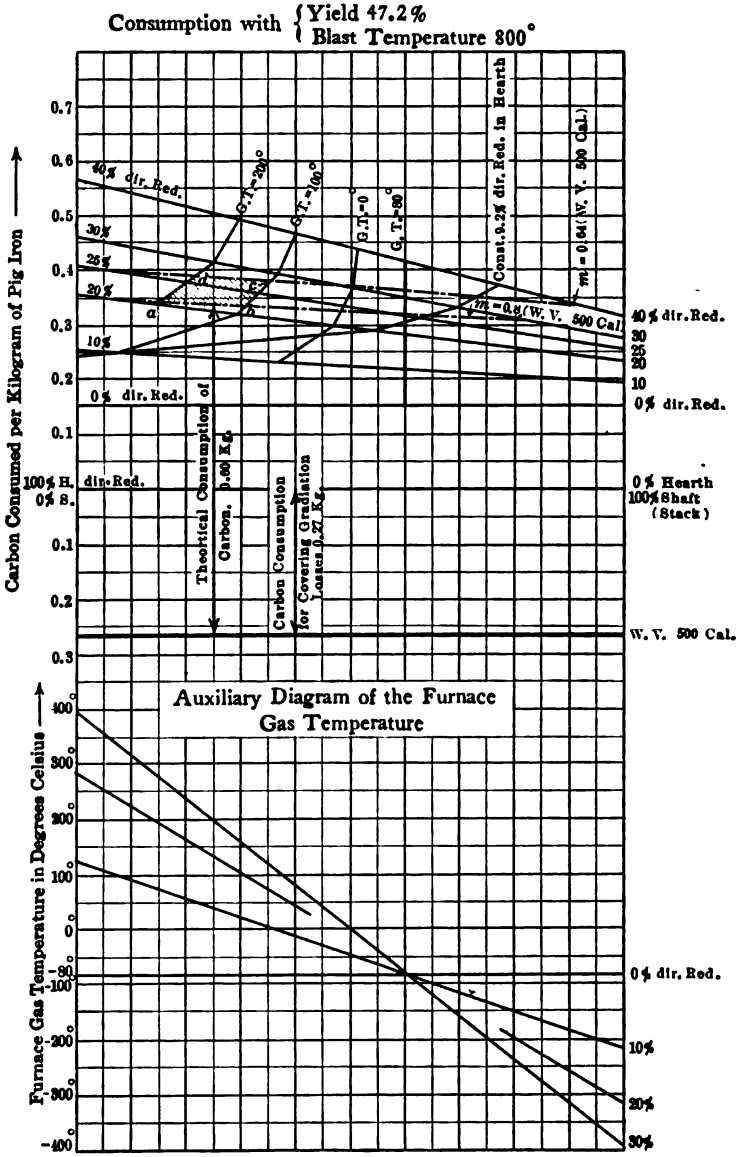

FIG. 2.

for gray iron and Thomas iron on otherwise identical conditions of operation are contrasted, the corresponding shifting of the lines of gas temperatures in the upper portion bring out these conditions saliently.

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Similar shiftings of the lines of gas temperature are seen in the diagrams established for identical operations of gray iron and Thomas iron when the two diagrams are contrasted for the differences in blast temperatures, which shows unmistakably that under otherwise equal conditions a higher blast temperature produces a lower gas temperature and this result corresponds with practical experience.

The lower gas temperature indicates generally a correspondingly lower temperature of the shaft. To diminish the direct reduction in the shaft of a blast furnace, the most feasible means advisable under otherwise identical conditions is to increase the blast temperature.

In the case of the Wisconsin Steel Co. furnace, the blast temperature could be carried considerably beyond the present 594°, since even for Thomas iron temperatures up to 800° practically prevail in Germany. A diagram establishing working conditions for this furnace with blast temperatures at 800° would obviously be interesting and is furnished in diagram No. 2.

Detailed computation of data relating to it is not contained in the appendix since it is merely a guide to the calculations and an exhibition of results; it is otherwise based on the constant operating conditions used for diagram 1, and at the same rate of production. Consequently the heat loss here too is 500 Cal. per kilogram pig iron.

The salient fact in this diagram for 800° is that the coke carbon consumption to cover the heat losses, which in the blast furnace operated with 600° blast temperature is 34 kg. per kilogram of pig iron, drops to 27 kg. The considerable saving of coke hereby indicated is obtained simply because more heat units per kilogram of pig iron are carried into the hearth by the higher blast temperature, so that to cover the heat loss, less carbon combustion is required.

But what is the consumption of coke carbon in the upper portion of diagram 2? Two m lines have been introduced in this diagram corresponding to m-proportion of 0.64 and 0.80. The reason for this procedure is based on a note in Howland's publication according to which the 0.80 proportion has actually been observed on a specially propitious day of operation.

If an m-proportion of 0.80 was actually obtainable operating with a blast temperature of 600°, it is reasonable to assume that the same m-proportion can be obtained with higher blast temperature, for a decrease in the coke carbon required should result since the same quantities of oxygen from the ore are transferred to lesser quantities of gas. Whether in reality these more favorable m-proportions are obtainable, remains obviously to be proven by actual observations in practice.

The lines of gas temperature obtained exactly as in diagram 1 were also entered in the upper portion of diagram 2. Practically normal operating conditions can naturally only exist with a minimum gas tem-

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perature of about 100°; and again only practical operation can show whether and in what measure the temperature of the furnace gas exceeds the 100° limit. There is no reason why the temperature should be higher than with a 600° blast; at least we can assume that it cannot exceed 150°.

All possible cases of furnace operations are embraced in diagram 2, in a quadrangle marked *abcd* which is formed by the intersections of the *m* lines and the lines of gas temperatures of 100 and 200°.

Presupposing the same rate of production, it is safe to assume that the direct reduction in the hearth, with the same ore, will not be materially altered from that exhibited in diagram 1. In the upper part of diagram 2, however, the line indicating the direct reduction in the hearth (9.2 per cent.) does not intersect the quadrangle *abcd*. Evidently, then, a furnace operated under conditions represented by diagram 2 must obtain a heat surplus in the hearth, supposedly resulting in considerably increased reduction of silicic acid much beyond the aims of operation; in other words, a furnace operated in this manner would produce pig iron containing an undesirable excess of silicates.

In the experience of practical furnace operations, it has been found that the temperature in the hearth is correspondingly lowered by decreasing the quantity of coke. Obviously, then, the temperature could be brought to the point where the desired admixture of silicates in the pig iron results by simply decreasing the coke quantity in the burden.

From the diagram, however, it is further evident that operating along these lines would be impracticable because it would result in gas temperatures below 100° and because an *m*-proportion would be obtained of a magnitude, at least at present, considered unattainable; or that the experiment to stimulate furnace production of pig iron with a normal percentage of silicates by the use of blast temperature of 800° is doomed to disappointment.

This recognition most probably explains why a blast temperature of 800° has practically not been introduced in the furnace under consideration.

The diagram points to the fact that such operation could be carried into effect if, with the introduction into the burden of less easily reducible ingredients, the quality of the charge were altered until the line in the diagram representing the direct reduction in the hearth cut the quadrangle *abcd*.

We learn from this discussion of operating conditions that using somewhat easily reducible ores, such as the Mesaba ores, with blast temperatures of 800°, under ordinary circumstances pig iron with relatively low percentage of silicon is not obtainable unless less easily reducible admixtures are added to the charge in such proportion that the line of constant reduction in the hearth intersects the quadrangle *abcd* in the dia-

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gram. It is immaterial whether this addition consists of magnetite or corresponding percentages of slow reducing agglomerates.

Thus we arrive at the interesting observation, that furnaces working with easily reduced ores under conditions as represented can employ higher blast temperatures effectively only if the charge is correspondingly altered to become less easily reducible. This result is directly contrary to those in German operations, where a saving of coke can generally be gained by decreasing the mass of slow-reducing constituents in the burden, as the percentage of direct reduction in the hearth in most of these cases is above the requisite. And so the question arises whether saving of coke is actually possible in the manner here described. Diagram 2 furnishes the answer, showing that the consumption of coke carbon can reach the amount of 0.60 kg. only if in the new operation an m-proportion of 0.80 per cent. and a gas temperature of 150° is present. This would result in a saving of 0.08 kg., or about 12 per cent., of coke when compared with coke carbon consumption in diagram 1.

It should, however, be observed that increasing the blast temperature is not the only means available in furnaces to obtain a decrease of direct reduction in the hearth of the furnace under consideration, since altering the quality of coke should produce similar results. The use of denser coke, for instance, would decrease the reductive action on the CO_2 rising from the lower portions of the furnace. The lower limit of operation thus attainable would be indicated in the diagram and would be at the intersection of the line of constant reduction with the line of the m-proportion = 0.80. According to experience at present, a better m-proportion cannot be assumed. The working condition thus attained should correspond with operation point B in diagram 1, which would point to a coke consumption of 0.65 kg. and a corresponding saving of coke of 4.6 per cent. The diagram, however, makes it also evident that in such a case an increase of the blast temperature could not effect a further economy of coke because then an m-proportion of unattainable magnitude would have to be present.

The working out in practice alone can show whether a denser quality of coke could be used without interfering with the present rate of production, or whether the use of such a denser and consequently less easily combustible coke would not materially retard the course of reduction in the hearth, lower the production and correspondingly increase the heat loss per kilogram of iron, and therefore necessitate a higher coke consumption.

A third means to decrease coke consumption would be the acceleration of the rate of production by which the proportionate heat loss per kilogram of iron would be lowered together with its attendant coke consumption.

In German operations, with charges of smaller yield than here em-

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ployed, a not inconsiderably larger production per cubic meter of furnace capacity has actually been achieved. In regard to this there are two limits, the one being the combustibility or speed of reaction of the coke in the hearth, and the other the duration of the time that the molten mass remains in the hearth being so shortened that it is insufficient to allow a completely adequate separation of the smelted iron drops from the slag. In such a case the slag drawn from the hearth contains such a considerable quantity of fine suspended particles of iron that important losses of metal result. This limit must admittedly be variable according to the viscosity of the molten slag.

Recapitulation

The foregoing review based on detailed observation concerning the operation of a blast furnace furnished by Howland and with the aid of the theory of the reduction process developed by the author, demonstrates that a correct idea can be obtained of the process of operations taking place in the furnace under discussion.

The coke consumption arrived at theoretically coincides sufficiently with results established in practice.

It also demonstrates that even in an operation as perfect as the present one a saving of coke could safely be expected by an increase of the blast temperature from 600° to 800° and some further measures.

It may safely be anticipated that the systematic elucidation of conditions in other furnaces might in a majority of cases lead to the discovery of ways and means for further considerable saving of coke.

Abstracts from the Calculations

Concerning the working of furnace No. 19—Table 1, p. 342—the following data are given for the month of February, 1915:

American Measures		German Measures
1. Production	580 tons of iron	589 tons of iron
2. Consumption of coke	1,673 lb. containing 88.6 per cent. C	747 kg. per ton of iron
3. Consumption of limestone	780 lb. per ton of iron (100 per cent.)	348 kg. per ton of iron
4. Addition of scrap	75 lb. per ton of iron (85 per cent. Fe)	33.5 kg. per ton of iron

Analysis of produced pig iron:

	Per Cent.
Si	= 1.560
Mn	= 0.750
P	= 0.075
C	= 4.000
S	= 0.035
Fe	= 93.580

Analysis of furnace gas:

CO_2 = 15.1 vol. per cent.; therefore m' = 0.64.
CO = 23.6 vol. per cent.
N_2 = 58.6 vol. per cent.
H_2 = 2.8 vol. per cent.

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Moisture in coke = 1.85 per cent.
Blast temp. = 594°.
Furnace gas temp. not given.

The consumption of pure carbon to produce 1 kg. of iron is therefore $0.747 \times \frac{88.6}{100} = 0.6615$ kg. That is assuming the average constituents of the Mesaba ore, according to literature, to be as follows:

1. Amount of iron in the ore 53.8 per cent.
2. Amount of H_2O in the ore 11.9 per cent.
3. Percentage of ash in coke 7.5 per cent.

Calculation of Production

One ton of iron contains 935.8 kg. Fe, of which $0.85 \times 33.5 = 28.5$ kg. are furnished by the scrap and the remainder of $935.8 - 28.5 = 907.3$ kg. by the iron ore. The ore contains 53.8 per cent. Fe = 76.9 per cent. Fe_2O_3. Since 907.3 kg. Fe are equivalent to 1,295 kg. Fe_2O_3, the amount of iron needed for 1 ton of iron is: $1{,}295 \times \frac{100}{76.9} = 1{,}684$ kg.

The constituents of the burden are:

	Kg.
1. Ore	1,684.0
2. Scrap	33.5
3. Limestone	348.0
4. Ash in coke	56.0
	2,121.5

The production is therefore: $\frac{1{,}000}{2{,}121.5} \times 100 = 47.2$ per cent.

Calculation of Data for Diagram

Calculation of slag. The burden per 1 kg. of iron is composed of:
1.295 kg. Fe_2O_3.
0.200 kg. moisture in ore.
0.153 kg. CO_2 in the form of $CaCO_3$.
0.030 kg. Fe in scrap.
0.046 kg. oxide from Mn, Si, P, according to analysis of pig iron (Mn assumed as Mn_3O_4).
y kg. slag

Total burden

$$\frac{y + 1.724}{\text{pig iron}} = \frac{100}{47.2} \qquad 1.724 + y = \frac{100}{47.2}$$

$y = 0.3975$ kg. of slag per 1 kg. of pig iron.

In the calculations which follow these abbreviations have been used:

C = the quantity of carbon in kilograms to produce 1 kg. of pig iron.
C_{Fe} = the quantity of carbon in kilograms contained in 1 kg. of pig iron.
C_x = quantity of carbon in kilograms consumed in the course of incidental reactions (splitting of CO_2 in shaft; reduction of Fe_2O_3 in the hearth).
$C_{Mn, Si, P}$ = the quantity of carbon in kilograms consumed in the reduction of the oxides of Mn, Si, P.

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From the total heat balance obtained in the usual way from the foregoing data, it follows that the actual loss of heat by radiation and cooling water amounts to 509 Cal. per 1 kg. of pig iron. In the calculations which follow, this total loss of heat has been assumed to be 500 Cal.

The theoretical values of the consumption of carbon and the respective temperatures of the furnace gas had been calculated assuming that the direct reduction takes place solely in the hearth, and that it amounts to 0, 10, 20, etc., per cent. and further assuming that the direct reduction of 10, 20, 30, etc., per cent. takes place solely in the shaft.

The values calculated in using the actual existing blast temperature of 594° are shown in Chart No. 1.

The values obtained in using the assumed blast temperature of 800° are shown in Chart No. 2.

The calculation of the quantity of carbon required and the temperatures of the blast-furnace gas have been made by separate heat balances, one for the hearth heat, the other for the shaft heat.

The following assumptions have been made for these heat balances:

1. That two-thirds of the loss of heat through cooling water and radiation takes place in the hearth and one-third in the shaft.

2. That the materials to be smelted in the hearth enter the hearth from the shaft preheated to 1,400°; and that the gases leaving the hearth have a temperature of 1,650°.

The hearth balance gives us the consumption of the coke carbon per 1 kg. of pig iron, and the shaft balance enables us to determine the corresponding temperatures of the furnace gas.

The separate hearth and shaft balances for the case: 0 per cent. "direct" reduction, heat loss = 0; x = coke consumption; z = furnace-gas temperature, are:

I. Hearth Balance

Heat received and developed:

1. Combustion of C to CO = $2{,}140x - 96.6$.
   The iron absorbs per 1 kg., 0.04 kg. C.
   Combustion to CO = $0.886x - 0.04$.
   $(0.886x - 0.04)\ 2{,}416 = 2{,}140x - 96.6$.
2. Heat carried by charge entering hearth at a temperature of 1,400°:

	Quantity	Spec. Heat	Temp.	
(*a*) C	$0.886x$	0.35	$1{,}400 = 434x$	
(*b*) Pig iron	1.0	0.17	1,400 =	+ 238
(*c*) Slag	0.3975	0.264	1,400	+ 147
(*d*) Oxygen of the oxides of Mn, Si, P, calculated from 0.046 kg. total quantity of oxide	0.0218	0.24	1,400	7.3

3. Blast temperature of the total carbon burned by the O_2 of the blast:
   $0.886x - 0.04 - 0.01629 = 0.886x - 0.0563$.
   $C \quad C_{Fe} \quad C_{Mn,\,Si,\,P}$

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Quantity of blast is: $(0.886x - 0.0563)\frac{16}{12} \times \frac{100}{23}$.

Factor $\frac{16}{12}$ corresponds to the commutation of carbon in oxygen.

Factor $\frac{100}{23}$ corresponds to the commutation of oxygen in air.

Blast heat: $(0.886x - 0.0563) \times 5.8 \times 0.237 \times 594 = 723x - 45.9$

The factors $\frac{16}{12} \times \frac{100}{23}$ give 5.8.

0.237 = spec. heat of the atmospheric air.

594° = blast temperature.

$= 3{,}297x + 249.8$

Heat absorbed and disbursed:

1. Heat carried by tapped pig iron; 265 = 265.0
2. Heat carried by tapped slag 0.3975×450 = 178.9
   Heat contained per kilogram of pig iron and slag = 265 and 450 Cal. according to Grüner.
3. Direct reduction of iron: according to assumption = 0
4. Direct reduction of Mn, Si, and P = 120.9
   (*a*) Mn = $0.0075 \times 1{,}990$ = 14.9
   (*b*) Si = $0.0156 \times 6{,}496$ = 101.5
   (*c*) P = $0.00075 \times 6{,}014$ = 4.5
   120.9

Heats of reaction according to Le Chatelier, Berthelot and Thomsen:

5. Heat carried by the gas leaving the hearth with a temperature of 1,650°:
   (*a*) Nitrogen: $(0.886x - 0.0563)\frac{16}{12} \times \frac{77}{23} \times 0.244 \times 1{,}650 = -101.2 + 1{,}592x$
   (*b*) CO: $(0.886x - 0.04)\frac{28}{12} \times 0.245 \times 1{,}650 = -37.8 + 836x$
   $425.8 + 2{,}428x$

Equating the heat received and developed with the heat absorbed and disbursed:

$$3{,}297x + 249.8 = 2{,}428x + 425.8.$$
$$869x = 176.0.$$
$$x = 0.2025 = \text{consumption of coke.}$$
$$C = 0.1794 = \text{carbon from coke.}$$

II. Shaft Balance

Heat received and developed:

1. Cooling of gas from 1,650° to z (furnace-gas temperature):
   (*a*) Nitrogen $(0.886x - 0.0563)\frac{16}{12} \times \frac{77}{23} \times 0.244\ (1{,}650 - Z)$
   $(0.886x - 0.0563)\ (1{,}797 - 1.089z) = 1{,}592x - 0.965xz - 101.2 + 0.0614z.$
   (*b*) Carbon monoxide.
   CO produced in the hearth.
   Cools unchanged down to 1,000° C.
   In temperatures above 1,000°, CO cannot be consumed through indirect reaction, as every molecule of CO_2,

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produced at a temperature above 1,000°, is through the action of free carbon (C), immediately reconverted into CO $(0.886x - 0.04)\frac{28}{12} \times 0.245$ $(1{,}650 - 1{,}000)$ $= 329x$ -14.9

(c) From 1,000° on CO deoxidizes Fe_2O_3 in accord with our assumption: direct reduction = 0 is here the total amount of Fe_2O_3 in the charge. 1.295 kg. Fe_2O_3 give 1.07 kg. CO_2. These give up heat in the process of cooling from 1,000 to z : $1.07 \times 0.2145\ (1{,}000 - z) =$ $+229.7 - 0.2297z$

(d) Cooling from 1,000 to z of the remaining CO. From the total amount of CO, $886x$ go to:

1. The carbon transferred to the iron = 0.040 =
2. The quantity corresponding to the CO_2, produced by indirect reduction = 0.292 / 0.332

The remainder of CO: $(0.886x - 0.332)$ goes as CO into the furnace gas.

Heat evolved: $(0.886x - 0.332)\frac{28}{12} \times 0.245$ $(10{,}008z)$ $= 506x - 0.5060xz - 189.6 + 0.1896z$

(e) Cooling of the limestone CO_2 from 1,000° to z, $0.153 \times 0.2145\ (1{,}000 - z)$.

The indirect reduction of the Fe_2O_3 through CO generates in heat $1.295 \times \frac{2{,}280}{159.7}$ = 18.5

The equation $Fe_2O_3 + 3CO = 2Fe + CO_2$ generates per gram mol.: 2.228 Cal.
159.7 = mol. weight of Fe_2O_3

Total: $2{,}427x - 1.471xz - 24.7 - 0.0015z$

Heat absorbed and disbursed:

1. The heating to a temperature of 1,400° of the burden entering the hearth:
   (a) Carbon (see hearth balance heat received, sub 2) $= 434x$
   (b) Pig iron (see hearth balance heat received, sub 2) $+238$
   (c) Slag (see hearth balance heat received, sub 2) $+147$
   (d) Oxygen contained in the ore heat $+ 7.3$
2. The preheating of the limestone CO_2 $0.153 \times 0.2145 \times 1{,}000$ $+ 32.8$
3. Decomposition of the limestone 0.348×450 $+156.5$
4. Preheating of the total quantity of the oxygen of the Fe_2O_3 to 1,000°, $0.3880 \times 0.24 \times 1{,}000$ $+ 93.2$
5. Evaporation and superheating of the ore and coke moisture:

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(*a*) Ore moisture 0.2 (607 + 0.292*z*)		+121.4	+0.0584*z*
(*b*) Coke moisture 0.02*z*(607 + 0.292*z*)	12.1*x*		+0.0058*xz*
Heat change of the superheated water vapors according to Renault.			
	446.1*x*	+796.2	+0.058*xz*+0.0584*z*

Equating the heat received and developed with the heat absorbed and disbursed:

$$2{,}427x - 1.4714xz - 0.0015z - 24.7 = 446.1x + 796.2 + 0.0584z + 0.0058xz$$
$$1{,}980.9x - 1.4772xz - 0.0599z = 820.9$$
$$x = 0.2025, \text{ substituted}$$
$$399.2 - 0.2993z - 0.0599z = 820.9$$
$$-0.3592z = 421.7$$
$$z = -1{,}173°.$$

The calculation therefore gives the blast-furnace gas the theoretical temperature of −1,173°.

It is understood that this figure has only a theoretical value with the aid of which value and the aid of a corresponding number of figures obtained by the same means, the curves or lines representing the blast-furnace gas temperatures can be entered in the chart. Only those parts of the chart can be of practical value in which the recorded temperatures of the furnace gas are about 100° and higher.

The determination of x and z fixes those points in the chart which correspond to the assumptions on which the calculations were based, *i.e.*, heat loss = 0, direct reduction = 0.

In the calculations which follow, it will be shown in shorter form, how the necessary ordinates for the other points in the chart are determined.

If the total loss of heat of the furnace through cooling water and radiations is 500 Cal. per kilogram of iron—of which 500 Cal., ⅔ = 333 Cal. belong to the hearth and ⅓ = 167 belong to the shaft—the x equation, previously computed under the assumption: heat loss = 0, will now be:

$$869x = 176 + 333 = 509.0$$
$$x = 0.586 \text{ kg. coke per kilogram of pig iron}$$
$$C = 0.519 \text{ kg. C per kilogram of pig iron.}$$

The shaft balance equation with heat loss = 500 Cal. will be:

$$1{,}980.9x - 1.4772xz - 0.0599z = 987.9$$
$$1{,}161 - 0.866z - 0.0599z = 987.9$$
$$0.9259z = 173.1$$
$$z = 187°$$

With heat loss = 1,000 Cal. the x equation is:

$$869x = 843$$
$$x = 0.970$$
$$C = 0.859$$

The z equation $1{,}980.9x - 1.4772xz - 0.0599z = 1{,}153.9$

$$1{,}921 - 1.434z - 0.0599z = 1{,}153.9$$
$$767 = 1.494z$$
$$z = 514°$$

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If the direct reduction is 10 per cent. and takes place only in the hearth, it means that 10 per cent. of the total quantity of Fe_2O_3 is reduced by solid carbon. The quantity of Fe_2O_3 is 1.295 kg., 10 per cent. = 0.1295 kg. require for their reduction of solid carbon:

$$0.1295 \times \frac{36}{159.7} = 0.0292 \text{ kg. C.}$$

The fraction $\frac{36}{159.7}$ corresponds to the ratio of the molecular weights 3C to Fe_2O_3.

The following values in the hearth balance are thereby changed:

(*a*) Under heat received and developed, subdivision 2 (heat carried by the burden into the hearth) will be augmented by the heat quantity which the oxygen of the 10 per cent. of Fe_2O_3 carries with it into the hearth. This heat quantity is 16.6 Cal.

(*b*) Under heat received and developed, the algebraic expression, corresponding to the blast heat, sub 3 will be smaller, because the oxygen of the blast burns a quantity of carbon smaller by 0.292 kg. than is burned in the case 1.

The blast heat is therefore:

$$(0.886x - 0.04 - 0.0163 - 0.0292)\, 5.8 \times 0.237 \times 594 = 723x - 69.8.$$

It must be remarked, so as to avoid mistakes, that the absolute quantity of the blast heat depends on the values of x, as determined by the final equation.

(*c*) Under heat absorbed and disbursed, the heat quantity necessary for the reduction of 10 per cent. Fe_2O_3 through C has to be substituted. The oxygen bound to 10 per cent. Fe_2O_3 is 0.0388; the needed quantity of heat is therefore $0.0388 \times 4{,}200 = 163.0$ because, for 1 kg. of oxygen, dissociated from the Fe_2O_3 through the reduction process, a heat quantity of 4,200 Cal. is needed.

(*d*) Corresponding to the variation in quantity of the blast, a variation in the quantity of nitrogen takes place, and therefore a variation of the heat taken from the hearth by the nitrogen. This heat quantity is:

$$(0.886x - 0.04 - 0.0163 - 0.0292)\, 1{,}797 = 1{,}592x - 153.6$$

Therefore, the balancing of hearth heat received and heat disbursed gives:

$$869x = 301.2$$
$$x = 0.346$$
$$C = 0.312$$

Shaft Balance

Heat received and developed:

The changes which appear in the shaft balance are somewhat greater, and they are therefore again related here in detail:

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1. Cooling of gas from 1,650 to z:

(*a*) $N_2\,(0.886x - 0.0855)\,(1{,}797 - 1.089z) = 1{,}592x - 0.965xz + 0.0932z - 153.6$	
(*b*) Cooling of CO to 1,000° remains unchanged $= 329x$	$- 14.8$
(*c*) Cooling of CO_2 produced by reduction of 90 per cent. of Fe_2O_3, $0.963 \times 0.2145\,(1{,}000 - z)$	$- 0.2065z + 206.5$
(*d*) Cooling of the remaining CO from 1,000° $- z$ from total amount of $- C$ are subtracted	
1. $C_{Fe} = 0\,040$	
2. C	
. The CO_2 obtained in the indirect reduction equals $\frac{0.2626}{0.3026}$	
The remainder passes as CO into the blast-furnace gas $(0.886x - 0.3026)\,\frac{28}{12} \times 0.245\,(1{,}000 - z)$	$= 506x - 0.5060xz + 0.1728z - 172.8$
(*e*) Cooling of the limestone CO_2 unchanged	$- 0.0328z + 32.8$
2. Reduction of 90 per cent. Fe_2O_3 by CO	$+ 16.6$
	$2{,}427x - 1.471xz + 0.0267z - 85.3$

Heat absorbed and disbursed:

For the establishment of heat balance, the only point to be considered is, that only 90 per cent. of the oxygen of the Fe_2O_3 is heated to 1,000°, and that 10 per cent. has to be heated to 1,400°. As no other change takes place, the total value of the heat disbursed varies little from that obtained at 0 per cent. direct reduction:

$$446.1x + 796.1 + 0.0584z + 0.0058xz.$$

The balancing of heat received with heat disbursed and the insertion of x gives for heat loss = 0: $z = -358$.

For heat loss = 500	For heat loss = 1,000°
$x = 0.730$	$x = 1.114$
C = 0.6465	C = 0.987
$z = 361°$	$z = 592°$

The balances are correspondingly computed for 20, 30, etc., per cent. direct reduction in the hearth.

If the direct reduction is transferred into the shaft, this means that a certain amount of the CO_2, derived from the direct reduction of Fe_2O_3, is dissociated by the carbon. Ten per cent. direct reduction means that in the indirect reduction of 1.295 kg. Fe_2O_3, 1.07 kg. CO_2 are produced, of which amount 10 per cent. = 0.107 are dissociated by the carbon according to the equation:

$$CO_2 + C = 2CO - 38.96 \text{ cal. per gram molecule.}$$

The requirement of C is 0.0292 kg., that is the same quantity, which at

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"10 per cent. direct reduction in the hearth" there directly reduced the Fe_2O_3.

Compilation of the Values Entered in the Charts 1 and 2

CHART 1

A. Direct Reductions in the Hearth Only

Direct reduction, per cent.	W.V.*	C	z
0	0	0.175	−1,173°
	500	0.519	187°
10	0	0.312	− 358°
	500	0.647	361°
20	0	0.430	52°
	500	0.770	491°
30	0	0.557	134°
	500	0.897	585°

B. Direct Reductions in the Shaft Only

Direct reduction, per cent.	W.V.	C	z
10	0	0.204	−1,355°
	500	0.550	104°
20	0	0.233	−1,620°
	500	0.574	− 20°

CHART 2

A. Direct Reductions in the Hearth Only

Direct reduction, per cent.	W.V.	C	z
0	0	0.152	−1,537°
	500	0.415	− 80°
10	0	0.258	− 662°
	500	0.521	129°
20	0	0.363	− 219°
	500	0.627	280°
30	0	0.468	58°
	500	0.732	396°

B. Direct Reductions in the Shaft Only

Direct reduction, per cent.	W.V.	C	z
10	0	0.181	−1,730°
	500	0.445	− 199°
20	0	0.210	−1,950°
	500	0.474	− 318°

*W.V. means heat loss.

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In the reduction 94.8 Cal. are required for each 0.0292 kg. C, a value to be inserted in the shaft balance under heat disbursed. In the establishment of the balances it must be considered, that the hearth receives a carbon quantity smaller by 0.0292 kg. and that in consequence, the heat developed in the combustion process of C to CO, as well as the heat carried by C from the shaft into the hearth will be correspondingly smaller. Further in the establishment of the shaft balance, under heat received, we must take into consideration that it has to be assumed first of all that the total Fe_2O_3 is reduced by the CO; and that only a corresponding amount of the so generated quantity of CO_2 is dissociated by the C. All other items of the balance remain unchanged.

In changing the heat blast to 800°, no change takes place in the balance with the exception of the heat values carried by the heated blast.

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Roll Scale as a Factor in the Bessemer Process

BY A. PATTON* AND F. N. SPELLER,† PITTSBURGH, PA.

(New York Meeting, February, 1917)

Introduction

THE use of roll scale in the Bessemer process dates back, to the best of our knowledge, at least 20 years. It was first used by the Ohio Steel Co., Youngstown, Ohio (now the Ohio Works of the Carnegie Steel Co.) under the direction of Sam McDonald, Superintendent of the Bessemer Department at these works. Two 10-ton vessels and one blowing engine capable of blowing one heat at a time were employed. The object of using the scale was to shorten the length of the blow, or in other words, to increase the production with the same equipment. Various means were tried out for introducing the roll scale into the bath of molten iron: It was shoveled into the vessel before the heat was charged, so that the metal would flow over the scale; it was shoveled into the bath after the vessel was turned up; it was dumped into the empty iron ladle by the wheelbarrow-load, and at times was dumped on top of the molten metal in the iron ladle. But the practice of introducing the scale into the iron ladle had to be abandoned on account of danger from explosions and of skulling the ladles. It was soon learned that the proper place to charge the scale was in the empty vessel, so that when the molten iron was poured into the vessel it flowed over the scale, causing a considerable reaction to take place before the heat was turned up. Eventually, cylindrical chutes similar to those now in use were installed. Into these chutes the scale is dumped and carried into the empty vessel. Before this convenient means of introducing the scale was adopted, the Ohio Works had satisfactorily demonstrated that roll scale would increase production, by blowing 107 heats in one 12-hr. turn (1,087 tons) with one blowing engine, blowing one heat at a time; whereas, prior to the use of scale, the best practice at these works was about 80 heats under the same conditions.

In using roll scale and other oxides of iron to facilitate and control the refining of pig iron in the converter, the Bessemer plant has borrowed one of the most active agencies used in the open-hearth process with, as would be expected, much the same results. The effect of such additions to the Bessemer charge may be discussed as to the influence of this practice on quality, production and cost.

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Experience has shown that the judicious use of roll scale in the Bessemer operation will not only increase production and reduce cost, but that at the same time it will improve the general quality of the steel. Most of the data on which these conclusions are based were obtained at the Bessemer plant of The National Works, National Tube Co., McKeesport, Pa.,* with the assistance of George Hitchins, Superintendent of Steel Works, and others of this company.

Influence on Quality

Taking up these phases of the subject in order of importance and with special reference to the manufacture of soft, weldable steel for pipe, it can be assumed to start with that the most important element in this problem is uniformity of operation. A higher standard of uniformity, especially in those points which affect the welding quality of the steel, is demanded in the manufacture of steel skelp than for other products, and it is probably true that this is generally obtained under modern conditions. Prominent among the factors contributing to this success is the use of roll scale in the converter.

The influence of this practice on quality is primarily due to the wider latitude in silicon which is thereby given to the blast-furnace operations, resulting in higher average silicon and lower sulphur. To obtain uniformly low-sulphur metal from the blast furnace, the Bessemer department must be designed and operated so as to be able to use without complaint iron that will run from 1 to 3 per cent. in silicon. This gives the blast-furnace management a larger margin of safety, thus making unnecessary sudden changes to prevent making iron too hot or too cold for the Bessemer plant. At the steel plant of the National Works, the average sulphur in the steel for 1915 was 0.038 per cent. with 95 per cent. under 0.050 per cent., the blast-furnace iron averaged for that year 1.77 per cent. silicon.

The proper use of roll scale, pig and steel scrap enables the blower to turn down his heats nearer to the same point in carbon by giving a sharper contrast on the final changes in the flame. This makes the loss in manganese and the residual manganese in the steel more constant. It also enables all heats to be blown more nearly in the same time and to the same temperature, which, of course, is favorable to uniformity in heating and rolling and makes all dependent operations more systematic.

The introduction of roll scale provides oxide of iron which would otherwise have to be formed by burning about 1¼ per cent. of the metallic charge. A still larger amount of oxide is reduced in the early stages of the blow. Somewhat higher silicon must therefore be present when roll scale is used to make up the thermal deficiency.

* Mr. Patton, one of the authors of this paper, was Superintendent of Steel Works at this plant for 10 yr. prior to Nov. 1, 1916.

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Production and Cost

In the design of a Bessemer plant, about all the engineer can do with regard to production is to make the vessels sufficiently large to blow the size of heat desired, with a bottom having sufficient blast area to blow the heat in a given time. He would probably use in his calculations a pig iron with 1.00 to 1.25 per cent. silicon. If the blast area was figured sufficiently large to blow iron containing 2.00 to 2.50 per cent. silicon in the same length of time, the operator would be in trouble when required to blow iron carrying in the neighborhood of 1 per cent. silicon. The vessels would slop badly, increasing the loss, and the quality of the steel would deteriorate. A vessel properly designed, with a bottom having sufficient blast area, using 20 to 25 lb. per square inch (1.4 to 1.7 atmospheres) blast pressure and designed to blow a heat of 1 per cent. silicon iron with pig scrap in 10 min., will blow a heat of 1.25 per cent. silicon iron in the same time if steel scrap is used. When the silicon rises above 1.25 per cent., roll scale and pig scrap can be used, increasing the scale as the silicon rises and so continuing to blow heats in approximately 10 min. When the iron runs above 2.00 per cent. silicon, it may be advisable to use steel scrap and roll scale, although the proportions required cannot be laid down according to rule, but must be varied by the operator, depending on mill conditions.

By this procedure, heats can be blown in 10 min. which would otherwise require 18 min.; thus the full capacity of the plant can be maintained regardless of variations in the pig iron.

The Bessemer department of the National Works has three vessels 8 ft. (2.4 m.) in diameter of 9 tons capacity; the bottoms have 18 tuyères, each having seven holes ½ in. (1.3 cm.) in diameter giving a blast area of 24.7 sq. in. (159 sq. cm.). Using 20 to 25 lb. (1.4 to 1.7 atmospheres) blast pressure, heats of metal with silicon in the charge ranging from 1.00 to 2.50 per cent. can be blown in 10 to 12 min. As most of these heats carry over 1.40 per cent. silicon, roll scale and pig metal are nearly always used for "scrapping," the roll scale occasionally running as high as 6 per cent.

Steel scrap is not used with roll scale as a rule unless it is desirable to blow the heats more quickly, and this usually becomes necessary only when the silicon in iron rises above 2.50 per cent.

The production of this plant has been increased at least 20 per cent. by the use of roll scale in this way. For example, the mill records for the month of March, 1916, show:

Ingots produced, 64,055 tons = 7,132 heats

Charge:

78.4 per cent. direct metal averaging 1.72 per cent. silicon
(only one cast was below 1.00 per cent. Si this month, the balance between 1.10 and 2.54 per cent.).

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10.0 per cent. cupola metal 1.22 per cent. silicon.

11.0 per cent. scrap consisting of { Steel 2 per cent. / Pig iron 5½ per cent. / Roll scale 3½ per cent.

Delays:

Hr.	Min.	
50	26	waiting for iron.
6	24	mechanical repairs.
0	8	electrical repairs.
7	50	converting mill delays.
Total 64	48	

Actual operating time = 583 hr. 12 min., or 14¾ min. per heat per vessel is the average time required to charge, blow and pour out each heat.

The sulphur in the steel for the above month averaged 0.039 per cen with 94 per cent. under 0.05 per cent. The time required to charge the heat, pour into ladle, dump slag, fix stopper, etc., is at least 4 min., which would make an average blowing time of less than 11 min. per heat. This month is fairly representative of current practice at this plant, as the average silicon in metal from the blast furnaces for 1915 and for the first 9 months of 1916 was 1.77 per cent. for each period. It is doubtful if this plant could make 80 per cent. of this amount of steel without the use of roll scale. The practice of using roll scale was started at this plant in December, 1906, prior to which the silicon in blast-furnace metal averaged 1.36 per cent. for 1905 and 1.37 per cent. for 1906. The period from June 15 to July 15, 1916, shows an average of 1.90 per cent. silicon in blast-furnace metal, the lowest cast being 1.03 per cent. and the highest, 2.98 per cent. In Table 3 under column heading "Time of blow" will be found some data obtained on heats blown at the same time and under the same conditions that illustrate the influence of roll scale on time of blow.

To analyze the comparative cost of the practice we have been discussing is naturally a very complicated matter. Some Bessemer superintendents claim that it takes 2 tons of roll scale to make a ton of steel. This estimate seems to us excessively high, but even if such were the case we have in this practice a direct method of producing steel far below the cost of any known process.

In order to determine the influence of roll scale by itself, we made several experiments in November, 1916, by running five heats with and without scale at the same time in this plant, all other conditions being maintained constant as nearly as possible. The results of these tests are given in Table 3. The difference in time of blow is the most striking feature. This is entirely accounted for in the shortening of the silicon blow. For example, we give the following records from heats which were carefully watched in this respect, from the last series in Table 3.

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Without Roll Scale			With Roll Scale		
Heat No.	To First Carbon Flame, Minutes	Finish of Blow, Minutes	Heat No.	To First Carbon Flame, Minutes	Finish of Blow, Minutes
3	7	14¼	3	2½	10
4	7¾	15	4	3¼	11
5	7¾	15	5	3	10½

The quicker removal of silicon is apparently due to the large excess of oxygen supplied by the roll scale, and to the oxide of iron which remains in the bath available for combination with the silica. Silica is thus removed from the bath as fast as it forms and we have a good slag from the time the vessel is turned up. The rapid reduction of the magnetic oxide is clearly indicated by the chilling effect on the heat, which is equal to about one and one-half times that produced by the same weight of pig iron.

To determine whether a larger percentage of iron was carried away in the slags under certain conditions, samples of mixer metal and slag were taken for analysis throughout the month of November, 1916, with results shown in Table 4. This indicates that these slags are fairly uniform. At least, the combined iron in the slags does not vary with the silicon in the charge, as would be expected if the variable amount of roll scale added had any material influence on the slag. As the converter slags were as a rule observed to have the same physical consistency as well as practically the same analyses under this practice, it may be assumed that the free iron or shot carried away in the converter slag is also proportional to the weight of slag produced, which varies with the silicon in the charge.

During the month of November, 1916, when the experiments referred to in Table 3 were made, we weighed all the converter slag that was shipped from the mill, and weighed or estimated the loss in the lining and bottoms of the converters. With these data and the analyses of the slags and the charge throughout the month, we have computed the loss in metal in the slag as follows:

Converter Slags

Weight of slag calculated from silica contents per charge of 22,400 lb.:

	SiO_2, Lb.
100 lb. from bottom × 80 per cent.	= 80.0
50 lb. from sides* × 97 per cent.	= 48.5
	128.5
1.74 per cent. Si in charge = 0.0174 × 22,400 × $^{60}/_{28}$	= 835.2
Total SiO_2 in charge	= 963.7

* Estimated by measuring vessel before and after 500 heats.

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Slags carry 60 per cent. SiO_2 (averaged from analyses of samples after removing shot).

Silicates in slag = $\frac{963.7}{0.60}$ = 1,606 lb.

Slag per heat, as weighed for month = 2,016 lb. (= 9 per cent. of charge).

Theoretical weight of slag from SiO_2 in silicates = 1,606 lb.

Shot and scrap = 410 lb. or 20.3 per cent. of slag.

Combined iron in silicates = 16 per cent. × 1,606 = 256 lb. = 12.7 per cent. of slag.

Total iron in slag = 33.0 per cent.

The silicon in the charge for the group of heats included in the test heats referred to in Table 3 was practically the same as the average for the entire month. The actual converter and cupola loss for this month amounts to 9.05 per cent., which is somewhat above the average, due to an unusually high percentage of shot in the slag and to the fact that the average silicon for this month was comparatively high. The converter and cupola losses for this month have been analyzed as shown in Table 1.

TABLE 1.—*Converter and Cupola Losses—National Works, Month of November*, 1916

Loss in Direct Iron	Per Cent.	Loss in Cupola Iron	Per Cent
Carbon	4.11	Carbon	3.90
Silicon	1.89	Silicon	1.66
Manganese	0.70	Manganese	0.60
	6.70	Cupola loss	1.24
			7.40

Known Losses of Other Materials

	Per Cent.
Steel scrap	0.35
Ferromanganese	30.00
Roll scale	(?)
Per cent. vessel and ladle slag	9.00
Total iron in slag	33.00*

*This is about 10 per cent. higher than the yearly average for 1915 due to stiffer vessel slag and greater amount of shot included.

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Material Charged	Per Cent. of Charge	Per Cent. Loss	Per Cent. of Total Charge Loss
Direct metal	76.58	6.70	5.13
Cupola metal	10.76	7.40	0.80
Steel scrap in vessels	4.27	0.35	0.01
Ferromanganese and ferrosilicon	0.57	30.00	0.17
Roll scale	3.93		(?)
Pig iron (for scrapping in vessel)	3.89	6.70	0.26
Iron in slag (9.0 per cent. × 33.00)			2.97
			9.34
Less credit received for iron in slag returned to blast furnaces for month			0.15
Theoretical loss			9.09
Actual loss			9.05

For comparison we give, in Table 2, the converter and cupola losses at this plant for 1915, which are more representative of the average practice.

TABLE 2.—*Theoretical and Actual Losses for* 1915—*National Works*

Loss in Direct Iron	Per Cent.	Loss in Cupola Metal	Per Cent
Carbon	4.25	Carbon	4.00
Silicon	1.76	Silicon	1.56
Manganese	0.70	Manganese	0.60
		Cupola loss	1.26
Total	6.71		7.42

Loss of Other Materials Added	Per Cent.		Per Cent
Loss in steel scrap (Mn)	0.35	Per cent. vessel and ladle slag	8.20*
Loss in ferromanganese	30.00	Iron in slag	23.40
Loss in roll scale	.....		

Material Charged	Per Cent. of Charge	Per Cent. Loss	Per Cent. of Total Loss
Direct metal	77.15	6.71	5.17
Cupola metal	11.49	7.42	0.85
Steel scrap used in vessels	3.57	0.35	0.01
Ferromanganese and ferrosilicon	0.59	30.00	0.18
Roll scale (metallic)	2.80	.....	(?)
Pig iron used for scrapping vessel	4.40	6.71	0.30
Plus iron in slag (8.2 per cent. × 23.40 per cent.)			1.92
			8.43
Less credit received for iron in slag			0.09
Theoretical loss			8.34
Actual loss			8.45

*Estimated from weight of slags for month of November, 1916.

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TABLE 3

Exper. Run No.	Analysis of Mixer Iron			Weight of Charge in Pounds					Per Cent. Si in Charge	Blast Press. Pounds per Sq. In.	Time of Blow, Min.	Analyses of Slags					
												Converter Slag			Ladle Slag		
	Total C	Si	Mn	Mixer Iron	Pig Iron	Steel	Roll Scale	Total Weight				SiO_2	Iron	Mn	SiO_2	Iron	Mn
No. 1.																	
Without Scale ...	4.34	1.96	0.84	18,640	....	3,160	....	21,800	1.80	23	19.0	65.27	15.30	8.11	53.12	15.70	14.00
With Scale	4.21	2.20	0.81	19,180	....	2,130	1,210	22,520	1.90	23	12.5	61.80	17.50	7.84	50.10	16.20	13.44
No. 2.																	
Without Scale....	3.98	1.60	0.76	19,000	3,000	....	....	22,000	1.63	25	18.6	63.60	16.20	8.11	48.06	15.60	12.84
With Scale	4.06	1.63	0.77	19,000	2,000	....	1,000	22,000	1.56	25	10.0	62.26	17.70	8.64	48.64	16.50	12.97
No. 3.																	
Without Scale....	3.92	1.62	0.70	19,100	3,500	....	....	22,600	1.62	25	18.9	65.34	16.50	8.24	50.48	13.00	11.59
With Scale.....	4.00	1.96	0.75	19,200	2,500	....	1,400	23,100	1.87	25	11.3	63.40	16.50	8.15	52.04	15.90	12.94
No. 4.																	
Without Scale....	4.24	1.79	0.64	19,020	2,540	....	....	21,560	1.80	25	15.0	63.50	14.50	6.82	47.40	18.20	18.21
With Scale.....	4.00	1.77	0.68	19,080	1,550	....	1,000	21,630	1.69	25	10.2	60.50	18.70	8.01	47.00	16.10	16.95
Averages	4.08	1.82	0.74						1.73								

NOTE.—The average weight of all converter slags produced for this month was 2,010 lb. per heat. The ladle slag averaged 417 lb. per heat and was quite constant.

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TABLE 4.—*Analysis of Converter Slag and Mixer Metal for Same Heat or Group of Heats—National Works, November, 1916*

Date	Analysis of Mixer Iron			Analysis of Vessel Slag*		
	Si	C (Total)	Mn	SiO_2	Fe (Combined)	Mn
Nov. 1–2–3	1.90	....	0.75	60.70	16.40	8.21
Nov. 4–6–7	2.00	....	0.76	61.50	18.10	8.40
Nov. 8–9	1.90	....	0.77	62.70	16.40	7.75
Nov. 10	1.63	4.06	0.77	62.26	17.70	8.64
Nov. 10	1.60	3.98	0.76	63.60	16.20	8.11
Nov. 11	1.74	4.02	0.86	.....	16.70	....
Nov. 13	1.78	4.12	0.83	.....	16.70	....
Nov. 14	1.80	4.58	0.74	.....	20.20	....
Nov. 15	1.93	3.90	0.72	.....	16.50	....
Nov. 16	1.68	4.16	0.82	.....	17.10	....
Nov. 17	2.40	4.28	0.85	.....	14.80	....
Nov. 18	2.00	4.04	0.74	.....	17.30	....
Nov. 20	1.96	4.00	0.75	63.40	16.50	8.15
Nov. 20	1.62	3.92	0.70	65.34	16.50	8.24
Nov. 21	1.46	....	....	.....	16.70	....
Nov. 22	1.92	....	....	.....	15.40	....
Nov. 23	1.96	....	....	.....	16.30	....
Nov. 24	1.42	....	....	.....	18.00	....
Average	1.82	4.10	0.77	62.78	16.85	8.21

The use of roll scale was started at the National Works in December, 1906. For that year the silicon in the charge averaged 1.29 per cent. Assuming all other conditions to be unchanged, the increase in converter loss due to raising the silicon in the charge to 1.61 per cent., as was the case in 1915, would be about 0.3 per cent., based on the slag carrying 24 per cent. iron in each case.

There is probably a little more loss due to the greater activity of the reactions in the converter when using dirty or moist roll scale, but this is recovered for the most part and is credited to the steel works. The free iron now lost in the converter slag might also be recovered to advantage so that the necessary excess loss by this practice would consist of only a little more than the combined iron in the larger volume of slag due to a higher content of silicon. As the per cent. of combined iron in the slag has been found to be practically constant, this would be lost in any case in proportion as the silicon in the charge is raised, which we maintain is warranted to a certain extent on the basis of better quality alone.

Against this loss, most of which is in consequence of the larger volume

* After removing metallic shot. The combined iron in average monthly samples of Bessemer slags for 1916 varies from 13.69 to 17.52 per cent., with an average of 15.92 per cent. for the year.

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of slag produced, we must credit a 20 per cent. reduction on steam cost and a similar reduction in most of the other items of cost, together with a 10 per cent. increase in scrap-melting capacity due to the use of higher-silicon iron. It is hardly necessary to produce further figures, even if we had them, to indicate that there is a substantial saving in cost of production by this practice. It may be said that it costs a little more to make the higher-silicon iron, but this is obviously more than offset by the advantages obtained, some of which, such as increased yield of finished product, are difficult to compute.

We have attempted to give briefly the results of 10 years' experience in the use of roll scale under the conditions that prevail at this particular plant. However, in summing up the benefits derived, as "better steel," "increased production" and "lower cost," we would not have the reader conclude that this is a "cure-all," as the practical Bessemer operator will readily appreciate that there are many other details such as temperature, ladle reactions, etc., which will require just as much attention as ever. Aside from all other factors, however, the judicious use of roll scale is a study within itself.

Discussion

E. T. McCleary, Youngstown, Ohio (written discussion).—Perhaps there is no question before the steel manufacturers of America today that causes them more worry than that of maximum production, together with good quality, and Messrs. Patton and Speller have clearly shown in their excellent paper how this may be accomplished. Any operation that tends to uniformity is certainly conducive to good practice, and as the use of roll scale in the converter eliminates to a large extent the variations in temperature due to varying silicon content in the pig iron, it should certainly give a more uniform product.

At the plant of The Youngstown Sheet & Tube Co. the use of roll scale was started in the year 1908. A great many tests have been conducted as to the results obtained by its use, and our conclusions have been practically the same as those of Messrs. Patton and Speller.

We have followed through to the finished product, heats, the temperature of which has been controlled by the use of roll scale alone, roll scale and pig-iron scrap, roll scale and steel scrap, and heats in which the temperature was controlled by use of steam blown through the molten metal, and in every case, all conditions being equal, those made with roll scale and scrap (either pig iron or steel) have given the best results.

The use of roll scale, as the authors have stated, must be a judicious one. It has been the writer's experience that the main determining factors in its use, in so far as it relates to the Bessemer process, are as follows:

(*a*) The silicon content of the pig iron to be blown.

(*b*) The combined and free carbon content of the iron.

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TABLE 1.—*Average Results from Various Tests at the Youngstown Sheet and Tube Co.*

Heat	Mixer Iron			Cupola Iron			Average Iron to Vessel				Weight of Charge in Pounds			
	Total C	Silica	Mn	Total C	Silica	Mn	Total C	Sil	Mn	Iron	Pig Iron	Steel Scrap	Roll Scale	Total Weight
No. 1 Without scale	4.06	1.96	0.65	3.60	1.10	0.45	3.93	1.70	0.59	25,500	None	4,000	.....	29,500
With scale	4.06	1.94	0.67	3.82	1.03	0.43	3.99	1.67	0.60	25,240	None	2,910	1,000	29,150
No. 2 Without scale	4.04	1.97	0.68	3.64	1.06	0.43	3.94	1.75	0.62	24,420	None	4,000	.....	28,420
With scale	4.06	2.00	0.69	3.64	1.03	0.42	3.96	1.77	0.63	25,260	None	3,000	1,000	29,260
No. 3 Without scale	4.10	1.93	0.68	3.70	1.08	0.43	4.00	1.72	0.62	24,000	None	3,985	.....	27,985
With scale	4.02	1.95	0.66	3.72	1.05	0.45	3.93	1.71	0.60	24,360	None	3,070	1,130	28,560

	Per cent. Si in Charge	Blast Press., Sq. In.	Time of Blow, Silica Blow	In Minutes, Total Blow
No 1. Without scale	1.48	23	4.70	11.00
With scale	1.48	23	2.50	9.00
No. 2 Without scale	1.52	23	4.55	11.02
With scale	1.57	23	2.50	8.80
No. 3 Without scale	1.49	23	4.62	10.55
With scale	1.50	23	2.50	8.40

	Average Analysis of Slags					
	Converter Slag			Ladle Slag		
	Silica	Iron	Mang.	Silica	Iron	Mang.
No. 1 Without scale	61.60	16.50	9.18	56.60	16.60	11.92
With scale	56.20	20.30	9.66	57.00	16.05	12.52

NOTE.—The average weight of all slags was 2,270 lb. per heat or 8 per cent. of total charge.

It is apparent from a study of the above table that the tonnage is increased approximately 20 per cent. when roll scale is used, due to the shortening of the blowing time and that the results in general agree with those shown by Messrs. Patton and Speller.

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(*c*) The manganese content of the iron.

(*d*) The initial heat of the iron to be blown.

We have found that iron with a silicon content under 1.15 per cent. does not warrant the use of roll scale and that it does not follow that the higher the silicon content the greater the amount of roll scale that may be used; for example, the writer has seen pig iron with 3 per cent. silicon with which no roll scale at all could be used. This I believe to be due to what the practical man calls "dead iron," or, from a theoretical standpoint, iron which is low in initial temperature, low in combined carbon and undoubtedly carries a large amount of occluded gases.

The use of roll scale with iron containing over 1 per cent. of manganese and the normal amount of silicon causes a sloppy blow and, quite often, steel which is not uniform. Increased silicon content of the iron tends to offset the effects of high manganese.

Table 1 shows the average results obtained from various tests made at the Youngstown Sheet and Tube Co.

H. H. CAMPBELL, Steelton, Pa. (written discussion).—About the year 1900, the Bessemer department at Steelton was short of scrap, and we tried using large, hard, compact lumps of ore in the converter. These, were thrown in immediately after blowing began and the heavy lumps, plunged down into the molten iron. It was to be expected that they would disintegrate under sudden heating, but this same ore when thrown into the bath of an open-hearth furnace did not vigorously decrepitate, so that no trouble was looked for from this source. There was no outward sign of loss at the nose of the vessel, but the deposit in the stack showed plainly that some of the ore was being projected out of the converter in particles about the size of a small pea. It is not possible to make even a guess at the loss from this cause, or to give any figure regarding the gain from the reduction of the iron in the ore, because the amounts involved were so small.

The charge of pig iron was about 22,000 lb. while the average ore addition was about 400 lb. This ore carried about 68 per cent. of iron, so that the metal in the ore represented only 1.2 per cent. of the weight of the pig iron. If one-quarter of the ore had been lost, the weight of steel produced would have been only 0.3 per cent. short, and we can not be sure of such accurate weighing in a small number of Bessemer heats. The slag from the vessel was normal, showing that all the ore that stayed in the converter was reduced; but it brings neither pleasure nor profit to put good ore in the vessel and then blow it up the stack.

When these experiments were made, the pig iron contained only a moderate proportion of silicon and a normal heat required about 1,000 lb. of steel scrap. At the same moment an alternate heat blown in another vessel would be satisfied with 400 lb. of ore. According to the authors of this paper, about 650 lb. should have been necessary to balance 1,000 lb.

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of pig iron; but there is a chance for quite an error of judgment on the part of both of us. The thermal calculation involved will be left for others to investigate.

It must never be forgotten that the scrap used in a Bessemer vessel represents a profit, for it is converted into steel without a cent of expense, except the cost of the recarburizer. Ore will furnish a little cheap metal; but 400 lb. of ore do not contain over 270 lb. of iron, and if a good part of that is blown up the stack, and if this ore addition makes it impossible to convert 1,000 lb. of scrap into steel at no cost at all, then it would seem that the argument is not all onesided. The experiments just cited were not pushed, and the writer did not give much personal attention to the matter. He merely suggested to the superintendent of the Bessemer department that ore should be tried, and it was used a few days on one converter. The experiment was not a failure, but the Bessemer superintendent reported that there was no particular advantage gained and so the story ended.

M. R. Stevenson, McKeesport, Pa. (written discussion).—I am heartily in accord with the facts that have been so ably presented in this paper and from the standpoint of the furnace operator can assure you that the high-silicon iron furnished to the Bessemer Department did not work for bad practice in the blast-furnace operation. Although it may require a very slightly larger quantity of coke to produce this higher-silicon iron as compared with the lower-silicon, yet the results obtained in all the succeeding departments, ending with the finished product at the tube and pipe mills, show a fairly large saving over the practice obtained with the use of lower-silicon iron.

Henry D. Hibbard, Plainfield, N. J.—This paper deals with an advance in Bessemer steel practice, an art which was assumed by most steel metallurgists to have reached its highest attainable development. The three features:

1. Of using iron oxide instead of a part of the far more expensive crude iron of the charge;

2. Of the greater cooling effect of that oxide as compared with scrap, thus lessening the amount of scrap needed or permitting higher silicon in the crude iron;

3. Of shortening the time of blowing so as to get a greater output; are each of them important and worthy to be considered a distinct step forward in the art.

The first feature owes its value in part to the supplying of oxide of iron for the requirements of the slag instead of causing metallic iron to be oxidized for that purpose.

The second feature, greater cooling effect, comes from the absence of heat ordinarily generated by the oxidation of some of the iron of the

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charge and to the absorption of heat in the reduction of metallic iron from its oxide. This latter takes place to a small extent through the reducing power of the silicon while that element is still plentiful in the charge.

The shortening of the blow comes from the presence in the vessel at the start of available oxygen in the roll scale which lessens the quantity of air that it is necessary to blow in to complete the oxidizing processes.

When the basic process was being worked out about 1877, Bell tried in England the effect of adding iron ore to an acid-lined converter to see if he could maintain a sufficiently basic slag to eliminate phosphorus from the iron. He added so much ore that the ganister lining of the vessel was badly scorified, and an excessive quantity of slag was made which contained so much silicic acid that no phosphorus was taken from the metal. The experiment failed and no further trials were made on that line.

The calorific effects of oxidizing the commonly determined elements in crude iron by magnetic oxide of iron or roll scale at 1,350° C., which may be taken as the temperature of the charge at the beginning of the blow, are given approximately in Table 1.

TABLE 1

Element	1 Kg., Oxidized to	Heat of Combustion, Calories	Heat Consumed, Calories	Heat Lost in Gases, Calories	Surplus, Calories	Deficit, Calories
Silicon	SiO_2	7,595	4,836	.....	2,759	
Phosphorus	P_2O_5	5,892	5,642	.....	250	
Manganese	MnO	1,653	1,612	.....	41	
Carbon	CO	2,430	5,674	1,169	.....	4,413
Carbon	CO_2	8,100	11,348	3,270	.....	6,518

One kilogram of iron burned by oxygen to Fe_3O_4 gives 1,612 Cal. The "heat consumed" column gives the heat spent in reducing enough of the Fe_3O_4 to give the quantity of oxygen required for each reaction considered.

In the Bessemer works at Steelton the cooling effect of ore in the converter was years ago found to be three times as great as that of an equal weight of scrap, which is about the proportion found by calculations. That is 100 kg. of ore absorbs about 105,000 Cal., while 100 kg. of scrap absorbs 37,000 Cal. in melting.

The table explains why the silicon is the chief heat producer of the acid process and why its heating power is decreased to little more than a third when it gets its oxygen from Fe_3O_4 instead of from air. It also shows the great heat absorption in the reaction of carbon on Fe_3O_4 by which metallic iron is reduced.

The benefit from the lessened cost per ton of ingots is, in times of great demand, like the present, overshadowed by the profit on the increased production. For a dime saved in costs a dollar is made in profits.

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C. S. ROBINSON, Youngstown, Ohio.—Mr. McCleary's paper is not conducive to much discussion, but it is a very pleasant thing to be able to so thoroughly corroborate the main points brought out in the paper of Messrs. Patton and Speller. Referring to what Mr. Stevenson has said, it seems to me that the range permitted in the analysis of pig iron is such that it enables the blast-furnace man to make more pig iron, suitable for the purpose, and also that he makes a greater amount of a good grade on a more acid slag at a somewhat less cost. He has to carry less lime and he gets into fewer difficulties in his operations, thus working to a more uniform steel due largely to the use of scale; and this is reflected in the welding properties of the skelp and the general quality of the pipe.

J. W. RICHARDS, South Bethlehem, Pa.—It is extremely interesting to hear the details of this application of the open-hearth ore reaction in the Bessemer converter. It undoubtedly functions by limiting the amount of air which is necessary to oxidize the iron, and thus limits the length of the blow, and the heat losses by radiation during the blow. There seems to be, however, a contradiction which I hope will be solved by a study of the conditions. The reaction of the scale upon the iron must undoubtedly be a reduction of the scale by the silicon in the iron. Whether the scale is reduced to ferrous-oxide or to metallic iron, its reduction by silicon is an exothermic reaction, against which you have only the heat necessary to raise the scale to the reacting temperature which is a much smaller amount of heat than is evolved. I cannot, therefore, imagine why the use of the scale should have one and a half to three times the *chilling* effect of putting in cold scrap. There is some contradiction here which needs to be cleared up by a further study. Then there is the further great saving in the heat otherwise carried out by the gases. The heat lost by the nitrogen of the air, which would ordinarily do the oxidation, is entirely suppressed and that is an enormous factor. Therefore, I think that when we get this heat balance straightened out, it will be found that the use of the scale is not in itself a chilling reaction. I think it must be an exothermic reaction, the silicon of the iron being the reducing agent.

A. PATTON.—Prof. Richards questioned the loss, or the difference between that which goes into the bath as Fe, and that which is lost as an oxide or combined iron in slag; that is, a part of scale used is not accounted for. As Mr. Speller has told you, the combined iron in slag is just about the same, regardless of whether scale is used or not. Now if we use, say, 1½ per cent. of charge (metallic) scale, that probably will take care of the combined iron in slag. If 3 per cent. is used, then there must be 1½ per cent., or half of your scale used, that is taken care of and reduced to Fe. We do not know, of course, whether the scale goes with the slag as combined iron and takes the place of what we would make to satisfy

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this slag if the scale were not used, but one will offset the other, as we find that the iron in the slag is just about the same when the scale is not used as when it is used, so in this way you can find that we recover practically all the iron in the roll scale as Fe.

J. W. RICHARDS.—In that case, I must object to the statement of the chilling effect of using the scale, because the heat reactions show that the reduction of the scale to metallic iron by silicon and the formation of the silicate slag provides considerably more heat than would be necessary to raise the scale to the reacting temperature. There is some contradiction here which needs to be straightened out by more information.

F. N. SPELLER (communication to the Secretary*).—It would certainly be interesting to know more about the heat balance in this practice, but there seems to be no doubt that the net result is a greater absorption of heat where roll scale is used instead of solid metal scrap. In the experimental heats referred to, about an equal weight of pig iron or steel scrap was replaced by roll scale. This caused a reduction of about one-third in the time of blow compared with a heat of the same metal blown under the same conditions using solid scrap but no roll scale. Notwithstanding the fact that there was considerable reduction of radiation losses when roll scale was used, the finishing temperature was observed to be practically the same in each case. This is probably due in a large measure to the fact that when roll scale is added it is not necessary to oxidize 1¼ to 1½ per cent. of iron in the charge to satisfy the slag. The shortening of the silicon blow is no doubt due not only to the reduction of a large portion of the roll scale by silicon but also to the presence of a surplus of oxide of iron in the bath available for the formation of silicates.

* Received Apr. 19, 1917.

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The Significance of Manganese in American Steel Metallurgy*

BY F. H. WILLCOX,† PITTSBURGH, PA.

(New York Meeting, February, 1917)

IN Bessemer-steel practice, air is blown through a bath of iron, or projected strongly upon its surface to burn out silicon, manganese, and carbon. Toward the end of the blow, when the iron is not protected from oxidation by these elements and the excess of air becomes great, there is oxidation of the iron. In open-hearth practice, the same difficulty is experienced with dissolved gas and oxides in the product, though not to as great or severe a degree as in the Bessemer process because the oxidizing conditions grow less severe as the end of the heat is approached, and if the bath is allowed to boil for a time it undoubtedly frees itself from a large part of the gas and oxides which have been absorbed in the earlier part of the process. Basic open-hearth steel is usually assumed to contain a larger proportion of oxides than acid open-hearth steel, but it may be said that all steel, Bessemer and open-hearth, contains more or less oxide of iron dissolved in the fluid metal at the end of the "blow" or "heat."

That certain metals are solvents for their respective oxides has been shown, in the case of copper, by Heyn, and, in the case of iron, by Law. It is indicated that the solubility of iron oxide in iron increases with the purity and temperature of the metal. This confirms what is observed and known by operating men, and these observations may be summarized as follows:

1. Cast iron is rarely subject to oxidation, as is logical from its composition.
2. Normal steel, unless deoxidizers are added, contains blow holes.
3. Steel known to be oxidized is full of blow holes.
4. The addition of deoxidizers prevents, to a greater or less degree, the formation of blow holes in normal steel.

With these observations in mind, it is logical to deduce that the cause of blow holes lies in part in the presence of dissolved oxide of iron in the molten steel and its consequent reaction with the carbon of recarburizers, when these are added, to form gases. (Another factor in the origin of blow holes is, of course, the solubility of gases themselves in the steel and

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their consequent separation when the metal solidifies in cooling.) In addition to the propensity of dissolved oxides to form blow holes in the steel, they are harmful in that they produce brittleness in the steel. Also, oxides are at present under suspicion in that they are believed to promote corrosion of the metal.

To produce a dense homogeneous steel, free from honeycombing, there is required above all the removal or suppression of gases; occluded, dissolved or mechanically held. To prevent dissolved gases, or gases formed as a reaction product from separating out at solidification, fluid compression and hot tops have been employed. But average practice is more concerned with the removal or suppression of gases before solidification. The wilder the metal (*i.e.*, the more gases evolved) the more boiling there is and, consequently, the more segregation and blow holes, so that, in extreme cases, the ingot is absolutely unfit for use. Hand in hand with the necessity for removal or solution of dissolved gases is the necessity for reducing the iron oxide dissolved in the steel, because, apart from any probable detrimental effect of the oxide as such, there is the probability of the formation of carbon monoxide or dioxide by the reaction of the iron oxide with carbon. Whether these ends are met by the direct elimination of the gases and oxides, or by their elimination as compounds of the deoxidizer used, or by increased solvent power of the steel for gases, is perhaps immaterial solely from the viewpoint of structural density of the solid product, however important it may be from the viewpoint of immunity from corrosion or gain in dynamic properties. These last two points must be considered when the matter of deoxidizer additions is under consideration, but the purpose of this paper and its length permit only consideration of the value of deoxidizers from the standpoint of removal of gases and oxides.

Materials Used as Deoxidizers

Manganese

It may be fairly said that the basis of modern steel making lies in the several functions of manganese alloys when added to a steel as carburizers or deoxidizers. These functions may be classified as follows:

A. In foundry practice:
   1. Its use as a deoxidizer and desulphurizer.
   2. Its use to alter the constitution of grain in the metal.

B. In steel-works practice:
   1. Its use as a deoxidizer.
   2. Its use to impart certain static properties to steel.

Ferromanganese is the most prominent and widely used of deoxidizers. Added in the form of ferromanganese or speigeleisen, or intermediate prod-

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ucts, manganese is readily oxidized and seizes with avidity any oxygen dissolved in the steel as oxide, this property being strong at the temperature of molten steel. For this purpose it is added in proportions varying between 0.3 or 0.4 to 1.5 per cent.; below 0.3 per cent. the steel is liable to be harmfully charged with oxides. The deoxidizing action is usually considered to lie in the greater affinity of manganese than of iron for oxygen, and the consequent seizing by the manganese of the greater part of the oxygen existing as dissolved oxide of iron. The manganese oxide thus formed is insoluble in iron and goes almost entirely into the slag, returning iron to the bath as metal. The usual additions of manganese do not entirely eliminate oxygen from the steel, and probably have a small effect upon dissolved gases. Traces of oxygen are commonly taken care of by the addition of aluminum after the ferromanganese addition, but the scavenging effect of manganese is nevertheless very strong, and it is this action, in addition to the other functions, that makes its use so indispensable. Another action of manganese, to which is due in part its value, is its behavior with sulphur. As steel exists at the end of a "blow" or "heat," the sulphur is present almost entirely as sulphide of iron. Microscopic examination shows iron sulphide to exist in steel largely as films between the metallic crystals, a condition which gives rise to weakness and brittleness of the metal when hot—"red shortness," due to the impairing of the closeness of texture and cohesion of the iron molecules. Manganese additions convert the iron sulphide to mixed sulphides. With the present low sulphur specifications and fast practice, it may be doubted whether very much manganese sulphide separates out from the metal into the slag. It probably remains in very large part in the metal, but is found to exist as sharp or rounded inclusions instead of films. A third property of manganese, which is lacking in the more common deoxidizers to be mentioned, is that it raises the critical temperature to which it is safe to heat the steel, preventing coarse crystallization at high temperatures.

Aluminum

Aluminum possesses to a high degree the ability to remove traces of oxygen from steel, very small quantities, about 0.05 per cent., "killing" the steel. In itself, it is a more efficient deoxidizer than manganese or silicon, but when considered as the sole means of deoxidizing the steel rather than as an ingredient to be used for removing the last traces of gases, there are objections to its use. First, the product of reaction, alumina, is a relatively infusible product, its melting point being higher than the usual temperature of steel, and it may therefore chill and remain suspended in the metal instead of coalescing and floating or separating out into the slag. If used in a large amount, therefore, it may give rise to

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a dirty steel and render the mechanical properties weaker on account of the presence of non-metallic impurities. Second, alumina is incapable of eliminating or changing the condition of sulphur, as does manganese. Third, the use of too much aluminum introduces a tendency to form a larger pipe. Aluminum is without question beneficial when used rationally, for several reasons: The heat of reaction is appreciable and possibly renders the steel more fluid momentarily; by "killing" the steel, the metal is quiet and crystallization of the type described as "land locking" or "pine tree" is encouraged, thus decreasing segregation; and because aluminum seems to increase the solvent power of steel for gases, its use prevents blow holes to a marked degree. The point to be carried in mind is the natural limitation of aluminum as a total substitute for ferromanganese.

Silicon

Ferrosilicon is effective for removing dissolved oxygen, even when added in proportions as small as from 0.1 to 0.2 per cent. by weight of the steel. It reduces honeycombing, is said to diminish segregation and to increase the solvent power of the metal for gases. A large pipe is formed in ingots when ferrosilicon is used in excess. Another drawback is that the products of oxidation may remain in the steel should the reaction end product be silica rather than a silicate. Analyses do not show whether the silicon is present in the metal, as a result of additions, as a silicate, silica, or a silicide. Silicon additions have no effect in eliminating red shortness caused by sulphur, or molecular derangements of the metal, to compare with the effectiveness of manganese. An experiment is recorded where ferromanganese was reduced by 50 per cent. and ferrosilicon increased in varying percentages in an endeavor to economize in the cost of manganese additions, but with the result that the metal was honeycombed and permeated with inclusions of slag Ferrosilicon finds its most effective use in the foundry rather than in the steel works.

Titanium

Ferrotitanium is given an excellent rating as a deoxidizer by some authorities, but is far from recommended by others. It is said to increase ductility, to prevent honeycombing and segregation, and to remove both dissolved oxygen and nitrogen. The removal of nitrogen is probably without question, and since the removal of all gases is necessary to obtain sound steel, there is no doubt of the worth of an element that will remove nitrogen. Titanium does not freely alloy with iron, but is more likely to be found as microscopic inclusions in the form of nitride or

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carbide When used alone, it is not as effective a deoxidizer as when used with ferromanganese, to replace appreciable quantities of the latter. With ferrotitanium, as with ferrosilicon and aluminum, the use of ferromanganese is usually essential to obtain a dense workable product.

Other Deoxidizers

In addition to the above materials, many other deoxidizers have been suggested and some have been used. Among these are magnesium, calcium sodium, vanadium, uranium, boron, and alloys of these with manganese, aluminum, silicon, and titanium. These washes have as a general base the idea that the reaction product should be a slag of great fluidity so that it will coalesce into globules of sufficient size to float out of the metal. Among these deoxidizers may be mentioned calcium-silicide, ferroaluminum-silicide, ferrocalcium-silicide, ferromanganese-aluminum-silicide, and ferrotitanium-aluminum-silicide. In most of these alloys the various elements reinforce each other and should make powerful deoxidizers, and be preëminently suitable for producing homogeneous steel. Whether they actually are suitable is not known; they are apparently playing little part in present-day practice. The extent to which thorough study of these and similar possible deoxidizers has been carried is apparently small. The importance, interest and value of experiments along these lines should be evident in view of the manganese situation in this country.

Present Scarcity of Manganese in Germany

Germany has relatively scanty resources of manganese within its borders. For that reason it may be of interest to discuss briefly the apparent situation there when the Central Empires are cut off from former sources of supply. From time to time, reports have come from Germany which seem to have great technical importance. For instance, about a year ago a Dr. Schroedter mentioned, as a metallurgical experiment and secret, means by which Germany was able to get along with less manganese than formerly in steel making. Substitute alloys have been mentioned and also a method by which metallic oxides of iron are eliminated from the slag in open-hearth practice before the heat is tapped, thereby freeing the steel itself of oxides. The facts in the case seem to be much as follows: In the year before the war almost twice the normal amount of high-grade manganese ore had been imported from India and Russia. Also, in the spring of 1915, it was reported that stocks of manganese ore had been confiscated at French and Belgian plants. During the last year, manganese has been reported scanty and prices have been high,

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though not as high as speculative dealings and scarcity have forced the price in this country. During the past year, piles of slag from old ferromanganese furnaces in Westphalia running from 5 to 14 per cent. manganese, have been drawn upon, and U. S. Consul Albert, of Brunswick, Germany, reports that the village of Adenslidt has been demolished to secure manganese ore running about 22 per cent. manganese. Lately, complaint is reported from Dutch sources in regard to the quality of German steel. It is said that the steel is daily proving worse and is becoming hard and brittle. The deterioration is attributed to lack of skilled workmen and to the lack of manganese.

Present Situation in America

Before the European war, little ferromanganese was produced in the United States except by one large company. The apparent reason was that supplies from abroad had always been abundant and had been offered at reasonable, and sometimes remarkably low prices, $40 or $45 seaboard. At the beginning of the war the price was $68 seaboard for British ferromanganese, and since the war it has touched over $400 per ton. At present the price is in the neighborhood of $165. Before the war, independent steel companies depended almost entirely upon importations and upon one large producer in America, the latter having adopted a liberal policy in sales and in tiding competitors over periods of temporary stringency. Within the last 18 months, several new producers have entered the field.

TABLE 1.—*Production and Imports of Manganese Material*

	Production			Imports	Apparent supply
	Ferromanganese, Tons	Spiegel, Tons	Total, Tons	Ferromanganese, Tons	Ferromanganese, Tons
1912	125,378	119,506	244,884	99,137	224,515
1913	119,495	126,081	245,578	128,070	247,565
1914	106,083	100,365	206,448	82,217	188,300
1915	146,542	93,282	239,824	55,201	201,743
1916	221.532	194,002	415,534	90,928	312,460

TABLE 2.—*Rate of Steel Production*

	Open Hearth	Bessemer	Other	Total
1912	20,780,000	10,327,000	142,600	31,251,300
1913	21,599,000	9,545,000	155,200	31,300,800
1914	17,174,000	6,222,000	117,400	23,513,000
1915	23,679,000	8,287,000	184,600	32,151,000
1916	31,415,000	11,059,000	298,600	42,774,000

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From Tables 1 and 2, it may be estimated that the average consumption of ferromanganese for the years 1912, 1913, and 1914 runs about 19 lb. per ton of steel. This is deduced by estimating that 30 per cent. of the Bessemer production for these years is recarburized and deoxidized by spiegel instead of ferromanganese. Applying the same estimate to 1916, we have 360,000 tons as the indicated consumption of ferromanganese for 1916.

Such an estimate is an approximation and the indication of a deficit of 50,000 tons of ferromanganese is, of course, not borne out by the market price at the present moment. Inasmuch as the method of approximation is applicable to normal years, the apparent deficit shows that unusual economies are being exercised in the use of 80 per cent. ferromanganese. Intermediate products, between 20 and 80 per cent. manganese, ferrosilicon, silicospiegel and special deoxidizers are being used liberally, so that the apparent lack of ferromanganese shown in the above analysis is compensated for by these substitutes and economies. From all indications—*i.e.*, market prices, estimates, and quality of steel—there is no danger in the present ferromanganese situation in regard to a sufficient supply of the alloy. Notwithstanding this fact, the general situation regarding the supply of manganese for the country is not without disturbing features, inasmuch as we are dependent upon British alloy and upon foreign ore for this ingredient in steel making.

The production of manganese ore in the United States for the year 1915 was 9,651 tons. This was in the face of an unprecedented demand, a shortage in the supplies of foreign ore and alloy, a price per unit of manganese that reached 97 c. (the average price per unit for the preceding 5 years was 12½ c.) and following extensive exploration work. This tonnage was scattered through 10 States and came from 34 producers, and should be contrasted with our importations of foreign ores.

TABLE 3.—*Imports of Manganese Ores*

	Tons
1914	288,706
1915	206,859
1916	492,850 (apparent rate)

The bulk of this foreign ore in the present year is coming from Brazil. In 1913, one-third of the ore came from Brazil whereas this year nine-tenths is coming from that country.

It is evident that we do not produce this ore in quantities commensurate with our consumption. In fact, our production is negligible; how negligible, may be realized from the fact that based upon the amount of ore necessary to furnish the apparent supply of 310,000 tons of ferromanganese available this year, our production of ore amounts to 1½ per cent. Our contribution is equivalent to about 2 per cent. of the ore required to furnish the apparent production of 220,000 tons of ferromanganese in

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this country. This is on the basis of last year's production. This may be increased this year, but it is not possible that it can be increased to a degree commensurate with our requirements. No deposits of rich manganese ore that hold any promise of furnishing the needed quantities of this material have been developed to date in this country. The situation can not be better expressed than in the words of an editorial in a recent number of an iron trade journal: "Insecurity lurks behind these facts (apparent supply of manganese). To supply this most necessary element in making steel, our own territory has furnished scarcely any ore. The iron industry is dependent absolutely upon foreign supplies and sources for manganese, without which thus far good steel is impossible. Were war to break out between us and a first-class power capable of controlling in part or entirely the high sea, our situation would be precarious, for no nation can defend itself without steel. Assiduous efforts to discover manganese ore in this country have been unavailing. We are dangerously dependent upon manganese. It is to be hoped that metallurgical science will discover a substitute."

This situation is not one that carries immediate danger. It is certainly comparable, however, to the potash and nitrate situation and the suggested production of these materials in this country. It may well be more serious than either of these two examples. In a national emergency we could economize and to some degree dispense with the one and manufacture the other, but we can not entirely dispense with nor manufacture manganese, nor can we economize in its use to the degree called for by our inadequate production and great demands.

Conclusion

There are two ways to meet such a situation as has been presented in the above paper. The first suggestion is an obvious one, and is that the manufacturers of the country accumulate a sufficient reserve of high-grade manganese ore to tide the steel industry over at least a year's stoppage in supply. The amount of capital that would be tied up by the investment in such a tonnage of ores at the present prices and scarcity of freight-carrying bottoms is the answer to this proposal at the present time, and may be a deterring factor for all time to come, because when the present war is over and peace established, the present very apparent need of some such precaution will naturally become less and less evident. The second suggestion is that an attempt be made to develop a substitute alloy which can be used in place of, or, more probably, with ferromanganese in such proportions as to conserve the manganese.

As far as can be learned, substitute deoxidizers are not playing a part commensurate with that which they would be called upon to do in case of emergency, nor are our resources of manganese in such shape, nor our

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means of utilizing them sufficiently developed, to enable us to count upon them in case of need. No thorough and independent study has been made of all alloy combinations as regards their efficiency in deoxidizing steel and rendering it homogeneous and workable, either as a whole or part substitute for manganese, or if this has been done, it is not generally known and available.*

Before undertaking work of this nature—which would involve preparation of many alloy combinations, coöperation with steel and foundry men, and chemical and metallographical examination of the steels treated with these alloys for inclusions of gas, oxides, and slag—it has seemed best to submit the situation and the need of investigations on substitute alloys, in order that an opinion may be expressed as to whether the matter is of sufficient importance and promise to warrant serious consideration.

Acknowledgments

In the preparation of this paper, reference has been made to Howe, Hall, Campbell, and Stoughton. The current numbers of the *Iron Trade Review* and *The Iron Age* have been consulted. Advice has been given by operating steel men, and also by Professors Crabtree and Macintosh of the Carnegie Institute of Technology, the latter of whom has carried on work in the preparation of ferromanganese-silicon alloys at the laboratories of the Carnegie Institute of Technology.

Discussion

The Chairman (Henry D. Hibbard, Plainfield, N. J.).—This paper is timely because of the changed conditions due to the great war, but apparently its scope is limited to oxidation process steels. Referring to the four numbered observations on page 412:

No. 1 seems to ignore Johnson's discoveries about oxygen in cast iron.

No. 2 ignores crucible steel.

No. 3 ignores red short steel containing enough gas solvents to make it solid, while

No. 4 assumes that a deoxidizer is necessarily a gas decomposer or solvent and *vice versa*, which it not always is.

From these observations it is deduced that the only hole-forming gas in steel is an oxide of carbon (CO presumably is meant), which is not true.

* H. M. Boylston's "Investigation of the Relative Merits of Various Agents for the Deoxidation of steel," *Carnegie Scholarship Memoirs, Iron and Steel Institute*, vol. 7, p. 102 (1916) became available and known to the writer after this paper was submitted.

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The assumption on page 413 that the more boiling of steel (presumably in the molds) the more segregation, is hardly warranted, as it would seem that agitation should tend to check segregation rather than promote it.

It is true that marked segregation exists in a large proportion of ingots containing gas holes, if not in all, but the steel may have been so dead that the top froze over quickly without any escape of gas bubbles or boiling after teeming was finished.

Referring to the paragraph on manganese, page 413, its use in steel making by an oxidation process may be considered and amended as follows:

1. Its use as a deoxidizer.

2. Its use to give the composition desired either for the purpose of meeting specifications or to give certain physical properties.

3. To prevent gas holes in some degree.

Use No. 1 requires time because the oxygen or oxides are so dilute, and because the manganiferous products formed, oxides and silicates (sonims), require time to precipitate, separate, agglomerate and float up to merge with the slag.

Nos. 2 and 3 require only time enough for an intimate mixture of the manganese alloy.

The fundamental reason why manganese is not wholly replaceable in making steels of the class under consideration seems to lie in the fact that its oxide forms with silica and iron oxide very fusible silicates which wet each other when they touch and so collect together, and when large enough float to the surface.

Other elements such as silicon, aluminum, calcium, and magnesium, have stronger affinity for oxygen than manganese has, but their oxides are insoluble and infusible in the molten steel and when these elements are used for deoxidizing without manganese the steel is likely to be red-short because of the widespread diffusion of the sonims which cannot, or do not, escape.

As for aluminum, the proportion the author calls very small, about 0.05 per cent., would better be called very large, amounting as it does to about 18 oz. per ton. Three ounces to the ton is common in moderately well-made open-hearth steel and 6 oz. with about 0.20 per cent. of silicon will kill any ordinary ingot steel. If it does not the furnace practice is not good. Half an ounce per ton will often determine whether or not a well-made steel will "rise" or "stand" in the ingot molds.

The present scarcity of manganese and manganese ore in Germany has, as the author states, been met by the utilization of blast-furnace slag accumulations of the past. These slags are smelted in electric furnaces, the alloy produced at one plant analyzing about 60 per cent. Mn, 20 per cent. Si and 2 per cent. C. The phosphorus, of course, is

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very low, as all that originally in the ore went into the metal on the first smelting.

Economy of manganese is now demanded. In the first place, let it be realized that freedom from redshortness is due not to the presence of manganese but to the absence of impurities, both soluble and insoluble, which cause it. Steel made from wrought iron and charcoal or blister bar, would seldom, if ever, have any manganese, yet such steel was regularly made for half a century before Heath used manganese ore in the crucible.

Acid open-hearth steel may have much lower manganese than is ever specified and be free from redshortness.

In the first practice I personally knew of following Martin's first plan, a bath of pig and scrap was melted and then diluted with charcoal direct-process blooms made in the Catalan Forge and in 3 years, during which I knew about it, no steel showed any redshort tendency. We heard of redshort steel elsewhere but otherwise knew nothing about it. Our soft steel for boiler plate contained from 0.2 to 0.3 per cent. of manganese. The slags were extremely viscous, apparently without any uncombined iron oxide and the continued cleansing effect with such a slag, due to time, with some manganese in the bath, and the stirring caused by working the blooms (which were preheated) in the bath practically eliminated the oxides of every sort which, if present, would have tended to make the steel redshort.

The content of manganese is still incorporated in some specifications for structural steels, but it is doubtful if those who do it can give any proper reason therefor. They apparently do it because their predecessors did. Much good steel has been rejected because the manganese was too low, when a little more haste in casting would have kept it within the specification by retaining a part of the manganese which was, as it ought to have been, consumed in reducing iron oxide, making the steel so much the better.

When steel to be tempered requires low manganese, no objection is found against so specifying, but superior or varied furnace practice by which steels may be made free from redshortness and low in manganese is barred or manganese is wasted.

I may say also that we have read of a manganese substitute in Germany which I understand to be carbide of calcium. They cut the manganese down to about half of what was previously used, and by the use of carbide of calcium and that amount of ferro-manganese, made of blast-furnace slag, they are able to make fairly good steels, but the control is not as good as it used to be when the supply of manganese was not cut short.

J. S. UNGER, Pittsburgh, Pa.—When we speak of a metal, alloy or com-

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pound as a deoxidizer in steel manufacture, the term is a relative one only, as oxidation or deoxidation is largely a question of temperature.

Tin and lead resist oxidation at ordinary temperatures much better than iron, but when the first two are raised to their melting points they are quickly covered by their oxides, while iron is but slightly affected at the same temperature.

Metallic silicon changes very little in the air; at a dull red it oxidizes fairly rapidly to silica, but at a bright red the oxidation is quite slow.

Steel is melted and to a large extent oxidized in a stream of oxygen after the reaction has been started, while 80 per cent. ferro-manganese burns very slowly under the same conditions, even though the manganese is supposed to have a much stronger affinity for oxygen.

A bar of copper and steel may be bolted together, and the steel completely burned away from the copper by a stream of oxygen with very little injury to the copper.

Oxygen is found in practically all steels. If it exists as an oxide or as a gas combined with carbon, it is very difficult to understand how molten steel or a piece of steel heated to a bright red heat in a vacuum will show some free oxygen in the gases evolved.

Cast iron with a high percentage of carbon, silicon and manganese, all powerful deoxidizers, contains oxygen. In fact certain irons are improved by blowing a little air through the molten iron.

The determination of oxygen in steel is a very difficult operation. There is a grave doubt whether any particular method can be considered an exact quantitative one, but enough work has been done to show that oxygen may exist as oxides, as gases, carbon monoxide, carbon dioxide and as free oxygen.

Any investigation of the effects of what are usually called deoxidizers is likely to lead to confusion. It is impossible to say when a deoxidizing agent ceases to act as a deoxidizer and begins to confer certain physical properties on a steel.

Manganese may be beneficial in conferring hot or cold working properties, and the quantity is usually specified, but steels are made in large quantities every day which do not contain manganese. Again, manganese affects the qualities when added above the usual amount. A well known example is manganese steel.

Silicon, another well known deoxidizer, is found in nearly all steels. When added in fairly large proportions, as in silicon tool steel, silico-manganese steel, or in certain steels of special magnetic properties, it completely changes the character of the steel as the manganese did in the preceding example.

Such examples can be multiplied. All elements which can be alloyed with steel confer some property.

Very few steels are made by the addition of manganese alone. The

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three common deoxidizers, manganese, silicon and aluminum, are nearly always used in ordinary steel making. Sometimes but two are used, very rarely one alone. It would appear that each is necessary and has its function when applied at the proper time and place.

Experience has shown that manganese and silicon are best added to the bath or ladle, and aluminum to the mold. It is of interest to know that in some experiments made by using an alloy of these three metals and iron, the effect was to make a better product than when the constituents were added alone at different stages of the heat.

Many elements have been tried as substitutes for the three mentioned. Magnesium has been proposed as a substitute for aluminum, as it has a stronger affinity for oxygen. A simple calculation will show that 1 lb. of magnesium will take up 0.66 lb. of oxygen, while 1 lb. of aluminum will take up 0.89 lb. of oxygen.

When the magnesium in sticks ¾ in. square was added to the mold, it resulted in an explosion, blowing part of the steel out of the mold, making it a dangerous operation. These disadvantages, together with an extra cost of 100 per cent. over aluminum (before the war prices), condemned its use.

Calcium, sodium, titanium, vanadium and boron alloys have been tried in various combinations. My experience with those I have tried is as follows:

1. Their cost is too high to permit of ordinary commercial use.
2. When used alone, the steel is not of good quality.
3. To show favorable results they must be assisted by one or more of the common deoxidizers.

Slight honey-combing or sponginess, which must not be confused with piping, is rarely injurious, as these small cavities if unoxidized, unite into a common mass if the steel is rolled or forged. Their presence can not be detected by physical tests, nor does the finished material give poorer service when put into actual use.

A very minute quantity of gas may produce a cavity ¼ in. in diameter. In an ingot containing about 0.001 per cent. of oxygen, existing as carbon monoxide, the gas will at the freezing temperature of the steel occupy a volume equal to the volume of the ingot.

On page 170 of the excellent work by H. M. Boylston, referred to by Mr. Willcox at the close of his paper, is a table in which the relative value of the deoxidizers used is ranked. By adding the ranking of each deoxidizer and making a comparison, the results are as 3 is to 4 in the extreme cases. While I do not agree in using the mechanical tests as a measure of the quality of the deoxidizer, owing to the effect such material has on the steel itself, the results show very little difference.

To my mind, a good deoxidizer must have these qualities:

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1. For the work it will do, it must be cheaper than any other deoxidizer.
2. It must alloy with iron in any proportion.
3. Adding a small excess should not influence the quality of the steel; or perhaps I can make it clearer by saying that an overdose does no particular harm.
4. It must confer better hot working qualities.
5. The resulting product of the reaction must be easily fusible and of much less weight than the steel, in order that a prompt separation may take place.

Piping, segregation, soundness, specific gravity, etc., are secondary qualities, and can be almost entirely removed by cropping. No single one of the common deoxidizers possesses all the qualities specified above, but a careful use of them at the proper time is not alone cheaper, but has given better results than any materials tried thus far.

J. W. RICHARDS, South Bethlehem, Pa.—The author's remarks on the use of deoxidizers in steel are brief, and in many cases faulty, perhaps due to their too great brevity. I wish particularly to call attention to the remarks on the use of aluminium in steel, where it is said that the heat of reaction is appreciable and possibly renders the steel more fluid momentarily. In actual practice the amount of aluminium used in steel is not sufficient, by its oxidation, to raise the temperature of the steel 1°, so that the increased fluidity is due to other causes than the increase of temperature; it is due to the removal of the ferrous oxide, which reduces the fluidity. He also says that aluminium seems to increase the solvent power of the steel for gases, and thus its use prevents blowholes to a marked degree. I believe that its use in preventing blowholes is due to an entirely different principle than that of increasing the solubility of the gases in the steel. At least nine-tenths or more of its action is due to removing the oxygen of the dissolved ferrous oxide.

Coming to the significance of manganese and our supply of it, there is a remark on page 417 which may lead us in this country to think of how our supply of manganese for ferromanganese can be increased. It is the statement that during the past year piles of slag from an old ferromanganese furnace, running 5 or 10 per cent. manganese, have been drawn upon and reduced for making ferromanganese. The slags from ferromanganese, while high in manganese, are very low in iron, and therefore they are a sort of concentrate produced by smelting which gives a material from which you can get a higher per cent. of ferromanganese than from the original ore from which the slags were made. In this country we have very few supplies of rich manganese ores, and if our supply were cut off during a war, we would be at a great loss for material from which to make directly high-grade ferromanganese. However, it seems to me much more logical and quite possible to take the leaner manganese ores which contain considerable iron, and to give them a double treatment, first extracting almost all the iron with part of the manganese, leaving a rich manganiferous slag, making perhaps very little

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attempt to get much of the manganese out, in fact, taking as little as possible. Then re-treat the slags so as to get from them a much higher percentage of ferromanganese in the product than would otherwise be obtained. There are immense deposits of manganese ores, such as the Cuyuna Range in the Middle West, which could be utilized for that purpose and which would supply all the manganese for high ferromanganese that the country would need. It is even possible that they could be thus utilized at other times than in case of war and the suspension of our imports of manganese ores.

ALBERT SAUVEUR, Cambridge, Mass.—Dr. Unger made the statement that blowing air through cast iron improves its quality; I think that some of us would like to know whether this is Dr. Unger's expression of his own opinion or merely the expression of the opinion of others?

J. S. UNGER.—That is simply a statement of another man's opinion.

JOSEPH W. RICHARDS.—I have read carefully Mr. Johnson's paper on that subject, and I do not agree with him on the general proposition; but I think it is quite possible that a gentle blowing of air through melted iron may improve the quality of the iron, not by leaving oxygen in the iron but by removing some of the deleterious impurities by differential oxidation.

A. E. OUTERBRIDGE, JR., Philadelphia, Pa.—In the last month there has come to me a keg of ore from a very minute island in the Caribbean Sea that is occupied by a planter with a few negroes who cultivate the land upon the shore, and there is a great mountain that rises up like a peak. There have been rumors for quite a number of years, that this mountain was a solid mountain of manganese. The planter sent a keg of the ore to New York, to the shippers; and they asked me if I would look after it. The ore was shipped directly to Booth, Garrett & Blair, very old chemists in Philadelphia. As I remember, it is an ore, not the pure pyrolusite or kidney manganese ore at all, but contains 30 per cent. of manganese, only 2 per cent. of iron and, I think, about 20 per cent. of silica. There is no evidence whatever that that ore was selected with any intelligent supervision. I have done nothing with it and I am not in position to do anything further than to send it back, which I have done, to the shippers in New York. If this matter interests anybody, I will be glad to give them the benefit of what information I have about it.

LEONARD WALDO, New York, N. Y.—Many years ago, when I was engineer in charge of the Cowles Electric Smelting & Aluminum Co. at Lockport, at the beginning of the aluminum industry, the inertia of the market had to be overcome in the introduction of aluminum. We searched assiduously in every direction and finally found an outlet for aluminum in the manufacture of steel. A pamphlet was published at

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the time, but we found that if we attempted to introduce aluminum into steel, that the logical way and the only way was through the use of an alloy of aluminum with iron. A standard alloy was made, and the first introduction of aluminum into steel was made in that way. Later, the use of pure aluminum itself has taken place. I speak of that now because, at a hint from Baron Von Jüptner, I suggested, some years ago, the use of magnesium as a steel deoxidizer and its attempted use was followed by precisely the results which Mr. Unger has described, the trouble with magnesium being that under open air conditions it burns before it will vaporize. The rapid production of its own vapor causes an explosion, and therefore it becomes dangerous to use in the ordinary way, but there are certain theoretical advantages in the use of magnesium which necessitate a continued study of that subject which is now being made, and I very much hope that we will be able to add magnesium to steel in such a way that the full effect of its light specific gravity and its qualities as a deoxidizer can be brought fully into play. I say that now because magnesium is just commercially coming into the world, and various forces have contributed to make it an important material at this time. Several companies in the United States have started its manufacture. The deposits of magnesite are being carefully examined and chemical by-products from which metallic magnesium can be made have been carefully searched for. To show you what progress has been made, I would like to give an analysis of present commercial magnesium which is sure to find its use in deoxidizing. This is a sample from a lot of 22,000 lb. of magnesium.

	Per Cent.
Magnesium	99.867
Silicon	0.032
Iron	0.037
Aluminum	Trace
Copper	0.028
Cadmium	None
Bismuth	None
Lead	None
Nickel	None
Cobalt	None
Zinc	0.036
Specific gravity	1.74

It appears, therefore, that magnesium has arrived at the first stage; it is a commercial product, it is not too high in price, and, with our better knowledge of its use, it is sure to be a deoxidizer of value, not only in the ferrous metals, but in the non-ferrous metals. I might say that some experiments which were made without much thought, not by me but by a colleague in steel making, in the attempt to introduce it into crucible steel, were very disastrous. Men were hurt, but the reasons for the ex-

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plosion were well known, afterward at least, and I do not apprehend that the difficulty is a serious one of getting the magnesium into the molten steel. The other uses of magnesium in aviation and the problem of the military engineer are so great that whatever encouragement we can give it from the iron and steel side should be given.

RICHARD MOLDENKE, Watchung, N. J.—I would like to add that I also have made tests with metallic magnesium added to molten cast iron. This on the advice of Baron Jueptner von Jornstorff, then in Buda-Pesth, in 1900. I had the same experience that Dr. Unger had, when I introduced a piece of rod magnesium inserted in the end of a wooden bot-stick. The metal blew up, but enough was saved to note a wonderful fluidity in what was almost pasty and badly oxidized material before. Moreover, when cast into chills, the metal exhibited a clearness of crystallization that was remarkable.

In sending out the material for analysis, to see whether magnesium had remained—as I wished to make a ferro-magnesium for subsequent use in de-oxidation experiments—it was unfortunately lost. The price of magnesium then went up and I never took the subject up again, my belief being anyhow that it is far better to keep oxygen out of cast iron than to remove it after poor melting practice has introduced it.

J. W. RICHARDS.—That simply moves the problem one step further; how are you going to make the iron-magnesium alloy? But I think that problem will be solved, and when it is solved, it is quite likely that magnesium will come in as a fourth member to supplement manganese, silicon and aluminium in the final deoxidation of steel.

I wish to confirm Mr. Hewett's statement* about the manganese deposits of Costa Rica. A former student of Lehigh University, Mr. Yglesias, is president of a company that is mining the manganese ores, and I have had some correspondence with him. He has a high-grade ore which can be delivered in shipload lots on the Atlantic Seaboard. The problem of getting our manganese ore from abroad, in ships, would be one for peace, but in time of war we would have to fall back on our own supplies of manganese, which would bring up the importance of the other low-grade deposits in this country.

D. F. HEWETT, Washington, D. C. (communication to the Secretary†).—I am not prepared to discuss the metallurgical use of manganese in the form of alloys. In connection with other work for the U. S. Geological Survey, however, I have been investigating incidentally, for about 5 years, the American as well as other sources of manganese.

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*This is in answer to oral discussion that was revised and is now printed as communication to the Secretary.

† Received April 3, 1917. Published with the permission of the Director of the U. S. Geological Survey.

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There are some conclusions that come out of this investigation that may interest those whose greatest interest lies in the use of the alloys.

From a geological point of view, most manganese ore deposits may be regarded as concentrated masses of manganese oxides, as there are few masses of the other manganese minerals that are rich enough in manganese and low enough in silica to warrant smelting. The deposits of the Huelva district, in Spain, which are largely manganese carbonate and silicate, are exceptions to this statement. Some deposits of manganese oxides, such as those of Russia and Cuba, are bedded with ancient sediments, and although they are clearly concentrated under unique conditions, the source of the manganese cannot be determined. Probably 60 per cent. of the world's production of manganese ore, or 95 per cent., if that of Russia be excluded, is derived from the superficial parts of deposits that have undergone weathering and enrichment near the surface. In other words, the situation with the manganese ore supply is such as it would be with regard to copper, if instead of being derived largely from deep zones of deposits of deep-seated origin, such as veins, it were derived solely from the zone of secondary enrichment.

Manganese oxide occurs in all rocks but is most abundant in igneous rocks, of which, according to Clarke, it forms an average of 0.10 per cent. It also forms 0.05 per cent. of the average limestone, but only traces are present in shales and sandstones. It is interesting that although many commercially important manganese deposits occur in rocks that are but slightly richer in manganese than the average, the most important deposits occur in rocks of unusual character, that are much richer in manganese than the average. Thus, the most important deposits of India and Brazil overlie or occur near masses of rocks that contain excessive amounts of manganese garnet and pyroxene.

The chemical processes which aid in the formation of large masses of manganese oxide near the surface are obscure and probably complex. From available knowledge, it may be stated that many manganese deposits occur in regions where rock decay is most complete and persists deeper than usual. Relatively few deposits are known in regions with cold or arid climates, which favor rock disintegration rather than decay.

Even if allowance be made for the possible discovery of deposits not now known, it is apparent to any person who has given a little thought to the subject, that the United States is woefully deficient in large manganese deposits. This may be based upon two conditions. Either, on the one hand, there are few if any large masses of manganese-bearing silicates that could yield by weathering important quantities of manganese oxides, or, on the other hand, the climate and types of weathering now prevailing in the United States are not such as are capable of producing large accumulations of manganese oxides from the sedimentary rocks which so largely underlie the United States.

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I realize that generalized prophesies not based upon the thorough consideration of many data are little more than guesses, and where applied to ore deposits, are hazardous, but I think we are warranted in hoping that, within the next decade perhaps, there will be developed in Central and the northern part of South America, where the climatic conditions favor deep rock decay, deposits of manganese oxide capable of contributing largely to our needs. Thus far, only a few manganese deposits which are clearly formed by processes of rock decay are known in this region, but recently one was discovered in Costa Rica that appears to offer prospect for large production. With the shortage of manganese ores and the consequent high prices, the search for deposits becomes more active. It seems to me that those interested in the manganese ore supply would do well to investigate any statements which indicate that new deposits have been found in Central America or northern South America.

EDMUND NEWTON, Minneapolis, Minn. (communication to the Secretary*).—In discussing the question of the deoxidation of steel and material suitable for performing this function at the end of the heat, it would seem logical also to include the question of prevention of oxidation in the steel or the formation of iron oxide, FeO. I refer to the use of manganese in the basic open-hearth and basic Bessemer processes.

The advantages of high manganese in pig iron for these processes have evidently been clearly understood in Germany for a number of years, although little has been written in regard to the exact manner in which the benefits are obtained. In this country, while we make no steel by the basic Bessemer process, our basic open-hearth process is becoming more and more important. Until 10 years ago, in this country, manganese in the pig iron for this process was considered of little value and by many writers it was spoken of as a disadvantage. During the past 10 years, however, in certain parts of the United States, higher contents of manganese in basic pig have been used with considerable success, although there is apparently nothing in our technical literature to show the reasons for this trend. A German book, "The Basic Open-Hearth Steel Process" by Carl Dichmann does, however, offer an explanation.

Dichmann says that if the charge for either of the above-mentioned processes contains 3 per cent. of manganese or above, satisfactory steel can be made without further addition of ferromanganese or other deoxidizers at the end of the heat. The steel is not red-short and works well. He says that manganese tends to prevent the formation of iron oxide, the manganese of the metal being oxidized by free FeO in the slag or the bath, and the oxide of manganese combines with silica and lime to

* Received, May 1, 1917.

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form slag. This goes on until equilibrium is established. The presence of considerable manganese in the metal at the end of the heat indicates that little or no FeO is present in the steel. It then only remains to add sufficient carbon to bring the steel to the desired content.

Dichmann's conclusions possibly suggest how the present shortage of high-grade manganese ores and ferromanganese in Germany is being met. There is considerable manganiferous iron ore available in Germany which can be used to increase the manganese content of the pig iron for these processes. In the United States we have no ore suitable for making basic Bessemer steel, but our basic open-hearth process is rapidly increasing. We produce a very small quantity of high-grade manganese ore, although there are available in the Cuyuna Range in Minnesota and in other districts considerable tonnages of manganiferous iron ore, which could be used for the same purpose as ferromanganese made from high-grade manganese ore.

Mr. Willcox raises the question of the Bureau of Mines undertaking a general investigation of substitute deoxidizers for manganese, and I would suggest that they study the various phases of the above-mentioned process. There is the possibility of using this method to a great extent in time of national extremity and also the possibility of its competing successfully with high-grade ferromanganese in ordinary times. It might then be possible to evaluate the manganiferous iron ores against the saving in high-grade ferromanganese. It would, however, seem that such an investigation would be of considerable value and interest to the steel industry at the present time.

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Temperature Measurements in Bessemer and Open-Hearth Practice*

BY GEORGE K. BURGESS,† SC. D., WASHINGTON, D. C.

(New York Meeting, February, 1917)

I. INTRODUCTION

THE suggestion has often been made that it would be highly desirable, at least for certain grades of steel, to be able to control more certainly, by pyrometric measurement or otherwise, the temperatures of the operation of the open-hearth and other furnaces used in the manufacture of steel and iron.

That the properties of the ingot and finally of the finished steel product are intimately related to the final temperatures attained by the metal in the furnace and to the temperature of casting, has been recognized by metallurgists for a long time, and this question has been very thoroughly treated, especially by Prof. Henry M. Howe, and by A. W. and H. Brearley.[1]

It is the object of this paper to demonstrate that in so far as casting temperatures of furnaces, steel ingots, and similar operations involving the temperatures of streams of iron and steel are concerned, well-known pyrometric methods may be easily applied—if certain recently determined corrections are made and precautions taken—with a relatively high degree of accuracy. Greater but not insurmountable difficulties will be encountered in the case of open-hearth furnace temperatures, while for those of the converter type a ready solution does not seem practicable.

It appears that certain essential data have always been lacking in reporting observations such as those of Le Chatelier in 1892. Thus the correction to be applied for the characteristic radiation—or emissivity—has hitherto not been adequately taken into account nor the limitations of the several possible pyrometric methods clearly recognized.

In view of the fact that in open-hearth practice the temperature to be measured in the furnace itself, in the stream of metal and slag and in the teeming into molds or ingots is usually above 1,500° C. (2,700° F.)

* Abstract prepared by the Editorial Department from the manuscript of a Technologic Paper to be published by the U. S. Bureau of Standards.

† Chief, Division of Metallurgy, U. S. Bureau of Standards.

[1] A. W. and H. Brearley: Some Properties of Ingots, *Iron and Steel Institute* of Great Britain, vol. 94, p. 137 (1916).

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and may reach to above 1,750° C. (3,200° F.), it is manifest that we are limited to the optical and radiation types of pyrometer. Preference will be given to the former for the reason that the errors in the use of the radiation pyrometer caused by intervening gases, distance, size and specific radiation (emissivity) of objects sighted upon are greater and more uncertain than with the several forms of optical pyrometer[2] using monochromatic light.

II. Furnace Temperatures

Although one may arrange to observe temperatures of any portion of the open-hearth furnace, and also of the slag surface, what one is mainly interested in is the temperature of the metal, which bears but a remote relation to that of the roof and walls on which the heat of the fuel impinges.

Pyrometric control of temperatures of the roof is easy and may be exact and, although somewhat misleading, may also be made without serious interference from gases and smoke by sighting through suitably placed ports. With a knowledge of the melting range of the roof and arches, the wasting away of the bricks by overheating may be retarded by pyrometric control. Observation of temperatures within the furnace are simpler than of streams of metal or slag for no correction of any importance has to be applied to the optical pyrometer readings for selective radiation (see Sec. VI).

The temperature conditions in the bath of the open-hearth furnace may perhaps be most easily determined by sighting an optical pyrometer through a peep hole in a door or through a port upon the surface of slag, preferably near the center of the bath. This temperature is not usually that of the metal but does not, in general, appear to be far removed from the metal temperature especially as the time of tapping the furnace approaches and the fuel supply has been fairly regular and uniform.

The temperature of the metal itself may be determined—but not as readily and surely as one could wish—by removing with a preheated spoon some 50 lb. and sighting upon the surface of the metal contained in the spoon with an optical pyrometer, taking a series of temperature-time readings and noting exactly the instant of withdrawal of metal and of temperature readings, from which data the temperature of the metal in the furnace may be estimated. Here complications arise, such as the slag residue and the formation of oxides on the surface, their solidification sometimes before the steel, and other surface effects dependent upon the condition of the steel, escape of gases, etc., with attendant uncertainties

[2] Burgess and Foote: Characteristics of Radiation Pyrometers, *Scientific Paper* No. 250, *U. S. Bureau of Standards.* Waidner and Burgess: Optical Pyrometry, *Scientific Paper* No. 11, *U. S. Bureau of Standards.*

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in temperature readings, the speed required in taking readings and other difficulties of rapid manipulation and operations, for which at least three persons are required, whereas one observer is sufficient in taking temperatures of furnace and slag.

Illustrations of all these methods of estimating furnace, metal, slag, and stream temperatures are given later on.

At present, no pyrometric method seems adapted to give satisfactorily the temperature of the metal in the Bessemer converter. The apparent temperature of the flames may of course be observed but this has yet to be shown to bear any definite relation to the metal temperature. There are fortunately other methods of recognizing the status of the reactions and the moment when a Bessemer heat is ready to cast.

III. Furnace Tapping Temperatures

If one can get a side view and within 50 ft. of the smoke-free side of the tapping stream of an open-hearth furnace or converter, accurate observations may easily be taken of its temperature. Even when sighting upon the stream from a distance, good observations may be obtained. The observed temperatures here must be corrected for the emissivity or specific radiation of the metal or slag, a correction depending on the radiation which is characteristic of the stream surface, and upon the colored light used in the pyrometer (see Sec. VI).

The greater brightness of the slag, readily noticed with the unaided eye, is caused by its higher emissivity and, also, in part, sometimes to the fact that it may be slightly hotter than the metal. Slag, however, is not a material of uniform composition nor is its brightness always the same from one cast to another at the same temperature. It follows that pyrometric estimations of slag-stream temperatures may be attended with considerable uncertainty.

IV. Teeming Temperatures

Here we have almost ideal conditions for obtaining accurately the temperature of the stream, which is clean metal with a slight evanescent surface of oxide giving to the stream, viewed through red glass, a characteristic transparent appearance, the color of the metal being greenish and of the oxide yellow. The corrections to be applied to the temperatures for the emissivity of iron viewed in red light have been exactly determined.[3]

V. Limitations as to Pyrometer Station

The arrangement of the open-hearth or Bessemer mills is a considerable factor as to convenience with which temperature observations of

[3] G. K. Burgess and R. G. Waltenberg: The Emissivity of Metals and Oxides, II. Measurements with the Micropyrometer, *Scientific Paper* No. 242, *U. S. Bureau of Standards.*

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furnace, tapping and teeming may be taken. Usually it is possible, however, to bring the pyrometer to within 10 ft. of the stream when teeming ingots; but distant observations—even to 100 ft. or more—may be taken with but a slight increase in uncertainty. These difficulties of pyrometer stations can usually be readily overcome. It is evidently necessary to have a portable apparatus that may be moved about while taking observations; and one that may be sighted from a considerable distance on a stream issuing from a 2-in. or 3-in. nozzle.

VI. Pyrometric Method and Emissivity Corrections

The observations here described were taken with the Holborn-Kurlbaum form of Morse pyrometer which has been frequently described;[4] although, of course, other types of optical pyrometer might have been used. The outfit is portable and readily adjusted and permits taking temperature readings as fast as they can be recorded. The instrument is calibrated so as to give correct temperatures when sighting into a clear, closed furnace at uniform temperature, which approaches the theoretical condition of a "black-body," which is said to have an emissivity equal to unity.

When sighted on an incandescent, free surface having a specific radiation or emissivity e, always less than 1, its true temperature Centigrade $t(=T-273)$ may be computed from its apparent (too low) temperature $s(=S-273)$ by means of the formula

$$\log_{10} e = \frac{c \log E}{\lambda}\left(\frac{1}{T}-\frac{1}{S}\right)$$

where $c = 14{,}500$, E the Naperian base, and λ the wave-length of light used in the optical pyrometer; here $\lambda = 0.65\mu$. This type of formula holds for any optical pyrometer using monochromatic light. The accuracy attainable in practice by this method is about 5° C. at 1,500° C.

The emissivity (e) of a smooth surface of liquid iron free from oxide is 0.37 (for $\lambda = 0.65\mu$) and no appreciable difference appears to exist between the emissivities of pure iron and of the steels containing considerable percentages of carbon, nickel or manganese, nor is there any appreciable variation of emissivity with temperature.[5] A solid surface of incandescent iron—which has the same emissivity as the liquid—cannot be maintained free of oxide in practice; it is always the oxide which is observed. Liquid iron oxide has a somewhat higher emissivity, 0.53, than liquid iron and a very much less value than the solid oxide the emissivity of which is about 0.92. Liquid slag has a variable emissivity of uncertain limits depending on composition but probably usually ranging

[4] *Scientific Paper* No. 11, *U. S. Bureau of Standards* and *Technologic Paper* No 38. Burgess and LeChatelier: *Measurement of High Temperatures*, 1912 Ed.

[5] See *Scientific Paper* No. 242, *Bureau of Standards*, cited above.

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between 0.55 and 0.75, and 0.65 is apparently the usual value of e for "dark" slag. For reasons noted in Sec. IV, the emissivity of the metal stream of steel is taken as 0.40 instead of 0.37 in computing true temperatures in Sec. VII.

VII. The Observations

In the following tables are given certain typical series of observations —taken from among many obtained in several large steel plants during the past few years—of furnace and stream temperatures, including all the cases mentioned above. The attempt has not been made here to correlate the ingot or heat characteristics with the temperature, although some data have been accumulated on this subject. It is believed, however, that these observations demonstrate the possibility of obtaining accurate temperature observations for the establishment of any of the numerous correlations which may be suspected to exist; and it is also believed that these observations indicate the practicability of pyrometric control of open-hearth practice, ingot casting, and similar operations.

Of course, the tracing out of the effect, say, of casting temperatures upon this or that ingot characteristic, presupposes the possibility of varying mill practice at will; and in any particular case, considering the large number of other variables that may enter, it will probably take a carefully laid out plan involving a considerable quantity of metal—some of which will undoubtedly be scrapped—to accomplish the end sought. This is also a case where the experimental work has to be done on the same scale as in practice.

1. *Observations in Bessemer Mill*

Table 1 gives the record of furnace casting and ingot teeming temperatures for 19 consecutive Bessemer heats of some 20 tons each of rail steel with a statement of the condition of the ladle as observed by the melter. The temperature estimation of the stream pouring from the converter may be rendered somewhat uncertain by the interference from smoke during the addition of spiegel, and to the intermittent presence of slag, and to the considerable distance from which the stream was viewed, since the pyrometer was located on an opposite gallery adjacent to the ingot molds. The temperatures given in the table, for pouring the converter, are each based on a series of observations taken after the addition of spiegel. It is assumed somewhat arbitrarily that the emissivity of this stream is 0.45, corresponding to a temperature correction of 119° C. which gives an average converter pouring temperature of 1,595° C. (2,903° F.) which may be somewhat low. If these apparent temperatures were for a more strictly metallic stream (e = 0.40) the true pouring temperature would be 1,623° C. (2,953° F.) on the average.

The teeming temperatures were more exact. Each temperature

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recorded is the average of several observations, usually 3 or 4, taken during the filling of an ingot mold of 3½ tons capacity, this operation occupying a time somewhat less than a minute. The emissivity of this stream is taken to be 0.40, which is probably exact to within 0.02 corresponding to a precision of 10° C. at 1,500° C.; the corresponding correction to observed temperatures is about 125° C. The observed temperatures are uncorrected for emissivity; the average temperatures only are corrected.

The most striking facts these observations bring out are, the relatively narrow temperature range of less than 50° C. within which the Bessemer converter may be and apparently must be operated, and the corresponding remarkable uniformity of ingot teeming temperatures.

2. *Observations in Open-hearth Mills*

(*a*) *Temperature of Metal and Slag Streams.*—Table 2 shows the casting and teeming temperatures for a series of heats and top-poured ingots of rail steel from a bank of 60-ton basic open-hearth tilting furnaces. As some 4 to 6 min. are taken in tapping such an open-hearth furnace bath the slag and metal streams may be observed, although it is necessary, on account of the pyrometer station being on opposite gallery, to wait for the smoke to clear away after additions are made to the metal in the ladle. The emissivity of the metal is taken as 0.40, and if that of the (dark) slag is assumed to be 0.65, the slag and metal streams have on the average the same temperature of 1,607° C.

As in the case of the Bessemer converter, Table 1, but a small variation in temperature from one heat to another is noticeable. The metal had its temperature lowered in the ladle by about 55° C. before teeming and ingot 19 was cast on the average at some 35° C. lower temperature than the first. The casting temperatures of the open-hearth and Bessemer practice here shown are seen to differ but slightly.

The tapping and teeming temperatures for a series of heats of steel of various compositions from acid and basic open-hearth furnaces in another steel plant showed the uncertainty of slag stream temperatures associated principally with the varying color of the slag. It was also demonstrated that all the heats were satisfactory and that the heat of the metal in each case was rated as "hot" which corresponded to a tapping temperature above 1,600° C. and a teeming temperature between 1,550° and 1,600° C., whereas the teeming temperature of the Bessemer and open-hearth rail ingots of Tables 1 and 2 ranged very closely, about 1,525° C. and 1,550° C. respectively.

Very satisfactory detailed observations of the temperatures of tapping of several open-hearth furnaces were taken by viewing the stream from the side with the pyrometer within 20 ft. of the tapping stream. It was noted that the heat may or may not be distributed uniformly through the metal in the furnace but the uniformity attained would seem to

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depend, in part at least, on the melter. Thus the temperature of the stream of one heat fell off some 80° C. in the 2 min. of pouring; that of two heats remained reasonably constant; and the temperature of the stream of a fourth heat increased some 50° C., showing that the top of the bath must have been considerably hotter than the bottom. The extreme range here observed in the true temperature of the metal in the tapping stream is 1,710° to 1,520° C.

In order to show how readily and accurately the temperature of a stream from a furnace, which is being tapped, may be followed, the actual readings taken for tapping heat 12 × 36 are given in Fig. 1. The condi-

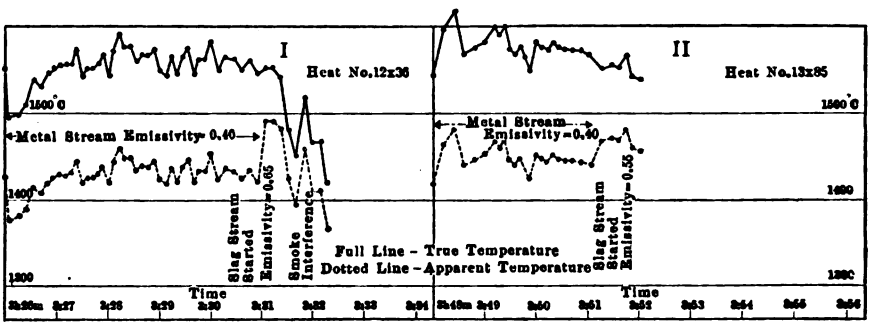

FIG. 1.—OPEN-HEARTH TAPPING TEMPERATURES.

tions of observing this tapping stream were less favorable, due to smoke, than usual. The fluctuations in pyrometer readings, particularly at the start and finish, are partly caused by intervening smoke. A piece of slag will give an occasional high reading. It will be seen that observations with the optical pyrometer may be taken easily at the rate of six or eight a minute. The observations on tapping another heat 13 × 85 are also shown in Fig. 1.

The temperatures of miscellaneous operations involving metal streams show that the average, corrected for emissivity, $e = 40$, for metal from a cupola is 1,394°; for metal pouring from a 1,000-ton mixer, 1,372°; for metal pouring into a 1,000-ton mixer, 1,396°; for spiegel pouring into the ladle, 1,289°; for recarbonizing iron pouring from small ladle, 1,287°; and for blast-furnace iron going into open-hearth mixer, 1,356°. The average values in each case have been corrected for emissivity on the basis that $e = 0.40$. All observations were taken with the Morse optical pyrometer.

(*b*) *Open-Hearth Furnace Temperatures.*—Observations of temperatures of open-hearth furnaces were made for some of the same heats as the casting temperatures of Table 2. The readings of the Morse and Féry pyrometers were corrected for instrument errors, but no correction was applied for emissivity or for absorption of radiation by the gases within the furnace. In the case of the Féry pyrometer, based on total

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radiation and calibrated in terms of the Stefan-Boltzmann laws[6] the correction is much larger than for the Morse or other optical pyrometer. As the amount of gases in the various parts of the furnace varies from time to time, it was not practicable to make any correction for the absorption of these gases. The use of a total radiation pyrometer to determine furnace temperatures does not appear to be warranted.

Each furnace had three doors with openings through which the slag bath could be observed. The method of firing here used impinged the flames down on the bath surface so that the flames interfered quite seriously with obtaining satisfactory observations. The effect of intervening flames is usually to cause the pyrometer to read too high temperatures. Those taken with the optical pyrometer through the middle door were, however, fairly satisfactory. The furnace casting temperature and that of teeming of first ingot were taken for comparison.

An indication of the uniformity of heating the open-hearth bath is seen by comparing the slag and metal casting temperatures with that of the slag surface as viewed in the furnace. Thus for heat 3 × 98 the slag in the furnace attained the unusually high temperature of 1,700° C. and maintained this for 20 min. up to 10 min. before tapping; the slag stream registered 1,621° and the metal only 1,559°. The slag stream appeared to be usually slightly hotter than the metal and the relation of the two is undoubtedly dependent upon the details of methods of firing. In noting the considerable difference between furnace and slag-stream temperatures, it is also to be remembered that the furnace cools very rapidly when heat is shut off, which often occurs several minutes before tapping.

In another heat the slag in the furnace registered 1,653°, the slag stream 1,606° and the metal 1,625°; in another heat the three temperatures were 1,635°, 1,612° and 1,597°; in another 1,610°, 1,659° and 1,628°.

In Table 3 are given measurements of temperatures of an acid open-hearth furnace, furnace heat which was followed in detail for 4½ hr. before tapping.

These three methods of observation were used: Sighting through small ports situated well to right or left corners of furnace, the pyrometer being directed toward middle back wall of furnace at arch and wall intersection; on the surface of 30 to 50 lb. of metal removed from furnace in a spoon; and on the slag in middle of the bath through peep hole in center door.

That the roof temperatures may have no relation to those of the metal is seen from the observations of heat 10 × 90, for which the fuel was cut off for 20 min. immediately preceding casting and the temperature of the arch fell from 1,700° C. to 1,378° during the first 14 min., while the metal on the average remained above 1,585°, although the observations on the metal spooned out show that the metal at the top of the bath was

[6] Burgess and Foote: *Scientific Paper* No. 250, *U. S. Bureau of Standards.*

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cooling very rapidly from 1,640 to 1,520° during those 20 min. of no heat being applied.

The temperatures of the arch are seen to be higher generally than the bath or metal temperatures, usually by some 50° C., although there appears to be no definite relation, except that when the furnace has been clear for some time the bath and arch temperatures tend to become equal. All of these heats were rated as "hot" by the melter and the behavior of the metal in the molds was good.

The metal temperatures taken by observing the metal in a spoon were not as satisfactory as one could wish. They are not, however, inconsistent with the casting temperatures, and those of the bath when sighting on the slag surface.

There is some uncertainty as to the best value of emissivity to use for the clear surface (always greenish in color) of the liquid metal in the spoon. The value here taken is $e = 0.40$, but there soon develops a haze over the surface, probably of oxide, although a pronounced oxide surface, for which $e = 0.53$, will not usually form on a well skimmed, hot surface until the temperature has fallen considerably. It appears as if the oxide first formed by the air in contact with the very hot liquid metal is dissolved. It is possible that a value of $e = 0.45$ for the emissivity of this surface would be more exact; the recorded "spoon" temperatures would then have to be lowered by some 15 or 20° C.

The procedure followed for securing temperature determinations of the metal bath by means of observations on the surface of the metal dipped out in a spoon was as follows:

A place was marked on the floor near the open-hearth furnace charging door and the optical pyrometer sighted upon this mark at an angle of about 45° and set to the approximate reading expected; a spoon to hold 30 to 50 lb. of metal was first preheated, then filled with metal and placed on the mark; the metal was skimmed of slag with a single sweep of wood and the taking of temperature observations begun immediately. The instant of removal of spoon from the furnace was noted with a stop watch and of each temperature observation; also the condition of the surface, whether clear, hazy, oxide, slag, liquid or solid, quiet or boiling, or scintillating. Skimming the surface more than once had sometimes to be resorted to if the steel was not quiet, but with quiet metal the best results were obtained by leaving the surface intact after the first skimming and noting the formation of oxide and applying the emissivity correction for metal or oxide. Temperature observations can usually be begun within 12 sec. of removal of spoon from furnace.

The last two determinations in the spoon of metal temperature of heat 12 × 36 of Table 3 are shown graphically in Fig. 2.

A comparison of the temperatures of several open-hearth furnaces for a time of 3 hr. in various stages of melting was made, the observations

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being taken by sighting on the surface of the slag. There is apparently no great difference for the five furnaces after melting has taken place, whatever the condition of the metal; the temperatures ranged from 1,567° to 1,679° C. and were usually above 1,620° C. The bath temperatures, when they can be taken under conditions in which the flames from the fuel do not interfere, appear to give a fairly good indication of the temperature of the upper surface layer of the metal; if the stirring or heating

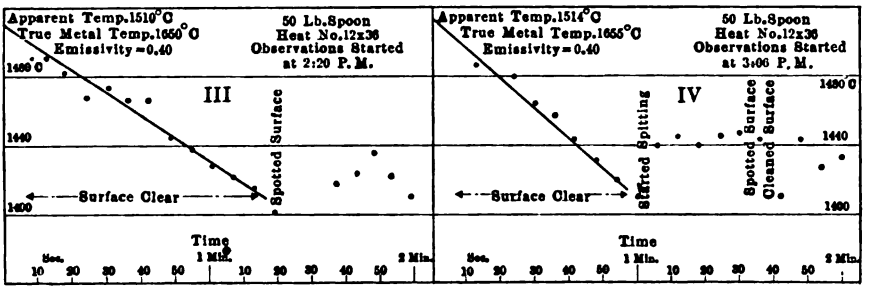

FIG. 2.—FURNACE TEMPERATURES BY SPOON METHOD.

has not been well done, however, the observations will be misleading if they are taken to apply to the metal as a whole.

VIII. SUMMARY AND CONCLUSIONS

The problem of temperature measurement and pyrometric control of furnace casting and ingot teeming temperatures is shown, by a series of observations taken in several steel plants, to present no serious difficulties or uncertainties.

For this purpose the most satisfactory type of instrument is one of the optical pyrometers using monochromatic light and permitting observation from a distance of streams of metal.

It is shown that the necessary corrections to the observed optical pyrometer readings for emissivity of metal and oxides to give true temperatures, are sufficiently well known, but there may be uncertainty in the case of liquid slags.

For streams of liquid iron or steel the most probable value of emissivity to take, with a pyrometer using red light of wave length $\lambda = 0.65\mu$, is $e = 0.40$, corresponding to a correction of 139° for an observed temperature of 1,500° C. The value of e for liquid slags is usually about 0.65 but varies with composition of the slag.

Determination of the temperature of the charge of Bessemer converters is not deemed practicable by pyrometric methods.

The operation of the open-hearth furnace can be gaged by the pyrometer, it being possible to control readily the temperature of the roof and

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of the bath of metal and slag by observations taken through ports; and the temperature of the metal may be had at any instant, with a fair degree of exactness, by observation with the optical pyrometer of metal removed in a spoon. This operation is more certain with acid than with basic practice, the basic slag being more difficult to remove.

The temperatures of the roof of an open-hearth furnace are shown to bear no necessary relation to that of the metal bath, which, it is again shown, may have zones of considerable differences in temperature depending upon the operation of the furnace.

The temperature of the roof of an open-hearth furnace, dependent upon the firing practice, may vary rapidly and within wide limits, 1,550° to 1,750° C. The temperature of the open-hearth bath is usually kept between 1,600 and 1,670° C.

There is a remarkable degree of uniformity in casting temperature actually acquired by the melters in practice. Thus, for 19 consecutive Bessemer heats the teeming temperatures of the ingots were all between 1,500° C. and 1,555° C., and a similar degree of concordance, although at slightly higher temperatures, was found in the open-hearth practice of several mills.

It is believed that a continuous, systematic following of the temperature, by the methods above outlined, of the various furnace and casting practices, on the part of steel and iron mills, would show the possibility of improvements, and greater certainty of production in quality of product; also changes and the effects of variation in ingot or furnace practice could undoubtedly be carried out with greater certainty than at present appears to be the case.

Several observers assisted in taking the above observations and acknowledgment for their help is due to Messrs. Crowe, Merica, and Waltenberg of the Metallurgical Division of the Bureau of Standards.

Acknowledgment of the opportunity for taking these observations is also gladly made to the management and members of the technical staff of the steel plants at Sparrow's Point, Md., Philadelphia and Bethlehem, Pa., and Washington, D. C.[7]

[7] In the complete paper are ten tables giving observations in detail with comments. Emissivity correction tables are also included.

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TABLE 1.—*Bessemer Mill*

Heat No.	Apparent Temperature of Metal Pouring from Converter	Apparent Temperature of Pouring into Ingot Molds						Mean of 1–6	Average True Temperature of 1–6	Ladle
		1	2	3	4	5	6			
29	1,476	1,397	1,377	1,377	1,389	1,369	1,367	1,379	1,500	Spots.
30	1,472	1,401	1,387	1,393	1,401	1,397	1,393	1,395	1,517	Spots.
31	1,488	1,415	1,419	1,403	1,393	1,383	1,375	1,398	1,521	Spots.
32	1,458	1,397	1,389	1,387	1,379	1,387	1,387	1,388	1,510	Spots—clean.
33	1,468	1,407	1,401	1,407	1,405	1,403	.....	1,405	1,529	Spots.
34	1,454	1,417	1,405	1,431	1,391	1,389	1,413	1,408	1,532	Clean.
35	1,462	1,419	1,417	1,403	1,393	1,405	.....	1,407	1,531	Clean.
36	1,490	1,434	1,429	1,432	1,429	1,419	1,417	1,428	1,555	Clean.
37	.....	.....	.....	.....	.....	1,383	1,372	1,397	1,384	1,505	Skull 4,000 lb.
38	1,490	1,385	1,393	1,377	1,403	1,385	1,385	1,388	1,510	Cut 1,500 lb. skull.
39	1,478	1,429	1,403	1,393	1,389	1,379	1,377	1,398	1,521	Cut 1,500 lb. skull.
40	1,486	1,424	1,408	1,408	1,401	1,399	1,399	1,406	1,530	Spots—cut 1,000 lb. skull.
41	1,492	1,436	1,401	1,375	1,389	1,424	1,399	1,404	1,528	Clean.
42	1,468	1,408	1,403	1,408	1,397	1,391	1,379	1,398	1,521	Clean—spots.
43	.....	1,434	1,419	1,409	1,407	1,389	1,372	1,405	1,529	Hot.
44	1,488	1,433	1,419	1,409	1,405	1,391	1,383	1,407	1,531	Hot.
45	1,474	1,405	1,405	1,405	1,403	1,401	1,387	1,401	1,524	Spots No. 1.
46	1,462	1,383	1,383	1,383	1,391	1,383	1,403	1,387	1,509	Spots.
47	1,484	1,434	1,432	1,417	1,413	1,413	1,409	1,419	1,545	Warmer—clean ladle.
Mean.....	1,476	1,414	1,405	1,401	1,398	1,394	1,391	1,400	1,523	ϵ = 0.45 for converter metal.
Correction for emissivity....	119	126	124	123	123	122	122	.....	.....	ϵ = 0.40 for ingot teeming.
Mean true temperature, Centigrade...	1,595	1,540	1,529	1,524	1,521	1,516	1,513	.....	1,523	

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TABLE 2.—*Casting Open-Hearth Furnaces and Ingots*

Heat No.	Furnace No.	Apparent Temperature of Pouring into Ladle		Highest and Lowest Apparent Temperature of Pouring into Different Ingot Molds			Apparent Temperature, Av.	True Temperature, Av.	Remarks
		Metal	Slag	Highest	Lowest	Molds			
5	64	1,490	1,540	1,419	1,393	16	1,405	1,529	Spots.
3	97	1,464	1,546	1,409	1,367	16	1,390	1,512	8,000 lb. skull. Melter during teeming said this heat poured hot.
5	65	.....	1,545	1,419	1,375	16	1,402	1,525	
3	98	1,431	1,554	1,421	1,403	19	1,410	1,534	500 lb. skull.
4	62	1,508	1,571	1,407	1,365	17	1,387	1,508	
1	32	.....	.....	1,466	1,393	21	1,420	1,546	Clean.
1	25	.....	1,530	1,448	1,389	22	1,407	1,531	
2	10	1,499	.....	1,427	1,367	18	1,401	1,524	Spots.
3	73	1,425	1,520	1,427	1,397	18	1,412	1,537	5,000 lb. skull.
3	74	1,429	1,572	.....	.....	..	.....	.....	
4	51	1,490	1,546	1,429	1,397	19	1,413	1,538	Spots.
1	36	.....	1,525	1,439	1,389	21	1,414	1,539	
1	41	.....	1,527	1,457	1,410	19	1,414	1,539	Clean.
3	80	1,478	1,546	1,450	1,397	20	1,411	1,536	New ladle spots.
1	42	1,516	1,545	1,448	1,389	19	1,414	1,539	Hot ladle spots.
3	82	.....	1,532	1,424	1,383	19	1,402	1,525	Spots.
1	43	1,468	1,532	1,415	1,375	19	1,392	1,514	
Mean		1,473	1,542	1,424	1,392	26	1,406	1,530	
Emissivity correction ..		134	66	127	122	26			
True temperature, °C		1,607	1,608	1,551	1,514	26		1,531	

$e = 0.40$ for metal streams.

$e = 0.65$ for slag.

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TABLE 3.—*Open-Hearth Furnace Temperatures*
Acid Furnace No. 12, Heat No. 36

Oil from	Roof through Ports		Bath Slag	Spoon Metal	Remarks
	Left	Right			
R	1,647	.....	.....	.....	Pyrometer sighted over flames. Not melted yet.
R	1,666	.....	.....	.....	
R	.....	1,667	.....	.....	
R	.....	.....	1,623	.....	Bubbles appear dark.
R	.....	.....	1,602	.....	Slag bergs.
R	.....	.....	1,627	.....	
R	1,682	.....	.....	.....	Furnace stirred.
R	.....	1,667	.....	.....	
L	1,694	.....	.....	.....	Oil reversed to left. About melted
L	.....	1,727	.....	.....	
L	.....	1,729	.....	.....	On flames.
L	.....	.....	1,655	.....	Black bubbles.
L	.....	.....	.....	1,566	Reversed oil to right.
R	1,693	.....	.....	.....	Flames.
	.....	1,710	.....	.....	Not on brightest flames.
R	.....	1,694	.....	.....	O. K. "Medium furnace."
	.....	.....	1,653	.....	
R	1,687	.....	.....	.....	Uniform.
L	.....	.....	.....	1,584	Reversed oil to left.
L	.....	.....	.....	1,618	
L	.....	.....	.....	1,604	
L	.....	.....	1,672	.....	Metal quiet, few bubbles.
L	.....	1,745	.....	.....	Fairly uniform.
L	1,725	.....	.....	.....	Reversed oil. Charging hematite.
R	1,710	.....	.....	.....	Fairly clear.
R	.....	1,710	.....	.....	
R	.....	.....	1,679	.....	Reversed oil.
L	.....	.....	.....	1,649	Reversed oil.
R	.....	.....	.....	1,660	
R	1,648	.....	.....	.....	Clear.
R	.....	1,640	.....	.....	Clear.
R	.....	.....	1,638		
R	.....	1,626	.....	.....	Clear.
R	.....	.....	1,601	.....	Slag bergs.
R	1,633				
R	.....	.....	.....	1,650	
R	1,628	.....	.....	.....	Clear.
R	.....	.....	1,606		
R	.....	1,627	.....	.....	Clear.
R	1,660	.....	.....	.....	Charged ferrosilicon.
R	1,655	.....	.....	.....	Stirred furnace.
R	1,650				
R	.....	1,674	.....	.....	Flames.
	.....	.....	1,638		
R	.....	.....	.....	1,655	Charged ferromanganese.
R	1,687				
R	.....	1,702	.....	.....	Flames. Hard tap lasted 5 min. Tapped.

Casting temperature 1,520 to 1,570 in 6 min. Stream viewed from smoky side.

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Discussion

J. W. Richards, South Bethlehem, Pa.—I think my affections are still rather with the radiation pyrometer than the optical pyrometer, for practical use, and I wish that Prof. Burgess would use the two together, under the same conditions, using the optical as his standard, if he prefers. Then, assuming the optical emissivity which he feels is correct for the optical pyrometer, he could calculate the thermal emissivity for use with the radiation pyrometer. If we could thus get reliable values for the thermal emissivity, we could then use the radiation pyrometer, and would avoid the use of the standard lamp, which has to be frequently calibrated for use in the optical pyrometer.

It is very advisable to distinguish carefully between the terms optical emissivity and thermal emissivity. There is considerable confusion caused by the bare statement that molten iron has an *emissivity* of so much, meaning by that its optical emissivity, for a light wave of a certain length, whereas what we want in the radiation pyrometer is its total thermal emissivity. I think it would be better to always use the terms optical emissivity and thermal emissivity. I should prefer to go on using the radiation pyrometer where possible, for it does not need a standard of comparison, or a standard light, but needs only the information as to what is the total thermal emissivity of the material being examined.

G. K. Burgess.—Regarding the use of the optical or radiation pyrometer, I purposely avoided the discussion of their relative merits for this purpose. I am perfectly willing that Prof. Richards should use the radiation pyrometer if he wants to get up as close to the hot metal stream as he has to do, to use it. The main difficulty in the use of the radiation pyrometer, as at present constructed, is the danger of not getting sufficient aperture when sighting on a small stream and the optical angle is such that you have to put the instrument within a few inches, almost, of the stream. That is a rather serious objection for convenient use. I have taken observations within 10 ft. of the stream from an open-hearth furnace, but I do not care to do it as a matter of practice; whereas, with the optical pyrometer, you can be 50 ft. away. I took some observations with the two together only the day before yesterday, and my preference has not been changed by that experience.

R. C. Drinker, Pittsburgh, Pa. (communication to the Secretary *). —A brief survey of the interesting paper by Mr. Burgess indicates that any discussion I might offer would be of little value, since my work of late years has been in the direction of a measuring apparatus so simple, and yet fairly accurate, that it may be placed in the hands of an operator just able to read figures—a condition so frequently found among steel melters.

Although my methods are crude, in comparison to the able work of

* Received Feb. 20, 1917.

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Mr. Burgess, yet my contention "that melting temperatures are quite as critical as those of heat treatment," makes it immaterial whether those determinations are accurate temperature measurements, so long as they are comparatively accurate, *i.e.*, enable one to duplicate tomorrow a satisfactory temperature of today, and thus in time establish valuable data.

HENRY M. HOWE, Bedford Hills, N. Y. (communication to the Secretary*).—Dr. Burgess would add to the value of this important paper if he would give us the carbon content of the steel made in the several charges, or, if this cannot be done, if he would indicate whether, as might be inferred from his paper, the teeming temperature is approximately constant and independent of the carbon content of the steel, or whether, as would be expected, it varies with the content and is materially higher for low- than for high-carbon steel. If he could add to Table 3 the time of the several tests, that would be welcome.

His Table 3 leads one to infer that, once the bath is melted somewhat more than flat, the temperature of the roof is nearly constant and is approximately the highest compatible with reasonable longevity. That is to say, the metal is heated by means of a roof and flame at nearly constant temperature.

The temperature of the observed upper surface of the slag should reflect that of the roof approximately, and in Table 3 this seems to be true, the temperature of both rising toward the middle of the record, and again falling. The slag as the medium through which the heat passes from roof to metal, should be intermediate in temperature between the two, as it is here.

But the temperature of the metal ought to rise continuously, unless when cold additions are charged, and this too seems to be true, allowing for slight observational errors, the total rise from the first to the last spoon-test being 89°.

The temperature drop of from 85° to 135° from the last spoon-test to the teeming is surprisingly great, suggesting that the spoon-tests represent the metal immediately beneath the slag, and therefore appreciably hotter than the mean of the bath. This is in general harmony with the relative temperatures of slag in furnace, slag stream, and metal stream.

In three of the cases on page 439 the slag stream is materially cooler than the slag in the furnace, and the metal cooler yet, from 28° to 41° cooler than the slag in furnace. In the fourth case the slag in furnace deviates from this order, and is 49° cooler than the slag stream and even 18° cooler than the metal stream, indicating that the furnace lay for some time without gas just before tapping, so that the visible upper surface of the slag cooled down to well below the temperature of the underlying slag layers, and even to below that of the metal.

* Received Apr. 17, 1917.

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The Manufacture of Weldless Steel Tires for Locomotive and Car Wheels

BY GUILLIAEM AERTSEN,* PHILADELPHIA, PA.

(New York Meeting, February, 1917)

THE derivation of the word tire (or tyre, as it is spelled in England) is obscure. Some dictionaries suggest that it is the aphetic form for "attire, covering," so called as being the outside covering or band of a wheel; also as a corruption of "tier," the band which holds, or "ties," the rest of the wheel together. One dictionary states: "The supposed derivation of the word is that bands were first used on wheels in the City of Tyre, Syria."

In the infancy of railroads, the tracks consisted of flat bars of iron, spiked or bolted to the upper surfaces of longitudinal timbers, which, in turn, rested upon masonry foundations. Stone blocks installed for this purpose on the old Germantown & Norristown Railroad, now the Chestnut Hill branch of the Philadelphia & Reading Railway, were to be seen for many years on the right-of-way, north of what is now Wayne Junction, Philadelphia, Pa. The "cornerstone" of the Baltimore & Ohio Railroad was laid Apr. 4, 1828, and the first locomotive in America was built by Peter Cooper at the St. Clair Works, Baltimore, Md., in 1829 "to demonstrate its adaptability to a curved road." It is believed that the first successful locomotive was built in Philadelphia by Tyler & Baldwin in 1832 for the Germantown & Norristown Railroad. Rufus Tyler and Matthias Baldwin were brothers-in-law; the latter's name will be identified with American locomotives until locomotives are forgotten.

The wheels on these first locomotives, designed to run on this primitive rail, were shod with plain iron bands, bent and welded. The flanges consisted of separate iron bands, bent sidewise, welded, and bolted to the sides of the wooden fellies, or rims.

Before many years had elapsed the tires were fabricated of iron bands, rolled in one piece with the flange projection on the side. The tire for each wheel was bent and welded and subsequently attached to the wheel center by shrinkage, bolts, rivets and various mechanical devices.

Germany is generally given the credit for first making such tires of

* Assistant to Vice-President, The Midvale Steel Co.

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steel without a weld, the ingot being cast solid, flattened, punched and subsequently spun, or rolled, on rolling mills designed for the purpose. Similar, or perhaps the same, mills had previously been used for "rounding up" the bent and welded iron tires. There is a tradition that weldless steel tires were made in the same manner in England about the time that their manufacture was being attempted in Germany, but the writer has been unable to discover any trustworthy record of this practice in England at so early a date.

As early as 1853 the German process was exploited, and before 1860 weldless steel tires were being turned out in considerable quantities there and in England. The popularity of these tires seems to have been instantaneous, their success being due not only to superior wearing qualities and accuracy of finish, but also to increase in safety.

Steel for tires was originally made in the crucible exclusively and the principal German maker had been engaged in their manufacture for many years, sending them to all parts of the world, before he ventured to introduce tires made from open-hearth steel. The ordinary practice was to cast individual ingots, poured from the top into cast-iron molds of great variety of shapes. The popular mold was pear-shaped, or the frustum of a cone with the small end up; this for the purpose of reducing the area affected by shrinkage. The ingots were subsequently flattened under a steam hammer, a small hole punched in the resultant slab, or disk, and this hole enlarged by hanging the punched disk on the horn, or beak, of the anvil, or lower die, on which it was rotated while the hammer struck successive blows on the outer edge. During this operation, the flange, where one was required, was roughly shaped. After this operation, the "beaked bloom," as it was called in England, was rolled to required dimensions in the tire mill.

Some of these early tire mills seem to have been horizontal two-high bar mills altered by extending the rolls through the housing farthest from the engine and hanging the tire on this extension. Many manufacturers followed this idea in designing new tire mills, the rolls being horizontal and the tire in the air while rolling.

It is interesting to note that even in these early days, tire manufacturers realized the improvement in quality that would be secured were it possible to cast material for tires in long ingots, cutting each into several blocks, one for each tire, and discarding the upper portion. As early as 1864, an English patent covering this was taken out. Each ingot was to be cut into four or five blocks by means of circular saws. A subsequent patent avoided the loss in sawdust by using, instead of saws, circular shears, or disks, the ingot being previously heated. These revolving disks were carried upon a shaft which, in turn, was supported at either end in eccentric bushings revolved by spur gearing so as to feed the shaft and its disks toward the center of the ingot until the cut was

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completed, when they were lifted to clear it. The hot ingot was supported by two corrugated rollers, driven, thus causing it to revolve.

The shaft bearing the disks could also be driven either by separate gears or by the friction between the disks and the revolving ingot.

The records do not show how successful this process was, but it seems to embody several ideas perfected by the next generation.

To avoid scrap in punching, a method was proposed in which a sharp-pointed instead of a flat-pointed punch was used. Ingots were top-cast in conical molds, small end up, the neck being closed after pouring by a plate clamped or cottered down upon it. Dead-melted steel did not seem to be expected in tire ingots.

The pioneers appear to have had great difficulty, at first, in punching, in the slab or upset ingot, a hole sufficiently large to admit the horn of the beaking anvil. One of the early methods was to drive a cutter, or wedge, about ¾ in. thick through the slab and afterward stretch and enlarge this slit by the introduction of tapered punches or mandrels. After this difficulty was overcome and the practicability of punches of circular cross-section proved, it seems to have been good practice if the four operations of upsetting, punching, beaking and rolling could be accomplished in four heats.

In the United States, prior to 1867, there were one or two mills designed for rolling, or rounding-up, iron tires. The manufacture of weldless steel tires had not yet been attempted. In that year, a member of an English family which had been long identified with the manufacture of cutlery, was induced by some Philadelphia capitalists to come to that city and establish a tire plant. From that beginning, 50 years ago, have sprung, directly or indirectly, all the other tire works in America.

At first, crucible steel was employed exclusively. The open-hearth furnace was at that time not sufficiently perfected to justify its use for such purpose, although but a few years elapsed before it practically supplanted the crucible for this specialty.

At first, ingots of circular cross-section were poured from the top in cast-iron open molds, one ingot for each tire. To insure accurate weights, pouring was stopped when the level of the fluid metal reached a chalk mark on the inside of the mold. Subsequently these molds were improved by the introduction of a cast-iron stopper, or heavy disk hung on the inside at the required height. This stopper, having a vent at its center to permit the air to escape, was suspended from a spider attached to the mold. After some years, top-casting was abandoned and all ingots were poured from the bottom. Still later, the practice of casting individual ingots, one for each tire, was replaced by the practice of casting long ingots, each of which was sliced cold into as many blocks as necessary and the top portion, enough to eliminate all danger from segregation and pipe, discarded. The advantage of this practice is self-

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evident. The slicing operation is stopped before the center of the ingot is reached, the remaining core being broken, so that the fracture at the center of the ingot can be examined.

In those early days, furnaces for heating high-carbon steel, as tire metal was then considered, were crude, and the circular cross-section of the ingots added to the difficulty. Consequently, it was thought necessary to give the ingots a preliminary, or "saddening" heat. They were charged into a comparatively cold furnace, brought slowly to a dull red heat, transferred to another hotter furnace and brought to the temperature necessary for punching. After this operation they were allowed to become cold; reheated and beaked; again allowed to become cold; reheated and rolled.

In a tire works with modern equipment, the sliced block is very slowly heated in a continuous furnace, upset, punched, beaked (when necessary) and rolled, without being allowed to become cold at any intermediate stage. The various operations are performed at a press, or hammers, and two or three mills, the partly finished bloom being transferred from one to the other with no appreciable loss of heat during the transfer. The results of this method are eminently satisfactory, the metal being continuously worked from the high forging heat to the proper temperature at which it should be laid down to cool.

Rolling tires differs from other rolling operations in one important particular not always appreciated by those unfamiliar with the details of the operation. In rail, structural or bar mills the billet is led from one pass to another, each designed to deliver the cross-section best suited for that following. Thus, in well-designed rolls there should be no risk of tearing or pulling the metal or setting up strains in one part of the section because its reduction has been less rapid than that of the adjoining section. On the other hand, in a tire, or other ring (except when it is possible to transfer from one mill to another), it is almost always necessary to accomplish the reduction in the same pass, without removing the tire therefrom, the reduction in the section being accomplished by forcing the rolls closer together while the metal is still between them. Any one familiar with ordinary rolling-mill practice will appreciate the difficulties this involves, and these difficulties are aggravated by the fact that in tires we are dealing with the cubical contents of the entire ring, not with the area of its cross-section alone.

The old tire mills were designed for tires of comparatively small diameter and thin sections and usually consisted of only four rolls, two of which might be called working rolls and two, guide rolls. One of these working rolls, the main roll, contained a single box pass, turned to fit the contour of the outside of the tire, the contour including the tread and flange with the front and back faces contiguous thereto. The other working roll, the pressure roll, was turned to coincide with the bore of the tire,

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being of exact width to enter the box pass in the main roll. One of these working rolls, usually the main roll, was rotated, or driven, by the engine, its bearings remaining stationary; the other working roll, usually the pressure roll, was rotated by friction between its periphery and the inside of the bloom as the latter rotated by friction with the main roll. The bearings supporting the pressure roll were carried on a slide, which, operated by hydraulic or screw power, advanced as required toward the main roll, thus reducing the opening between the two and the cross-section of the bloom or tire being rolled. The two guide rolls merely served to preserve the circularity of the tire by bearing against two points on its circumference equidistant from the main roll. They were driven only by friction between the tire and themselves and were carried back and forth on slides or carriages operated by screw or hydraulic power.

The more modern mills have four guide rolls touching the tire at four instead of at two points; have a pressure roll similar to that described above except that its face is longer than the width of the bloom or tire being rolled; have a main roll containing a very shallow box pass, not covering or overlapping the front and back faces of the tire, and, in addition, have two horizontal rolls, one driven by power, the other driven by friction, the latter being raised and lowered by screw or hydraulic power. With this universal type of mill, pressure is brought upon the section of the tire from all four sides, with beneficial results. These modern mills are, as may be imagined, more substantial and powerful than their predecessors. As a result, the quality as well as the quantity of work turned out shows much improvement.

Many changes, great though gradual, have occurred in the demands made by consumers upon tire manufacturers since the birth of the industry, and, as might be expected, most of these are for the better. The standardization of sections and diameters insures more satisfactory deliveries and eliminates many causes of error. The "mileage guarantee," usual in the early days, is now seldom demanded and still more seldom granted. Railroad-operating men appreciate that there are important factors affecting the hardships inflicted upon locomotive driving tires which can seldom be calculated, more seldom foreseen, and never observed or recorded. Safety can be demanded and secured. Wearing qualities would better be considered among the general results. Locomotive driving tires have sometimes shown abnormal wearing qualities, being apparently indestructible, and yet the efficiency of the locomotive shod with them has been reduced to a minimum by loss of tractive power.

One of the most successful manufacturers in Europe in the early days guaranteed that all the tires sent out from his establishment would do a certain amount of work before any repairs were placed upon them.

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This mileage guarantee, based upon the weight of the tire, he reduced to a formula in which W represented in pounds the weight of the tire and M the mileage it was guaranteed to run before turning. The formula was:

$$M = \frac{W \times 248.55}{2.2048}$$

which simply meant that for every pound of metal in the tire it would run 113 miles. This probably worked well in practice in those days, when the effect of brake action was nil, the loads were light, and practically the only difference in tires was their diameter, their section and the service to which they were subjected being the same. The heavier tire, larger in diameter though equal in thickness, would naturally outwear its smaller competitor, their lives being directly proportional to their weights.

In few specialties of the steel industry have producers and consumers worked so harmoniously in endeavoring to improve the quality of the product, and a comparison, could one be made, between tires manufactured in 1870 and those turned out today, would prove that this coöperative labor has not been in vain.

Discussion

The Chairman (Henry D. Hibbard, Plainfield, N. J.).—This excellent paper fills a gap in our *Transactions* and is most acceptable.

In the early days another way of casting tire ingots, sometimes called cheeses, because of their shape, was to use a 22-gage sheet-iron cylindrical can for each ingot of the proper size to give the weight desired. These cans were bottom cast in groups, usually of four, each can being surrounded by a heavy iron ring somewhat taller than itself, the top of the can being covered by a circular cast-iron plate having a small conical riser in the center, about 2½ in. in diameter at the bottom and 10 in. high. The top plate was clamped down to resist the upward pressure of the fluid metal and the space between the can and the outer ring, usually about 2 in. wide, was filled with dry sand. The riser permitted the escape of some of the air displaced by the steel and also increased the pressure in the steel when the mold was filled, and so diminished somewhat the tendency for gas holes to form.

But little was known of segregation and pipe in those days, though the center of an ingot was understood to be the worst part, and it was separated in part in the disk which was punched out in perforating the ingot. This disk was about 8 in. in diameter and from ¾ to 2 in. thick, the variation in thickness being to rectify the weight. When an ingot was too heavy, a thicker disk was made by not driving the punch in quite as far as when the ingot weight was right. Then the bloom was turned over and the punch

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driven in on the other side, which finished the punching and detached the disk. The tire bloom was then "becked" and later reheated and rolled as described in the paper. The forging was done with a 10-ton double-acting steam hammer.

The steel generally speaking had lower carbon, from 0.45 to 0.55 per cent., than tires today have, and the tires were smaller, the most common size of ingot weighing 1,060 lb. each.

BRADLEY STOUGHTON, New York, N. Y.—This paper was specially requested by the Iron and Steel Committee, because that Committee thought so important an industry should have its history written by one who was familiar with it, and before he had forgotten, or all the rest of the world had forgotten, its early stages. Mr. Aertsen has said that it is not an exhaustive research. I think it would not be any different if it were an exhaustive research, because our library made a careful attempt to find some written light on the history of these tires, but very little was found. Those who knew anything about it kept it to themselves or else were unable to put their ideas into writing, so that I really think we have a very valuable paper in this one by Mr. Aertsen giving us an account of the early days of this industry and the many important phases that it went through at the beginning and in the middle stages.

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The Seasoning of Castings

BY RICHARD MOLDENKE,* E. M., PH.D., WATCHUNG, N. J.

(New York Meeting, February, 1917)

ONE of the little-known characteristics of cast iron, which nevertheless has an important bearing on results where accuracy in machining is essential, is the ability of this material to ease up internal strains when allowed to remain quiescent for a more or less extended period of time. It seems as if the molecules in such a casting, by virtue of their "mobility," can adjust their relative positions to an extent sufficient to overcome some of the existing stresses.

The following instance will perhaps give a fair idea of the condition a casting may be in when just shaken out of the sand. A very large sheave-wheel, after shaking out, was taken outdoors to be cleaned and made ready for turning up. It was leaned against the side of the building, but before much could be done an arm tore apart with a loud report. Investigation showed that the sun had been shining on the upper rim, thus adding a slight strain to those already existing within the arm and thus overbalancing the strength of the metal in tension. Had this sheave been kept under cover for a while, or at least until machined, the strains would have eased off sufficiently to allow the sun to look upon it without disaster.

It will not be necessary to multiply examples. Every engineer knows the danger of water-hammer in pipe lines, particularly if the latter are of cast iron. Every mechanic knows, or should know, that it is not good to strike a fitting that is under steam pressure.

It will be necessary to say a few words on the "internal strains" in castings—the so-called "casting strains" we hear so much about. We all know that to get a casting reasonably true to the dimensions wanted requires a slightly larger pattern. The usual allowance for gray iron is $\frac{1}{8}$ in. to the foot (1 cm. per meter) and $\frac{1}{4}$ in. to the foot (2 cm. per meter) for white iron (all dimensions). This reduction in length, breadth and thickness in a casting is erroneously called "shrinkage." It should be called "contraction," as for practical purposes it is simply the difference in dimensions of the casting red-hot and cold.

The real "shrinkage" covers an entirely different situation. When a

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casting is poorly designed—thick and thin parts adjoining without special need—and in pouring the mold it is impossible to feed properly, the thinner sections set more quickly than the thick ones and may leave the latter without means of drawing in liquid metal to compensate for the reduction in volume in the act of setting. As the metal sets against the mold walls first, and gradually thickens from the surface inward, when the influx of fresh supplies is stopped there results a void in the center, or at least a spongy portion. This is "shrinkage," and can be seen more particularly in white iron, by reason of its greater reduction in volume from liquid to solid form, apart from the final contraction from red heat to ordinary temperatures. The favorite places for such "shrinkage" are at abrupt angles, in thick parts adjoining thin ones, in the rims of flywheels, hubs of pulleys, at the flanges of cylinders, etc. The correction of the trouble is not germane to this article.

It will be seen from the above that there are really two kinds of reduction in volume to be reckoned with: First, that due to the change from the liquid to the solid state; second, the reduction in volume after setting until ordinary temperatures have been reached. The first, often called "interior shrinkage," is a rather serious thing. The specific gravity of molten iron is about 6.65, and does not vary widely from this figure whether the metal on setting is gray or white in fracture—all the carbon being combined when in the molten state. On setting, however, if gray iron results the specific gravity will be over 6.8, and if white iron, up to 7.8—the formation of graphite in the structure accounting for the comparatively moderate increase in the case of gray iron. In an average cast iron, with 7.3 specific gravity, the increase in density is 0.65, or 9 per cent. —which means a very big decrease in volume for equal weights of molten and solid metal. This situation accounts for the quantities of molten metal that have to be added to a mold after pouring it full in the first place, and in the case of small castings—particularly when of white iron —for the funnel-shaped sprue left in the pouring basin or gate.

Contrast this with the eventual reduction in volume after setting. Here we have a linear reduction of about 1 per cent. in every direction, or say a 1-in. cube taken from a cube the sides of which are 100 in. each; an infinitesimal accomplishment as against the real metal shrinkage.

It stands to reason that if the metal in setting has the power to pull apart whatever liquid material may remain after feeding has stopped, and thus give large spongy parts in the interior of a casting, there must have been set up powerful strains which affect the strength injuriously. This is apart from the reduction of strength in the material for the section itself. In other words, not only will the metal have a smaller tensile strength because of the spongy nature of part of the section, but the interior strains counterbalance part of the tensile strength that is available.

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This situatior is intensified by the fact that the metal in setting does so far more quickly at the mold surface than in the interior—the cold sand walls drawing away the heat from the molten iron more quickly at the beginning of the setting process than later when this heat has to travel through a more or less thick shell of metal already set. The consequence is a higher percentage of combined carbon at the surface than in the interior of the casting. In the extreme case—that of chilling the surface—we have a white iron surface and a gray iron interior. The relative change in the specific gravities of the same molten iron turned into two extreme forms of iron as cast will indicate what strains there must be within the casting in question due to the differences in volume which the two metals want to occupy when set but cannot properly occupy on account of the quickness of the setting action.

Finally come the strains due to the contraction in the set material until ordinary temperatures have been reached. This has been stated as ⅛ in. to the foot in gray iron (about 1 per cent.) and ¼ in. to the foot in white iron (about 2 per cent.). In large castings this is very serious. Suppose, in the case of a big flywheel, the rim sets fast enough to hold the much cooler arm as in one set of jaws of a testing machine, the hub—held by the arms on the other side of the wheel—being the other set of jaws. Surely the arm in wanting to reduce in length ⅛ in. to the foot (1 cm. per meter) must be under a terrific strain if not allowed to do so. In the case of white iron the situation is much worse, such castings as hand-brake wheels (subsequently annealed for malleable castings) snapping apart when allowed to cool in the sand in the ordinary way. Such work must be shaken out as quickly as set, taken to special ovens and allowed to cool down very gradually.

Sufficient has been said to make the case of cast iron look very weak. Fortunately, there are two phenomena which help to overcome some of the injurious strains set up. The first is the fact that cast iron—particularly gray cast iron—in the act of setting (between liquid and solid) can be stretched. The second is the before-mentioned "seasoning" or easing up of the remaining strains after the final contraction—through the mobility of the molecules. It is the stretching of gray iron during the setting that saves the flywheel arm from rupture before the new strains due to final contraction are introduced. It is the inability of white iron to stretch very much which makes for so many cracked castings in the malleable process which would not be seriously affected by the final contraction.

The above discussion of the actual situation in making castings has another bearing. Purchasers of castings may wonder why foundrymen who really know something about their basic material are so uncompromisingly opposed to test coupons on their product. The arguments given above should convince the man who is at least somewhat familiar

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with cast iron that it is unfair to the maker of the casting to judge its value by a test piece subject to a variety of strains introduced as the result of position, manner of attachment, method of pouring of the metal, etc., of the mold. It is further unfair to the purchaser—if he only knew it—to judge by coupons, as there are many ways of artificially strengthening such test pieces. There is only one way of testing a casting properly, and that is to break it. Obviously this will not do, and hence for repetition work a given percentage of castings can be thus tested. For all other cases the only method of obtaining a reasonable assurance on the subject is the making of standard test bars, entirely apart from the castings but of the same iron, and making these test bars under conditions that give the iron the best possible chance to show just what it is, neither artificially strengthened, nor filled with strains and thus deliberately weakened.

This brings us to the real object of this note. Every mechanic knows that in planing up a slab of cast iron on both sides, to get a true job it is necessary to take a light cut, reverse, and take a cut on the other side, then reverse again for the finishing cut, finally reversing for the last cut. If this is not done there will be warped surfaces to deal with on account of the internal strains. Again, it is well known that to get a true piston is a rather difficult matter. Even after grinding to a finish it is apt to get out of true. It is not so generally known, however, that if such a cast-iron plate or piston is allowed to remain in storage for a long period, the results will be much more satisfactory. The castings have "seasoned." Where establishments are familiar with the situation, orders for castings are placed far ahead of requirements. Since, however, on getting to the bottom of a big pile the difficulty of tracing defective work becomes correspondingly harder, only shops having their own foundries are likely to do much storing.

The present demand for very high-class machined castings, as evinced by automobile cylinders, pistons, engine and compressor cylinders, etc., should bring this question of "seasoning" out very prominently. Inquiry by the writer has shown but little knowledge on the subject in the trade generally, though where first-class foundrymen were connected with the industries involved, these were very much alive to the matter. In general, the difficulty seems to be the inability of storing up ahead, or when so doing of striking defective product when least expected, particularly in such cases as castings for piston rings, etc.

For what seems the best indirect explanation of the "seasoning" action, we are indebted to the well-known metallurgist, A. E. Outerbridge, Jr., who in his famous experiments on "tumbling" castings to increase their strength, found that by the action of light blows, often repeated, the internal strains were relieved to such an extent that the real value of the metal came into play. The "mobility" of the mole-

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cules was aided by artificial means. Incidentally, however, the tests establish the "mobility" of the molecules in cast iron very satisfactorily. Replace half-an-hour's tumbling by 6 months' quiescence and the molecules will have done their work with somewhat the same results.

In view of the possible depression scheduled for this country on the close of hostilities in Europe, would it not be well to ease up operations slowly instead of shutting down tight? This would help the industrial situation adjust itself more safely and at the same time stock up supplies of castings which will be all the better for having seasoned. Inasmuch as similar improvement has been noticed in the case of ingots, billets and other iron-base products, the same argument would hold.

The writer presents this memorandum in the hope that a discussion may bring out present practice and possible applications with respect to an almost unknown but highly valuable characteristic of cast iron and allied materials.

Discussion

A. E. Outerbridge, Jr., Philadelphia, Pa. (written discussion).—The fact that iron castings improve with age has long been known. Many years ago the late Sir Frederick Bramwell, a distinguished engineer, formerly in charge of the metallurgical and gun-making department of the Woolwich Arsenal, England, determined to break up a large number of cast-iron mortars of an obsolete type that had been in stock for years. It was then reported to him that it was found to be much more difficult to break these old gun castings under the "drop" than it was to break similar gun castings recently made, under his own supervision; he changed the composition of the iron and still found that whenever castings made recently were broken up (on account perhaps of superficial defects) great disparity was observed in apparent strength between the old and new castings. Sir Frederick then made many other comparative tests, such as heavily charging with powder and firing these mortars repeatedly until destruction occurred by fracture. In each such test he was surprised and mortified to discover that the old guns, made years before he assumed charge of the works, withstood many more firing charges than any of the new guns that he submitted to these tests.

After thorough investigation of all of the brands of iron used, besides tests of "coupons" cut from the guns and other tests, physical and chemical, he finally arrived at the conclusion, which he incorporated in one of his official reports, that the difference in strength between the old and the new guns was not due to inferior metal or methods, but solely to the seasoning of the castings, which had long been made and allowed to remain undisturbed. Sir Frederick stated that in his belief the cooling strains existing in the castings when new gradually disappeared

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in course of years and the full strength of the metal in the castings was then secured.

In the year 1881 I was engaged at the large car-wheel works of A. Whitney & Sons (no longer in existence) in some metallurgical investigations which necessitated my personal attention frequently very late at night. It was quite a common occurrence for me to hear a sudden sound, like the firing of a pistol in the foundry yard, which was caused by the bursting, or breaking to pieces, of condemned chilled cast-iron car wheels that the inspector had caused to be thrown out while red hot, during the afternoon, instead of passing into the annealing ovens, owing to some molder's defects that were clearly apparent.

Castings of this nature are subject, of course, to abnormal cooling strains, as compared with ordinary gray iron castings, so that they almost always break apart unless put into an annealing pit as soon as they can be removed from the mold, and cooled very slowly during a period of several days. Then they are extremely strong castings if the cooling strains have been thoroughly released.

En passant, I may say that I have known a watchman who happened to be passing near a rejected car wheel at the moment it burst, in the night, to have his leg broken by the flying portion of the heavy wheel.

In 1888, another phase of this matter presented itself for study. At that time it was customary at the large machine tool works of William Sellers & Co., Inc., with which I had become connected as metallurgist, to "pickle" all small castings, such as pulleys below a certain size, in large vats containing sulphuric acid diluted with water, in order to remove the sand. A hard and fast rule obtained at that time that such castings must not be cleaned in the tumbling barrels for fear of breakage.

It was and is the daily custom to pour test bars from different portions of each melt of cast iron as it comes from the cupolas, and a card pattern was then made allowing 12 bars 1 in. by 1 in. by 15 in. to be cast side by side from one ladle of iron in one mold. These were all, of course, about the same size and quality and would break under nearly the same transverse strains under normal conditions. Four of these bars were cleaned with a scratch brush, four were pickled and four were cleaned in the tumbling barrel; all of the pickled bars showed about 10 per cent. loss of strength as compared with the companion bars cleaned with the scratch brush, while the bars that were "rumbled" all showed surprising gains in transverse strength, varying from 15 per cent. to more than 30 per cent. increase.

This led me to undertake a long and elaborate investigation, covering several years, partly to ascertain the cause of these marked differences.

In 1896, the late William Sellers, then president of William Sellers &

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Co., Inc., Philadelphia, asked me to furnish an account of my tests in response to requests that he had received.[1]

The claim was made in the paper that all iron castings are subject to cooling strains and that these strains can be, in great measure, quickly removed by subjecting the castings while cold to repeated shocks or vibrations. The opening sentence of this paper is as follows:

"It has been generally accepted as a fact that cast iron, under the influence of repeated shocks, becomes brittle, and will finally break under a blow which otherwise it would have withstood. It will probably surprise metallurgists, therefore, to learn that experiment disproves the supposed fact and establishes its exact opposite. The result of about a thousand tests of bars of cast iron of all grades, from the softest foundry mixtures to the strongest car-wheel metal, enables me to assert with confidence that, within limits, cast iron is materially strengthened by subjection to repeated shocks or blows."

The paper, though brief (comprising less than 10 pages) gives a résumé of a large variety of tests, together with tables, all showing that what I ventured to designate as "molecular annealing" of cold iron castings actually occurs from the effect of shocks or vibrations.

The rule at the Sellers' works was reversed so as to read that no small castings should go into the pickle tubs but must be cleaned in the tumbling barrels. Pickling, indeed, has long since been discarded entirely in this establishment.

After publication of the paper in the *Transactions*, the Franklin Institute in 1896 appointed a committee of Science and Arts to investigate these discoveries, and they made their own castings. Their report, dated May 5, 1897, entitled "On the Molecular Changes in Cast Iron Caused by Vibration," was printed in the *Journal of the Franklin Institute*, July, 1898. The report states "where the proper conditions are observed there will be a decided gain in strength in almost every case, at times running as high as 35 or 40 per cent. increase in the rumbled over the unrumbled bar. Many others have made similar experiments with the same results, both with high and low silicon irons." A table is given of their tests, showing results of breaking 166 bars, of which half were rumbled and half were not rumbled, showing gains in strength up to 40 per cent. The deflection showed as high as 48 per cent. increase in more than one case.

A later development, which pleased me greatly, was the insertion of a new clause in the revised specifications for tests of iron castings prepared and adopted by the American Society for Testing Materials, to the effect that test bars must not be rumbled or otherwise subjected to shocks

[1] A. E. Outerbridge, Jr.: The Mobility of Molecules of Cast Iron. *Trans.*, vol. 26, p. 176 (1896).

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or vibrations before breaking, but must simply be cleaned of adhering sand with a scratch brush. This clause was inserted through the influence of Dr. Moldenke, who has shown a sympathetic interest in all of these investigations as well as in others that have grown out of them.

I have also been informed that similar clauses have been inserted in some private contracts abroad for iron castings made to specification as to strength of test bars.

In 1904, at the Atlantic City meeting of the Institute, a paper under the same title as before was presented as a sequel,[2] giving results of tests of extraordinary molecular movement of cast iron caused by repeated heating and cooling. Companion test bars that were cast in molds 1 in. by 1 in. by 15 in. were there exhibited in which some bars had been caused to "grow" or increase in cubical dimensions over 40 per cent. without distortion or material change in appearance of the metal (unless highly magnified) as compared with untreated bars of the same original size.

A number of interesting and valuable practical applications have been made of these observations, none of which were presented at the patent office for the securing of any proprietary rights therefor before publication. Some of these uses have been made by others who independently perceived applications of the principles involved that probably never would have occurred to the author.

In conclusion, I would say that Dr. Moldenke's paper is a timely one and is well worthy of careful consideration.

THE CHAIRMAN (HENRY D. HIBBARD, Plainfield, N. J.).—This suggestive paper brings up an important though obscure subject regarding which more knowledge is needed. In addition to the internal stresses due to cooling from the casting temperature in the cases cited by the author, there are others which occur in certain kinds of steel articles, which may properly be considered with them in connection with seasoning. Thus, a piece of cold-drawn shafting if turned in a lathe will not be straight because the resultant of the internal stresses remaining is not the same as that of the stresses in the original piece. Again, a hardened steel shot contains internal stresses which may cause it to crack spontaneously, the most common case being that its point breaks off. This usually happens within a few weeks after hardening, if at all.

In the castings and shot, the internal stresses are due to different rates of cooling of the various parts, as the surface always cools in advance of the interior and must do so unless the rate of cooling is infinitely slow. In the cold-drawn shaft the internal stresses are set up by the cold work done in drawing the bar through the die. How do these stresses compare, and are they to be lessened or eliminated by the same seasoning? If so,

[2] *Trans.*, vol. 35, p. 223 (1904).

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what seasoning is best? Will a permissible time alone do it at uniform atmospheric temperature? Probably not. Will time alone do it at some moderately elevated temperature, say not over 200° C.? Possibly. Should the article be repeatedly heated moderately and then cooled? Probably. The daily fluctuations in temperature may be enough to effect seasoning in time. Old articles do not break spontaneously, nor presumably have excessive internal stresses.

The intensity of the internal stresses in a newly made or cooled article which seasoning might lessen are due to:

1. The rate of the change of temperature.
2. The shape of the piece.
3. The bulk or volume of the piece.
4. The elastic limit of the metal.
5. The ductility of the metal.
6. The coefficient of expansion of the metal.

It seems probable that heat, even if only of atmospheric degree, must be needed to effect the readjustment of the various parts of the article which results in diminishing or eliminating internal stresses. If, at any degree above absolute zero, heat causes vibration of the molecules, it seems reasonable to suppose that given a sufficient intensity of molecular motion ultimate positions will be such that each will be equally crowded or equally free on all sides, causing the annealed state which results in the greatest degree of softness and the highest specific gravity the metal is capable of possessing. Outerbridge's method of strengthening castings[3] which he calls annealing, by a succession of light blows may be founded on increased molecular movements, as the title of his paper assumes.

Iron castings have been seasoned by being kept for some days or weeks in a core baking oven with the temperature fluctuating from atmospheric up to 110° or even to 150° C.

Some projectile specifications require that the hardened shot be heated to the temperature of boiling water. Those that pass this ordeal without cracking are not likely to break spontaneously thereafter. The reason may be either that the heating increases the internal stresses so that if a projectile is in a dangerous condition it will break then as a consequence, or that the heating if accomplished without rupture, by its annealing effect lets down the internal stresses so that the danger of spontaneous rupture is avoided.

Delayed suicide of a shot when it occurs no doubt comes from fatigue of the metal, as it often happens when the temperature, and therefore also the stresses, are stationary.

Stresses due to cold working may be less amenable to seasoning. The strength of wire is often due more to the drawing operation than to composition Thus, wire for the cables of suspension bridges might be

[3] *Trans.*, vol. p. 26, 176 (1896).

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seriously weakened by seasoning if it took place to any considerable extent in a few years or even decades.

Howard found, in retesting iron test pieces which had been pulled beyond the elastic limit a quarter of a century before, that the elastic limit remained at the point at which the previous test was discontinued, showing no seasoning effect due to that length of time and ordinary temperatures.

On the other hand, as far as I have been able to find out, no pieces of good tempered steel are known which are more than a few centuries old and one surmises that seasoning has taken place in the old weapons. A friend of mine who tried with a file the hardness of some old pieces of steel in the British Museum came near being arrested therefor.

The whole subject needs investigation, the work being started now and being finished perhaps by future generations.

R. MOLDENKE.—We have with us Mr. Outerbridge, who came in while his discussion was being read, and I think he has something to show us.

A. E. OUTERBRIDGE, JR.—Owing to a delay in arrival of the train from Philadelphia, I reached this room in time to hear the latter portion of my own paper being read. It reminded me of a visit I paid to Menlo Park, N. J., about 40 years ago, on invitation from Mr. Edison, to hear the human voice reproduced on his first phonograph. After adjusting a sheet of tin-foil on the cylinder he asked me to speak into the mouth-piece, then reversing the cylinder and again turning the crank, I was astonished to hear the words "Mary had a little lamb" proceeding from the phonograph in an entirely unfamiliar voice.

I should, perhaps, apologize for the somewhat personal nature of the paper that has just been read, but as it is a reminiscence of original investigations along the lines of Dr. Moldenke's address this could scarcely be avoided entirely. I will not repeat any of the matter contained in the written discussion, but I now take pleasure in exhibiting some of the same cast-iron test bars which I had the honor of first showing before this Institute at its annual meeting in February, 1904.

Here are two bars of gray cast iron which were poured in one mold, from one hand ladle of iron, from identical patterns; they were both originally the same size, one bar remains as cast, the other has been caused to "grow" or increase in cubical dimensions 40.98 per cent., while still retaining its shape and metallic appearance.

R. MOLDENKE.—Tell how these bars were treated.

A. E. OUTERBRIDGE, JR.—The expanding of these iron bars was done by putting a dozen bars into a steel pipe which was 20 in. long, the ends stopped up. That pipe was put into a gas furnace with a pyrometer meas-

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uring the temperature, heated for 4 hr. and then taken out and allowed to cool. When it was cold, the bars were measured. After three heatings, the original shrinkage of these bars had been overcome and the bars were then 15 in. long, *i.e.*, the length of the mold. After 40 heatings, this bar which was originally $14\frac{13}{16}$ in. long became $16\frac{1}{2}$ in. long, and 1 in. square section became $1\frac{1}{8}$ in. It showed an increase of almost 41 per cent. in cubical dimensions while still retaining its metallic properties. That is the way in which these bars have been caused to grow, and a great many practical applications have been made of that property since 1904.

LEONARD WALDO, New York, N. Y.—What was the change in the specific gravity?

A. E. OUTERBRIDGE, JR.—The change in specific gravity bears a very close relation to the amount of expansion; pieces of cast iron originally 7.25 sp. gr. became 5.49. The increase in weight of the expanded bar is so small that I considered it negligible.

A MEMBER.—Do I understand that the specific gravity was diminished a third as the bar expanded?

A. E. OUTERBRIDGE, JR.—It diminished in proportion to the amount of expansion but not absolutely, 5.49 is the lowest specific gravity that I found of a gray iron bar which, before expansion, was 7.25.

R. MOLDENKE.—The figure you have given is practically the one obtained in the briquetting process for cast-iron borings, in which a pressure of perhaps 35,000 lb. per square inch is applied. The loose material is thus brought together to a density corresponding to that given gray cast iron which has been made to "grow," or open up in structure.

A MEMBER.—How was the specific gravity ascertained?

A. E. OUTERBRIDGE, JR.—Your question recalls another very interesting thing which I had entirely forgotten. In order to determine the specific gravity of these bars, pieces were cut off from the ends of a bar before expansion and from the ends of the same bar after expansion, from less than 15 in. in length to nearly 17 in., with corresponding increase in cross-section. I used the ordinary method in taking the specific gravity of the bar before expansion by weighing in air and then in distilled water, but I experienced unexpected difficulty in taking the specific gravity of the expanded metal for when immersed in distilled water bubbles of gas came off from the entire surface until I coated the expanded dried piece with a little waterproof lacquer to prevent the water from oozing in through the intermolecular spaces, driving the air out.

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It was just as easy to take the specific gravity in that way, of the expanded bar as of the other bar.

LEONARD WALDO.—May I ask what the temperature was that caused this remarkable growth or permanent expansion?

A. E. OUTERBRIDGE, JR.—The most efficient temperature is from 1,450° F. to 1,550° F. At lower temperatures you must heat the bars a great many more times, and at still lower temperature, say below visible redness, you can heat forever and get no such effect, unless heated in superheated steam, which causes considerable permanent expansion, as in the case of cast-iron valves.

LEONARD WALDO.—Suppose these growths took place in a vacuum?

A. E. OUTERBRIDGE, JR.—Prof. Carpenter of Manchester, England, while investigating these discoveries of growth of gray cast iron, heated bars with an electrical heating device in a vacuum, but his experiments were very inconclusive. He said that in some cases they did not seem to grow and in others they did.

MR. WALDO.—I understand that your remarks apply only to cast iron.

A. E. OUTERBRIDGE, JR.—Yes, only to gray cast iron, not to white iron, not to steel, not to wrought iron, not to bronze, not to copper. In fact, I have tested nearly all metals and alloys (except the rare and precious metals) and have found that gray cast iron alone possesses the peculiar property of "growth" or continued increase in cubical dimensions after being repeatedly heated and cooled.

M. H. MEDWEDEFF, Springfield, Mass.—If castings grow upon annealing, I would like to know how it is that castings are made to size, and retain their dimensions on annealing.

A. E. OUTERBRIDGE, JR.—All gray iron castings that are subjected to alternate heating and cooling above "cherry red" increase in size. There is nothing new in it, only that these experiments were made to find out why it was that grate bars warped and other castings cracked and twisted on being heated and cooled. The cause was heretofore supposed to be warping. We now know that there is actually an extraordinary increase in cubical dimensions. White iron (or "malleable iron castings" made of white iron) in which practically all of the carbon is chemically combined with the iron, does not, on the other hand, expand, when converted into gray iron by heat, sufficiently to overcome the original shrinkage of the metal. I stated this fact very plainly in my paper presented to this Institute in 1904 but it was questioned by the English investigators, who brought it out with an unimportant modification as an entirely new discovery some time later. I said that the free carbon in converted

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malleable iron was quite different from graphite existing in gray iron normally and is known as Ledebur's "temper carbon."

Among the practical applications that have been made of this property of gray cast iron that I recall at the moment, is that of a large number of cast-iron radiators that were too short to couple up with other similar radiators owing to an error in the new pattern. These were condemned and were being melted in the cupola when it was suggested that several of the castings be subjected to red heat over night in an annealing oven; after two such treatments the radiator castings had grown to the proper length and all that had not been destroyed were then treated and were actually placed in service in a large hotel in your city. Cast-iron pistons of internal-combustion engines and of pumps that have become worn too small to be efficient have been restored to size by this simple process of permanent expansion of gray iron by heat. These investigations all illustrate the mobility of molecules of gray cast iron and have therefore close relation to the subject of Dr. Moldenke's paper.

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Erosion of Guns—The Hardening of the Surface

BY HENRY FAY,* PH. D., D. SC., CAMBRIDGE, MASS.

(New York Meeting, February, 1917)

THE erosion of guns is a complex problem which can be solved only by a detailed study of all the factors involved. In the present paper it is proposed to submit the results of observations and experiments which have extended over several years. Certain facts have been established and preliminary reports have been published.[1] Conclusions have been reached since then which, it is believed, are worth presenting for the purpose of promoting discussion, and with the hope that ultimately the whole truth will be known. The particular phase of the problem which will be presented here is the hardening of the inner surface of the gun tube.

It has been known for a long time that after firing a large caliber gun for some time, the surface of the metal becomes hard and brittle, cracks, and wears away.[2] The character of this phenomenon may be learned by an examination of some results which are typical, obtained from a detailed study of a 12-in. gun. The gun was trepanned and rings representing various sections of the gun were cut out for examination. The first ring represented the metal at the muzzle end, and the other rings were removed at approximately 120, 240, 325 and 337 in., respectively, toward the breech end. The appearance of the surface of the metal in the different sections is shown in Fig. 1. The greatest amount of wear and maximum amount of hard surface layer was found in section marked *E*, which is nearest the powder chamber and at the beginning of the rifling; the maximum amount of cracking in *D*; the maximum amount of copper deposition in *C*. Section *B* showed heat cracks on both lands and grooves, and section *A* showed heat cracks *on the driving edge of the lands only*. There was much flow of metal in both sections *E* and *D*, and there was progressively less effect noticeable toward the muzzle end. The polished cross-section of these same pieces is shown in Fig. 2. The extent of the erosion is greatest nearest the powder chamber, and diminishes toward the muzzle end.

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* Professor of Analytical Chemistry, Massachusetts Institute of Technology.

[1] Tests of Metals, Watertown Arsenal, 1913 and 1914.

[2] This action is not confined to large caliber guns exclusively but will occur also in smaller guns, although, as will be shown, the action is much accelerated with the increase in the weight of the projectile.

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The depth of heat crack varies irregularly in each section, and somewhat regularly in sections proceeding from *A* to *E*, increasing in depth toward the powder chamber. The number of cracks does not, however, vary in the same way. In section *E*, where the greatest erosion has taken place, the smallest number of cracks appear.

The greatest depth of crack is found in section *D*, with an average depth of 0.038 in. The width of crack in this section varies between

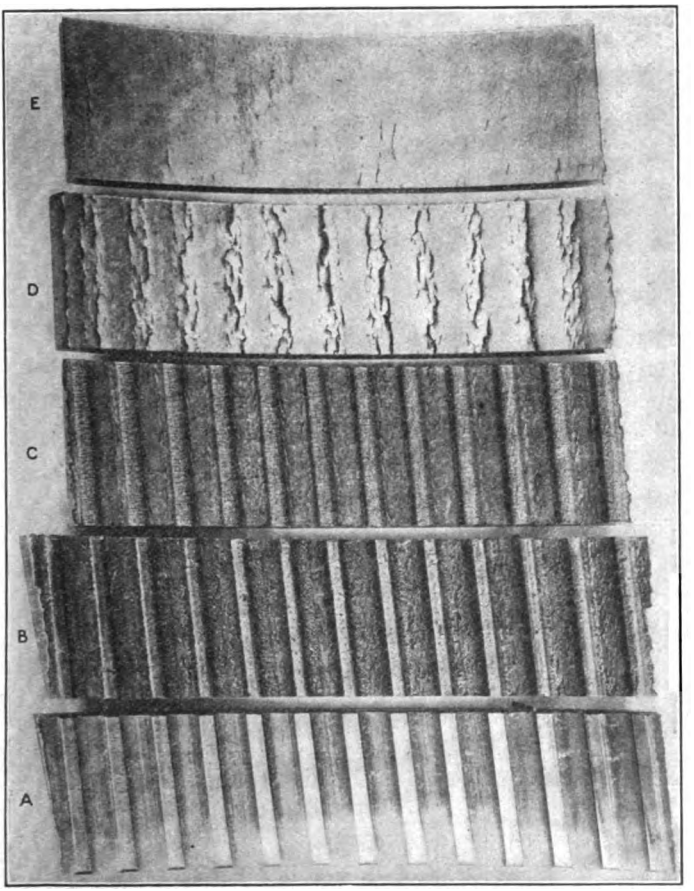

FIG. 1.

0.001 and 0.015 in. In section *E* the depth varies between 0.024 and 0.039 in. In section *C* the average depth of crack is only a trifle less than in *D*, but the width is not nearly so great. The cracks diminish progressively in depth and width in sections *B* and *A*, the latter section showing practically no cracks on the face normal to the axis of the gun. On a face parallel to the axis of the gun and cut along the driving edge, section *A* shows some clean, sharp cracks about 0.01 in. in depth

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Sections *B*, *C*, *D* and *E* show a hard layer of metal on the face cut normal to the axis. All five sections show the hard layer on the face cut parallel to and along the driving edge. The depth of the layer of hard metal on the face normal to the axis varies as follows: *A*, 0.0000 in.; *B*, 0.0004 in.; *C*, 0.0008 in.; *D*, 0.0013 in.; *E*, 0.0015 in.

The cracks are undoubtedly due to the unequal expansion and contraction between the hard and soft layers. When subjected to strain the hard, brittle surface develops cracks, and the cracks penetrate into the soft metal. The section showing the greatest thickness of hard layer

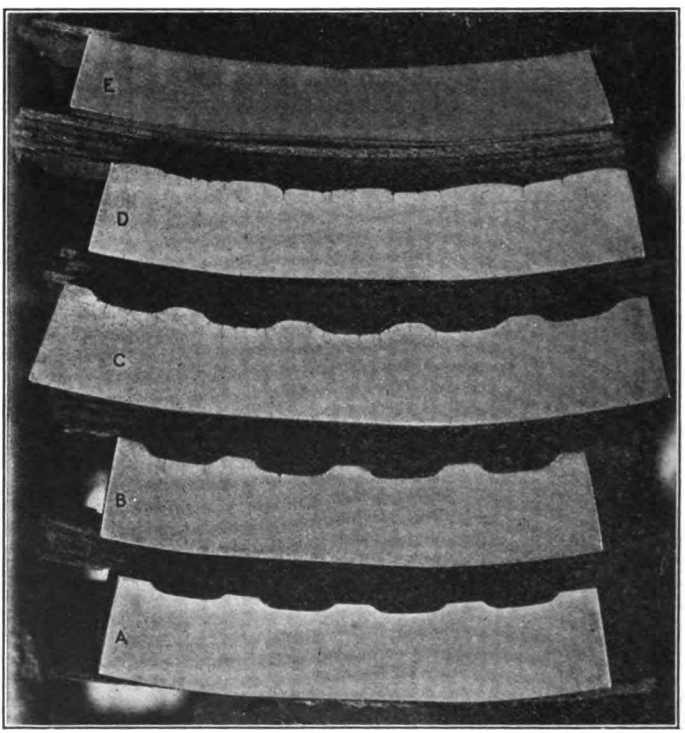

FIG. 2.

ought to show the greatest depth of crack, but other influences counteract this effect.

The microscopic appearance of a surface showing cracks is shown in Fig. 3, and in Fig. 4, the latter a characteristic structure always found at the junction of a land and groove. The interpretation of these photographs will be considered in detail after discussing the cause of the formation of the hard layer. In addition to the hardening which takes place in the gun tube proper, hardening and cracking of the surface takes place on the pressure plugs which are inserted in guns for the purpose of measuring maximum powder pressures. The pressure plug shows hard-

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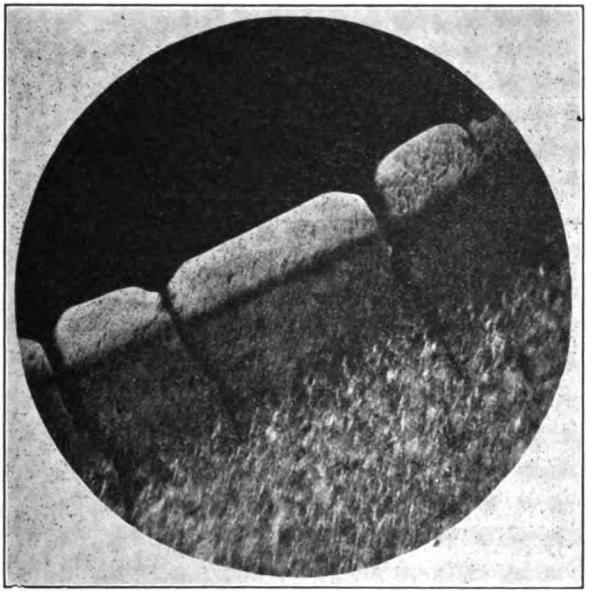

FIG. 3.—× 50.

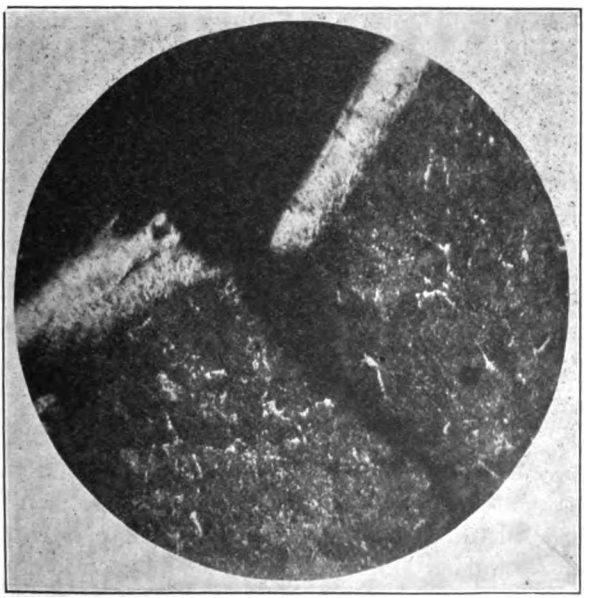

FIG. 4.—× 50.

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ness around the edge of the central hole, around the edge of the plug itself, and shows the effect of hardening in the form of cracks all over the surface, but particularly in the vicinity of letters which have been stamped on the surface. Any theory that may be offered to explain the hardening of the gun tube proper must also explain the hardening and cracking of the pressure plug.

While these results are in general typical of what takes place, there are variations in individual cases. For instance, the metal on the driving edge may not have been affected as it was in the 12-in. gun referred to, but it is merely a matter of time until the conditions as described are obtained. The action is a progressive one and is dependent upon the number of times the gun is fired.

The Cause of Hardening

Hardening may be produced by three well-defined methods: (1) cementation; (2) heat treatment; (3) mechanical deformation or cold work.

Cementation.—The principal products of combustion of the powder are carbon dioxide, carbon monoxide, water, and nitrogen. The equilibrium phenomena are very complex and will vary with the composition of the powder, the temperature and pressure. It is conceivable, however, that large amounts of carbon monoxide may be formed, and under the conditions of high temperature and pressure cementation could take place. Giolitti[3] has shown carbon monoxide to be an ideal carburizing compound, and on repeated firings small increases of carbon to the surface might take place. This would produce a hard, brittle layer on the surface richer in carbon than the original metal.

Heat Treatment.—When a piece of steel is heated above its critical temperature, Ac_1, and then suddenly quenched, it is hardened, the depth of hardening being dependent upon the temperature of heating and the rate of cooling. The temperature of combustion of the powder is sufficiently high to heat a skin of the metal above the Ac_1 point, and hardness would result if the surface were cooled with sufficient rapidity. The relatively low temperature of the large mass of steel would produce a rapid loss of heat.

Mechanical Deformation or Cold Work.—It is a well-known fact that when any ductile metal is subjected to mechanical deformation it becomes hard and brittle, and if the work is carried too far, cracking will result. Even the most ductile metals, such as gold and platinum, lose their plasticity and become hard and brittle when subjected to drawing, hammering, etc. In wire drawing it becomes necessary to anneal the metal frequently in order to restore plasticity and prevent it from cracking. This phenomenon is common to both ductile and brittle metals.

[3] *Journal of the Iron and Steel Institute*, vol. 84, p. 307 (No. 2, 1911).

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The mechanism of this action has been studied in detail by Beilby[4] and his conclusions have been confirmed and accepted by many other investigators. According to him, a surface skin may be built up by mechanical movement which gives unmistakable evidence that the surface must have passed through a state in which it possessed the perfect mobility of a liquid. This surface possesses distinctive properties which differentiate it from the surface beneath it. Hardening thus results from the formation at all the internal surfaces of slip or shear of mobile layers similar to those produced on the surface by mechanical movement. These layers retain their mobility for a brief period only, and then solidify in a vitreous amorphous state cementing together all of the surfaces of slip or shear throughout the mass.

Such a surface produced by polishing, burnishing, drawing, hammering, or cold work of any kind, possesses the property of hardness and brittleness. Metal in this condition may be restored to its natural crystalline, ductile state by annealing.

Having considered the manner in which hardness may be produced, it becomes necessary to examine the different methods in detail, and to see by which method the facts can best be explained. It would seem at first sight to be rather difficult to prove whether or not cementation had taken place, as the maximum thickness of layer which had been hardened amounted to only 0.0015 in. It was obviously impossible to cut off this layer for analysis, not only on account of the thinness, but also on account of the hardness.

Microscopically the appearance of this surface suggested cementite, as it remained bright after etching, as shown in Figs. 3 and 4, and, furthermore, in no case was there any evidence, even under high power, of martensite, which would also remain brighter than the original structure. Etching with boiling sodium picrate, which darkens cementite, gave negative results. The hardness of the layer and its appearance would indicate free cementite, but this test would, therefore, appear to be conclusive that the surface cannot contain any free or excess cementite. The test was carried out always with a control piece which contained about 1.25 per cent. of carbon, and therefore known to contain free cementite. Under all conditions the control gave positive results. There is some doubt as to whether or not a small amount of cementation takes place, and experiments are in progress which, it is hoped, will prove this point definitely.

While it seems fairly clear that the hard surface layer is not free cementite, judging from the sodium picrate test as evidence, it would have been difficult on this basis to explain the fact that the hard layer occurs only on the driving edges in the muzzle end of the gun. Selective formation would not seem probable.

[4] *Journal of the Institute of Metals*, vol. 6, p. 5 (No. 2, 1911).

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That hardness might be produced by raising the temperature above Ac_1 and suddenly cooling seems entirely possible, and most of the facts can be explained on this basis. The temperature within the gun at the time of the explosion is far above Ac_1, in fact may be above the melting point of the metal. A thin skin of metal is heated for a brief period during the explosion, and before the heat can penetrate any distance, it is cooled by the mass of metal back of this layer. Such a hardening ought to produce the microscopic constituent martensite, but this structure in its characteristic needle-like form is never found, although troostite is frequently found. Rosenhain[5] and Belaiew[5] have reported martensite in the hardened surface of the rifling of a gun, but so far in the ex-

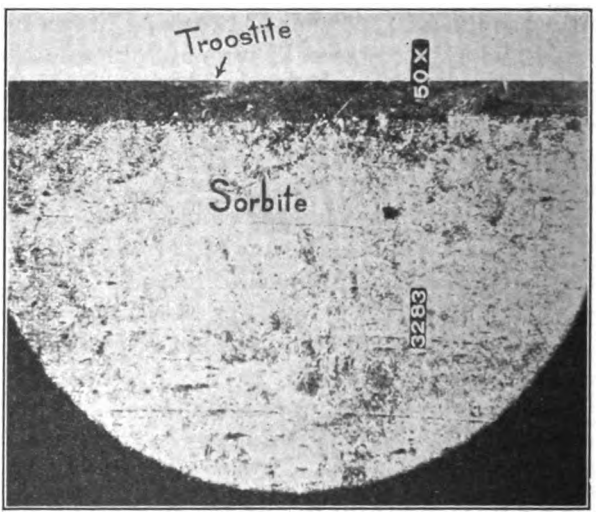

FIG. 5.—× 50.

amination of many gun fragments, and in trepanned guns, the characteristic martensitic structure has been missing.* In some experimental steels which had been subjected to erosion tests by allowing the gas of an ignited charge of powder to escape through a tube of the metal, there was found not only well-characterized martensite but also troostite adjacent to it. The essential difference in this case is that there was no projectile to produce mechanical deformation on the surface.

That the hard surface is actually composed of martensitic material is proved by the fact that on tempering it passes over to characteristic troostite. Samples were cut out of the ring from section *E*, and heated

[5] Rosenhain, Belaiew: International Association for Testing Materials, New York, 1912, *Proceedings*, vol. 2, section A, p. 127.

* Since the above was written, Dr. Langenberg, working at the Watertown Arsenal, has been able to indentify martensite in several samples.

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for 15 min. to 300°, 400°, 500°, 600° and 700°. The specimen heated to 300° showed troostite development most markedly and unmistakably, as shown in Fig. 5. The 400° and 500° samples also showed troostite, but less well defined, the transformation having progressed further, and there was almost complete reversion in the specimens heated to 600° and 700°. This experiment seemed to prove conclusively that the hard surface was martensite, but threw no light upon the fact that in the muzzle end of the gun the surface was hardened *on the driving edge only.*

It was thought for a time that mechanical deformation or cold work produced by the rotating projectile would account for all of the phenom-

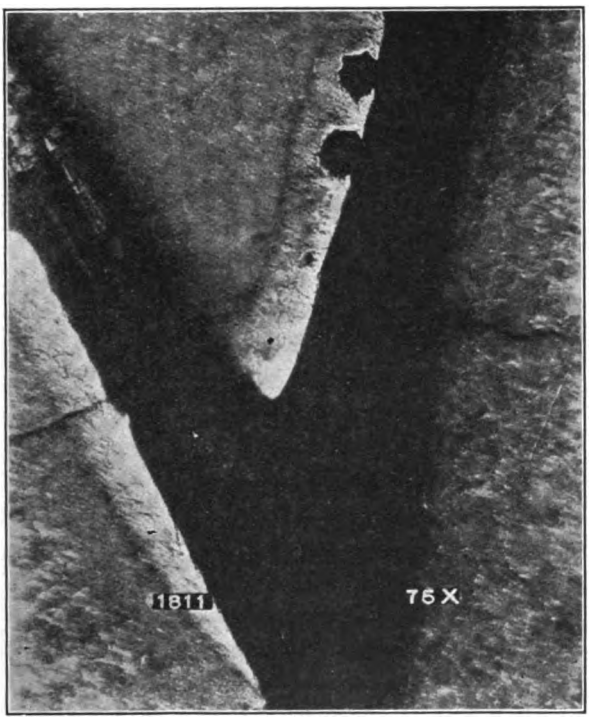

FIG. 6.—× 75.

ena, but this idea was negatived by (1) the martensite→troostite conversion; (2) by the fact that the powder chamber itself, untouched by the rotating projectile, showed hardness; (3) and by the fact that the pressure plug showed hardness.

That mechanical deformation has much to do with the production of hardness is shown by the selective hardening on the driving edge of the land on the muzzle section, and by some experiments which were conducted on pressure plugs.

Knowing that pressure plugs harden in the same way as the surface of

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the gun tube itself, it was decided to experiment with pressure plugs, and to follow the development of the hard surface layer in its various phases. For this purpose, a plug having approximately the same composition as gun steel was prepared for service. The metal was carefully analyzed, it was heat treated, and photographs were made of the fine sorbitic structure. The surface as a whole was uniform. The prescribed lettering was stamped on the surface. It was then placed in service, and after having been fired until it showed signs of heat cracking, was returned for microscopical examination. The face was polished and etched, and the

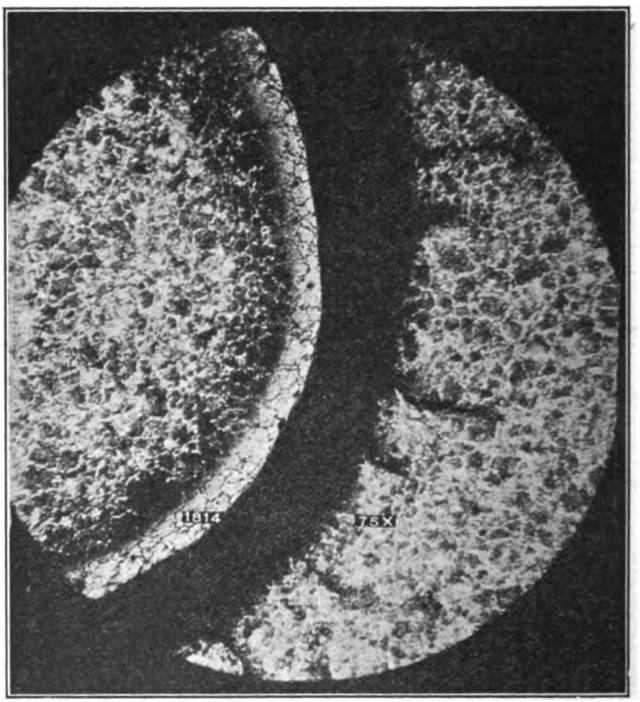

FIG. 7.—× 50.

structure seemed to be normal, but it was seen on detailed examination that the hard surface had begun to appear on the edge of the center hole and on some of the letters. Examination showed that this development had taken place at those points where the *maximum amount of cold work had been applied in stamping the letters.* This is shown in Figs. 6, 7 and 8, representing the letters A, O, and a period respectively. In the letter A and in the period the die was apparently not held in a truly vertical position and one side of the letter was cold-worked more than the other. Selective conversion of soft to hard metal has taken place at these points. This is shown in the lighter area of Fig. 6, and also of Figs. 7 and 8,

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the light area in each case being the hard area. Observations of a similar character were made on almost every letter on the face of the pressure

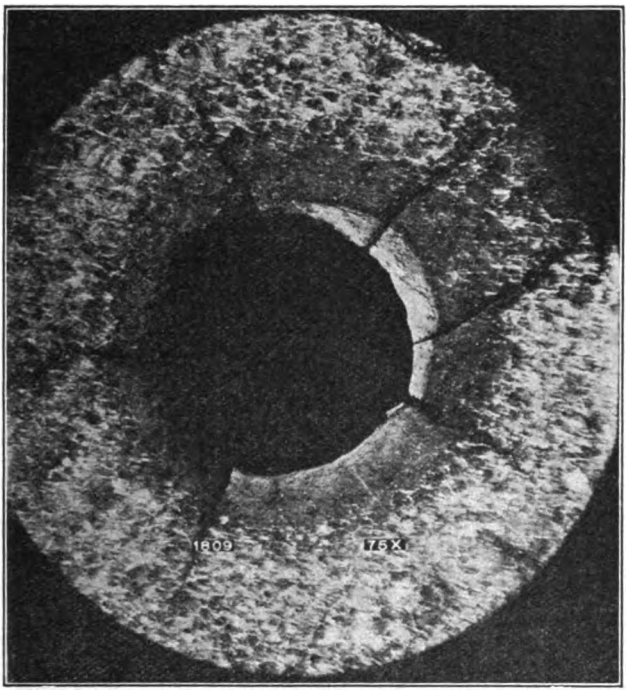

FIG. 8.—× 50.

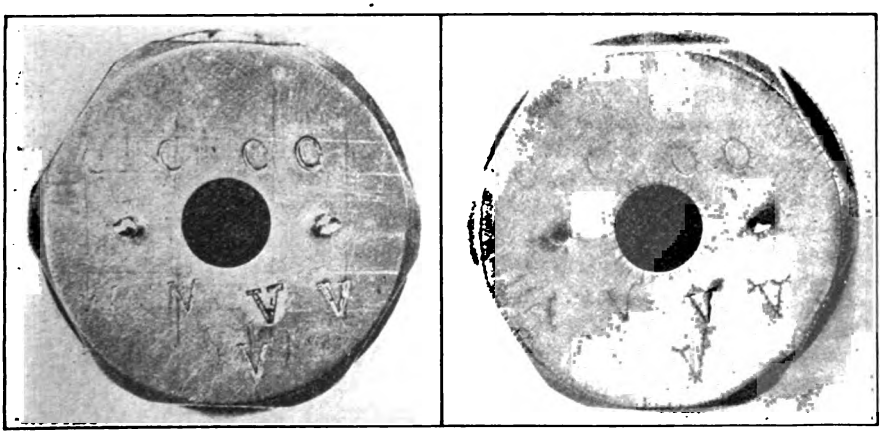

FIG. 9.—PRESSURE PLUG BEFORE HAVING BEEN PLACED IN SERVICE AND THE SAME PLUG AFTER HAVING BEEN IN A GUN WHICH HAD BEEN FIRED 100 ROUNDS.

The surface of the metal (right) has been worn away by some volatilization of the metal and by the polishing necessary to produce the structure.

plug, each showing partial conversion from soft to hard structure at the point of maximum cold work.

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This evidence seemed to favor the view that cold work was an important factor in the martensitization[6] of the metal.

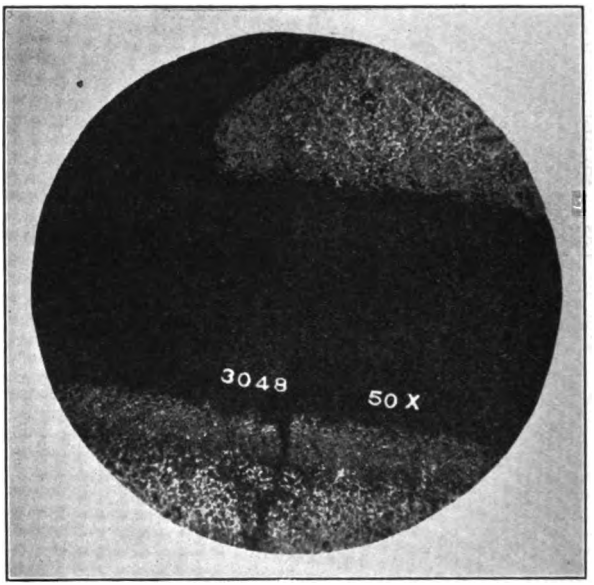

FIG. 10.—LETTER V IMPRESSED AT 5000 LB. × 50.

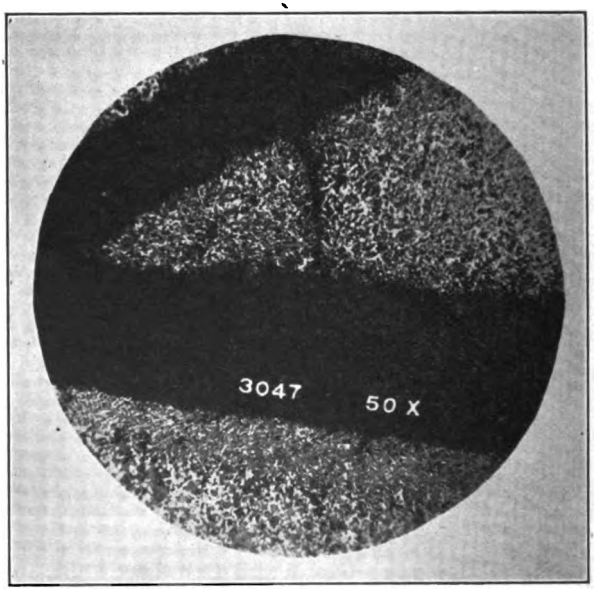

FIG. 11.—LETTER V IMPRESSED AT 2000 LB. × 50.

[6] The process of producing hardness by converting the free iron carbide into solid solution of iron carbide in iron. This is commonly done by quenching the steel from above Ac_1.

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In order to test this view systematically, a second pressure plug was prepared and the letters V and O were stamped on the face of the plug

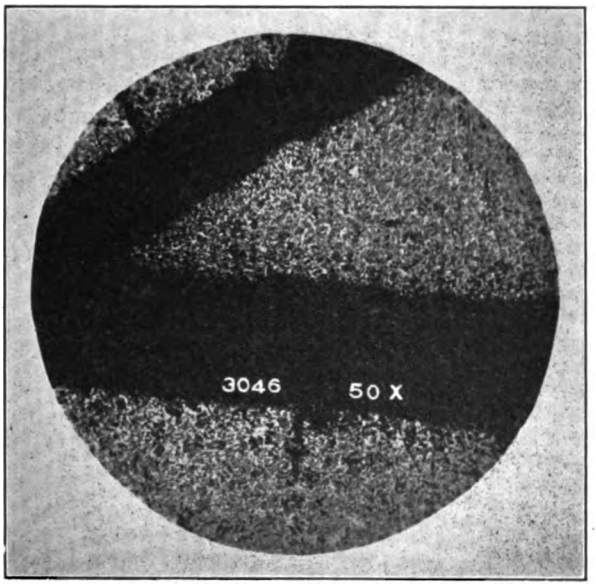

FIG. 12.—LETTER V IMPRESSED AT 1500 LB. × 50.

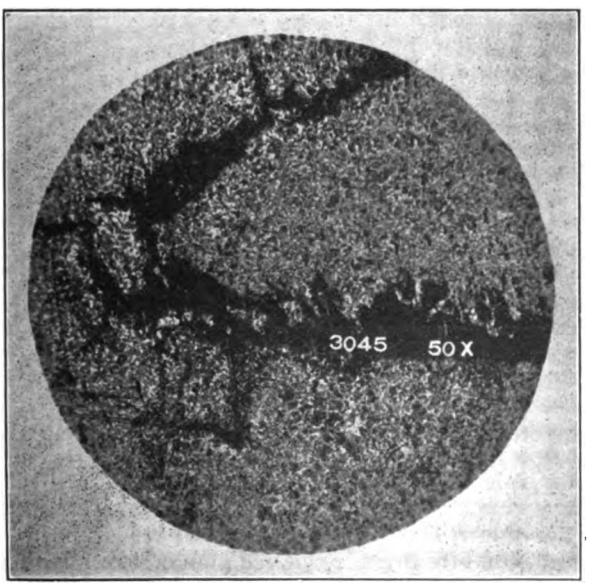

FIG. 13.—LETTER V IMPRESSED AT 1000 LB. × 50.

at varying pressures. This was done by holding the stamping die in one jaw of a tensile testing machine and the plug in the other jaw. The

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machine was now put into compression and the letters stamped at varying pressures, from 500 to 2,000 lb., as shown in Fig. 9. By mistake, one of the letters V was stamped at 5,000 lb. instead of 2,000 as originally intended.

The pressure plug was then put in service and the gun was fired 100 rounds, when the plug was returned for examination. The original sorbitic condition of the metal was found to have changed to the troostitic condition at those points of maximum cold work and to have remained practically unchanged at other points. Thus the first step in the change: sorbite→troostite→martensite had been accomplished. Figs. 10, 11, 12 and 13, show the letter V at 5,000, 2,000, 1,500 and 1,000 lb., respectively.

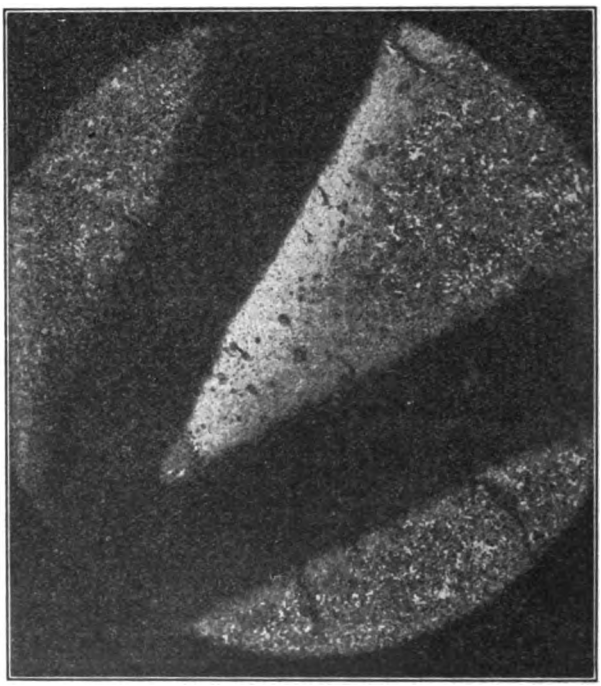

FIG. 14.—LETTER V SAME AS SHOWN IN FIG. 10, BUT AFTER FIRING 200 ROUNDS. × 50.

In Fig. 10, the troostitic character of the metal is clearly shown on the edge and apex of the letter V. The letter impressed at 2,000 lb. also shows the change, but not advanced so far as in the letter of greater pressure. At 1,500 lb. the change has begun but the advance has not been far, and at 1,000 lb. pressure the change is not noticeable.

In the same plug similar changes were noticed on the edge of the central hole, on the outer edge of the plug, and on two periods stamped at an angle of 45°. The period was strongly cold-worked on one side and the change was well-marked.

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The plug was again placed in service for 100 rounds and then returned for examination. The result is shown in Fig. 14, which is the letter V impressed at 5,000 lb. It will be noticed that the transformation has proceeded to its final stage, viz., the light etching structure which had previously been shown to be martensite.[7]

These experiments seem to prove that the hardness of the surface of a gun tube as well as the pressure plug is due to a combination of mechanical deformation and a process of martensitization. This process requires time, and the development takes place in stages. Even in the early stages there was evidence of heat cracking before the change of structure had taken place. When the structure begins to change it first shows as troostite, and then develops into martensite, but the latter is amorphous[8] in form. Temperature and work are also important factors. The transformation takes place with the greatest rapidity where the work has been most severe.

These factors counterbalance each other in the gun to a certain extent. In the powder chamber there is the maximum temperature, but the minimum amount of work. The only work which has been done upon the metal here is the original machining of the gun. In section *E* where the rifling begins, the maximum amount of cold work is done. The rotating band is undeformed and the whole surface bears on the rifling. This is also true of section *D*, but in each section toward the muzzle there is a decreasing amount of work due to the grooving of the rotating band until at the muzzle end the driving edge alone receives work.

The reason that a cold-worked metal is more susceptible to the sorbite→troostite→martensite change when exposed to firing is not clear. There may, however, be two factors at work: 1. Beilby has shown that mechanical deformation of any kind produces an amorphous state in the metal, and it is entirely possible that the amorphous metal may be more sensitive to the reaction, *i.e.*, it may have greater solvent power for iron carbide. He has shown, and many others have confirmed the observation, that such metal has a greater solution pressure and it is possible that it may have greater dissolving power for iron carbide.

2. Spring has shown that certain reactions, which ordinarily take place when under the influence of heat, can be made to take place at the ordinary temperature when under the influence of pressure. Thus he caused dry sodium carbonate to react with dry barium sulphate with the formation of sodium sulphate and barium carbonate. He also caused

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[7] There seems to be a difference in the degree of magnification of Figs. 10 and 14, but this is due to a wearing away of the metal by volatilization and polishing, thus exposing the deeper and narrower part of the groove of the letter.

[8] Capable of being easily converted into troostite, but showing none of the characteristic needle structure.

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powdered zinc to alloy with finely divided copper to form brass. In each case precautions were taken to eliminate any heat effects.

In a gun the original metal is sorbitic and the iron carbide is therefore mostly in the free form, only a minimum amount being held in solid solution. When subjected to the pressure of cold work, or the pressure of the explosion at a high temperature, solution may take place, each round of firing adding to the amount which goes into solution. This would lead naturally to the formation, first, of troostite, and when the solution is complete, of martensite. These steps are actually observed.

Either hypothesis—solution in amorphous metal, or solution by pressure—may explain the phenomena observed, but the latter seems the more likely. This would more adequately explain the fact that in erosion tests of steels of various composition, those steels high in nickel or manganese show the greatest amount of martensite formation. In these steels the Ar_1 point is lowered slightly, thus increasing the tendency for the carbide carbon to be held in solid solution form. Thus, in one case, a 3.50 per cent. nickel steel showed a martensite surface 0.05 in. in depth, while under exactly similar conditions a plain carbon steel showed only 0.004 in. Nickel and manganese lower the Ac_1 point, and hence make it easier for the sorbite→troostite→martensite transformation.

Experiments are now in progress with alloy-steel pressure plugs to see whether or not the hard surface develops more rapidly with them than with plain carbon steels. It is highly probable that those steels having the highest Ac_1 point will show hardening the least rapidly. A convenient means of testing this is to make up a series of steels into the form of small screws, stamp some letter at 5,000 lb. pressure, place them in the mushroom head of the breech block, and make observations after firing.

The preceding work was carried on at the Watertown Arsenal and the writer wishes to express his appreciation of Colonel Wheeler's interest and his courtesy in allowing the data to be published.

Discussion

Lawrence Addicks, New York, N. Y., (communication to the Secretary*).—There are two points regarding gun erosion on which I want to say a few words. The first is about the analogy to hardening of wire by drawing and the second the possible rôle played by thermal conductivity of the metal.

The ductile metals do not necessarily become brittle when cold-worked as it is possible to draw copper from ¼ in. in diameter soft rod down to the finest hair wire without annealing. This results, as stated in the paper, in forming a hardened core with a very hard skin. The

* Received Feb. 3, 1917.

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electrical conductivity is depressed some 3 per cent. and the tensile strength nearly tripled. A soft copper wire 0.08 in. in diameter and 6 in. long may be twisted 150 or more turns before the wire will snap under torsion. A hard-drawn wire will break after perhaps a dozen twists, the skin breaking down at one spot. Just wiping the wire with a cloth damp with dilute nitric acid has a remarkable effect in increasing the number of twists without appreciably lowering the tensile strength, showing the skin to be very thin. On the other hand, pickling the rod, from which the wire is to be drawn, in dilute nitric acid also greatly increases the number of twists which can be applied to the resulting wire. This indicates that the copper oxide scale produced when the wire bar is rolled hot is more or less rolled into the rod and may have much to do with the skin effect. We also know that if the conditions of drawing are not correct, the heat of deformation in the die may produce quite an annealing effect.

The application of this analogy to what occurs in a gun to produce the effects noted offers some difficulty.

The second point is that as the temperature of the surface of the gun bore has undoubtedly a great deal to do with the amount of erosion, one natural remedy would be to conduct the heat away fast enough to keep this temperature within desired limits. This brings up the question of the thermal conductivity of the metal of which the gun is made.

We know that the conductivity of pure metals is greatly altered by alloying and that the alloy is usually, though not always, lower in conductivity than the metal before alloying. This is interesting when it is considered that the high-nickel and manganese-steel alloys show greater erosion as stated.

Would it not be instructive to study the question from the point of view of heat conducted away from the eroded surface?

THE CHAIRMAN (ALBERT SAUVEUR, Cambridge, Mass.).—In forcing us to face and to discuss the important question of erosion of steel guns, Prof. Fay is performing a public service. His investigation has been fruitful and his results illuminating and suggestive.

It is not in a spirit of adverse criticism, therefore, that I venture a few remarks suggested by his paper, but rather to add my mite to the discussion in the hope that it may not be entirely without value.

Prof. Fay considers it conclusively demonstrated that the hard layers forming in the inside of guns of large caliber after they have been fired a number of times consist of martensite. Still, he admits that he never was able to detect in the hard layers the structural characteristics of martensite. This leads him to conceive the existence of amorphous martensite.

We may well hesitate before accepting this theory on the strength of the evidence offered. Martensite, as we know it, is essentially a crystal-

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line condition assumed by a close association of iron and carbon, possibly if not probably of the nature of a solid solution. Lacking this crystalline character we are hardly justified in calling it martensite on the ground, merely, of its being very hard.

The author appears to have demonstrated that the hard layers are thickest and hardest in those portions of the gun that had been subjected to the greatest amount of cold deformation. To account for this occurrence, he offers two possible explanations. In the first he assumes the formation of the amorphous state in proportion to the mechanical deformation, and he attributes to these amorphous layers the power of dissolving carbon and of being thereby converted into amorphous martensite. This, as the author undoubtedly realizes, is highly speculative.

I should like to ask Dr. Fay whether, in his opinion, this transformation of an amorphous aggregate of iron and carbon into amorphous martensite might take place above or below the critical range or both below and above it. It would seem as if it could occur only below the range, seeing that above the range the amorphous state no longer exists.

In his second explanation he conceives the possibility of sorbite being converted into amorphous martensite by work alone, or by pressure and heat combined. It is hardly thinkable, however, that pressure alone could convert sorbite into martensite—were it so, severely cold-worked wire should be martensite—and any piece of sorbitic steel could be made martensitic by crushing it in a testing machine.

Has not the author overlooked the important part that might be ascribed to cementation and quick cooling? He tells us that the products of combustion resulting from the burning of the powder contain much carbon monoxide, that the heat generated is sufficient to carry the skin of the metal, at least, above the critical range, and that the cooling of that skin must be very rapid because of the mass of metal abstracting heat from it. If we add to this that the gases are under great pressure which would greatly promote cementation, does it not seem that we have all the needed requirements for case hardening, and for the hardening of the carbonized layers, and that probable cementation and rapid cooling must be taken into consideration when attempting to account for the erosion of guns?

HENRY FAY.—I want to say that recent work has shown that we have been able to detect the martensitic structure. The reason why I did not get it in the first place was that I always had taken a cross-section which was at the maximum 0.0015 in. in thickness, and I never found it in that section. Polishing the face of the section, I have been able to detect martensite in it, not well-established martensite, but undoubtedly martensite. And what seems to me as conclusive is the fact that you can get well-developed troostite by tempering. The troostite can then be converted into sorbite, and then into pearlite.

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HUDSON MAXIM, Brooklyn, N. Y.—When a gun is fired using a charge of modern smokeless powder—I refer, of course, to large guns—the heat supposed to be generated by the combustion of nitrocellulose smokeless powder is about 4,000 to 4,500° F., possibly 5,000°, varying, I should say, with conditions, such as density of loading, weight of projectile, etc. The heat developed gives the products of combustion in a gun employing a nitroglycerin compound, that is to say, a compound of nitrocellulose and from 25 to 50 per cent. nitroglycerin, a temperature, it is supposed, of about 5,000 to 6,500° F.

Now, steel melts at 2,650° F., so you can see there is a large margin of temperature above the fusion of steel.

Then again, we must take into account the great density of the products of combustion under a pressure of about 35,000 lb. to the square inch. The result is that there is supposed to be a thin film of metal on the surface of the gun, fused, practically instantly, as far as our senses of measurement are concerned. When a 12-in. cannon is fired, the projectile is in the gun about $\frac{1}{16}$ sec.; the longer the gun, the longer the time and the greater the amount of erosion in the gun. These are some of the factors to take into account in attacking this problem of erosion in guns.

As soon as the projectile has left the gun, of course there is a terrific outrush of gases, and this thin film of metal is washed away, to a certain extent, by the outrushing gases; and when a big gun, of 12 or 15-in. caliber, is fired with the usual service charge of nitrocellulose smokeless powder, there is practically about 0.001 in. of erosion at every shot; that is to say, a film or layer of the gun has been fused and washed away enough to increase the caliber of the gun by about 0.001 in. at every shot. With the powders containing a large percentage of nitroglycerin, like those used abroad, especially in England, when they used 58 per cent. of nitroglycerin in the cordite, the erosion of the guns was many times greater than with pure nitrocellulose powder. The enormous advantages of employing a nitroglycerin compound are so great that it was very easy to err in the direction of the employment of nitroglycerin powder.

When one of our big, long, 12-in. naval guns is fired with a 1,000-lb. projectile, the amount of heat represented by the striking energy of the gun is, say, 50,000 tons—enough to melt 750 lb. of cast iron, which has to be taken from the powder gases, which are cooled by the act of propelling the projectile just that much. Now then, the time is so brief that there is not much depth to the heated layer in the gun. If you take a rod of steel and put it up to a very hot object, with the temperature of the electric arc or the temperature of a bar of carbon heated to the point where it will bend in an electric circuit, you can shove the steel bar up to it, melt off the end, volatilize the metal and not warm the bar to any considerable distance beyond the zone of fusion; consequently, immediately below the layer of heated metal in the gun, the gun is practically cold.

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Now, when the projectile has left the gun, followed by the gases, the thin film of heated metal necessarily contracts. In the act of heating and melting, of course, the metal has to displace itself upon itself, or crush itself, so that when it cools, it necessarily sets up terrific tension and cracks. Take a pane of glass, spread some glue over the surface of it and let the glue dry—the surface of the glass will be pulled off. The same thing is true if you dissolve some guncotton in acetone and pour it over the glass and let it dry, it will pull off the surface of the glass. The thin layer of this film adhering strongly to the surface of the glass will pull away the surface. Now then, when this thin film of heated metal shrinks in the gun, it not only cracks, but also it tends to deepen the cracks in the metal underlying or at the bottom of the cracks. We see that thing illustrated in a similar way when a puddle that has had in suspension a lot of earthy matter or clay has dried up and the surface has dried and cross-cracked, or crazed. The cracks not only go through the dry part, but they also pull apart the underlying clay and carry the cracks down into the wetter portion. The same thing takes place, it is presumed, in a gun. There is a tendency, also, to do work on the wall of the gun or the bore of the gun, due to the tread of the driving ring over the surface of the gun and the torsion set up by rotating the projectile, which serves to harden the surface, but I do not think that the mechanical work on the gun is anything like as destructive as the erosion from the powder gases.

Now, in this country, we have far less erosion of our guns than they have in any other country in the world. Erosion is, nevertheless, a very serious problem, even with us, but it is not so serious with us as it is with other nations who do not have a powder as good as ours. Before this war came they used to think, over on the other side, that their powder was better than ours, but they had to buy our powder after the war came, and then they learned that our powder is better than theirs. Of course, where they have built their guns over there to take nitroglycerin powder, employing smaller powder chambers, they have had to buy nitroglycerin powder from us and have it made for them, but we have, to a certain extent, in this country, met the problem of erosion with improvements in our smokeless powder.

I do not think it is any secret to say that when we took up the problem of rifle powders in this country we first tried a powder containing, say, 25 per cent. nitroglycerin, and I think the gun lasted about 3,000 rounds. Then we got some powder from the other side, fired that, and the gun lasted about 5,000 rounds. The DuPont Powder company then made some powder with which you can fire a rifle 20,000 times, and it will still be ready for more work, so the question of erosion of small arms is practically solved. Of course, there is room for still greater improvement, for there is great need of firing the maximum number of rounds with a rifle.

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BRADLEY STOUGHTON.—There is a great difference in the thermal conductivity of nickel and the thermal conductivity of manganese steel. Both are lower than the thermal conductivity of pure steel. Now, if the life of the manganese-steel gun was very much lower than the life of the nickel-steel gun, it would tend to strongly corroborate the theory that Mr. Addicks has advanced and perhaps he has some information on that point which he could give us.

LAWRENCE ADDICKS, New York, N. Y.—I regret to say that I have not.

HUDSON MAXIM.—May I say one word more about that matter of conductivity? I am very firmly impressed and believe that there is not very much conductivity. I should say that there is not time for a very great amount of conductivity from the heated surface of the metal. The condition we have when we fire a big gun is practically this: it is just like firing a shot out of a gun made of ice, using superheated steam for a propelling means. Now you get something of the idea of the condition in a cannon. If you figure it out, you will find that the result would be very much in favor of the ice gun using superheated steam, provided the ice were as dense and hard as steel.

LEONARD WALDO, New York, N. Y.—Some years ago it was my pleasure to be at Fishers Island, and while there in connection with the officers in service, I made a series of photographs of the interior of 12-in. guns after being fired. The photographic records are exceedingly interesting and very easily made. The structure of the edges is particularly clearly shown, and if the Department has no objection, I shall be very glad indeed to reprint these because they have never been made public. They give a very distinct idea of the nature of the disappearance of the edges of the rifling itself in the act of firing. With the erosion of guns, there also came up the question of a material to resist erosion. Breech blocks were made of aluminum bronze, and when I say aluminum bronze, I mean about 9.85 per cent. of aluminum and the balance pure copper. It was found to answer admirably for breech blocks. When it was proposed to use it for a gun, I pointed out that the modulus of elasticity was so low in any of the copper alloys, that it would not do for the gun itself, but that it might do for an inserted lining because it was shown to be very free from the action of these highly erosive smokeless powder gases under pressure. To try the experiment, I sent the rolled material for a rifle barrel to the Winchester Arms Co. Instead of using it as a lining, the company made the gun itself out of the aluminum bronze and sent back word that it was a very good barrel so far as resisting the erosion was concerned, but that it expanded under the impact of firing charges, which was, of course, exactly what one would have expected; but in the aluminum bronzes, I venture to think there is the best of the untried

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materials for making the linings of these large guns, because if you carefully inspect the whole theory, you will find that it works better than anything that has been proposed, and in the auxiliary parts, breech blocks and those things, it has been shown to be highly resistant to the temperature and effect of corrosive gases. I am very much impressed with the fusion of the skin-deep stuff in the lining of the gun, but I do not quite catch the argument of our friend, Mr. Addicks, about the failure of copper wire to take more than 40 twists in 6 in. on account of the exceedingly microscopic skin. I think that in the case of the copper wire, the working has gone pretty well through the wire itself and it is an integral cause of the failure of the wire to take this 60 or 100 twists as it should.

H. C. WILSON, Springfield, Mass.—This valuable paper deals specifically with the erosion of great guns, presenting problems quite different, in many respects, from those involved in small arms, with which I am immediately concerned. However, there are some questions of common interest which I would like to ask Prof. Fay.

Prof. Fay first says that after continued firing the surface of the bore becomes hard and brittle, cracks and wears away. Mechanically, to what extent may this deterioration be accelerated by the "whip" or intensive vibrations of the unsupported breech end of the gun, when we consider that a 12-in. 50-caliber gun is unsupported for 36 ft. beyond the trunnion mount to the muzzle, with an actual deflection of 0.2 in. below the bore-axis, equal to 4 or 5 min. muzzle angular deflection? Could the pounding or hammering of the traveling projectile be considered as "working" or forging the surface-metal of the bore? This axial vibration is sometimes so severe as to "ring" off the muzzles of our great guns. Would it be advisable to redesign the gun-chassis to better support this present outboard mass or otherwise dampen or flatten out these wave vibrations?

General Rohne has noted that the very fine skin veins of the fractured bore-surface appeared as ridges when magnified, and concluded that the metal had reached the melting point and had extruded under the high internal stresses.

It is interesting and valuable that Dr. Fay notes the impropriety of trying to compare tests made by firing powder charges in bombs or shells equipped with drilled plugs, and guns actually propelling projectiles; the conditions as to the erosive effect of the gases of combustion are quite different, largely due to the presence of the projectile in the gun, with no similar mass and obturation in the experimental bomb.

It appears that there must be two kinds of erosion in the bore of the gun, abrasive wear due to the travel of the projectile mass, and scorification or wire-drawing of the gases, not considering, for the moment, the

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destructive effect of great variations of temperature and pressure on the lining; and I recall that some years back the Ordnance department issued us projectiles with extra-wide rotating bands, having the immediate effect of increasing the M. V. and, we believe, accuracy, undoubtedly due to more efficient obturation. I believe that had we a spectroscopic screen or other suitable apparatus, we might have observed, and even measured, a darker color corona in the muzzle-blast, indicating more effective gas-checking of the broader banded projectiles.

Endeavoring to secure the highest polish possible in the small rifle bore, we have used several means, including lapping, and have also fired the first few rounds after dipping the bullets in deflocculated graphite, with highly satisfactory results. The resultant silvery-black surface seemed highly resistant to wear and erosion, was entirely unaffected by our hot-soda solution, and the subsequent lack of rusting indicated that the minute surface pores and scratches may have been covered or filled up by the graphite. I am wondering if this form of carbon did not have some case-hardening effect on the gun steel under the high temperature present.

As to alloy steel, the state of the art hardly yet justifies our use of other than a mild plain carbon steel, commercially basic open-hearth, of about 0.45 to 0.55 carbon, with manganese running from 1.0 to 1.28, silicon not over 0.30, phosphorus not over 0.06 and sulphur not over 0.06, so heat-treated as to give us a desired elastic limit running from 80,000 to 90,000 lb., with better than 20 per cent. elongation and 50 per cent. contraction, showing a satisfactory fracture, and free from seams, piping or segregation of any kind. Does Prof. Fay feel that the introduction of some alloy, molybdenum for instance, might give us equal or better machinability and ballistic characteristics, and at the same time decrease the erosion?

J. W. Richards, South Bethlehem, Pa.—If the heat conductivity of the metal has anything to do with the forming of these surface cracks by the rapid heating and subsequent rapid cooling, I think the question could be investigated by Mr. Fay's method of taking the test plugs which he puts into the breech, putting on them a thin sheet of metal to be tested, and back of it some heat insulation so that the metal might become heated. By putting different amounts of heat insulation back of it, it could be heated and chilled more or less rapidly by cutting out the cooling influence of the metal back of the surface, so that the effect of the more or less rapid cooling by the metal back of the surface might be investigated.

Mr. Maxim has spoken of the fusion of the surface, because of the high temperature. I think he used the term vaporization also, and I wish to speak a little on that point. The metals have vapor tensions at lower temperatures than is usually supposed. We know what the vapor tensions of some of the metals are at ordinary temperatures, and

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the tension goes up in an ascending logarithmic curve. We often speak of their boiling point, meaning their normal boiling point under one atmosphere pressure, but they have vapor pressures all the way down to ordinary temperatures. The vapor pressure of iron at its melting point is about 1 mm. of mercury, or $\frac{1}{760}$ atmosphere; but if you assume that at the temperature of the explosion the gases in the breech of the gun become saturated with the vapor of iron, you will find a quantitative correspondence between the loss of weight on the inside of the gun and the amount of iron vapor that could be contained in that volume of gases. Mr. Maxim has said that a large gun will gain about 0.001 in. in diameter in one shot; that 0.001 in. would correspond to a loss, on 1 cm. length of the gun, of about 0.6 gram of iron; whereas, if the gases in the gun were saturated with iron vapor, it would correspond to about 1 gram. The quantities are, therefore, of the same order of magnitude and the vaporization of the iron may account for the loss of weight, or the increase in caliber. This, if true, points to using means to reduce the vapor tension of the iron. We know that alloys of metals of higher vapor tensions have less vapor tension than the original metal; or, to put it in another way, that you can decrease the vapor tension of a metal by alloying it with a metal of higher boiling point. Now iron has a high normal boiling point, about 2,450° C., and there are not many metals with a higher normal boiling point, or a less vapor tension at a given temperature. The metals that can be named as having a higher boiling point than iron are particularly boron, titanium, tungsten and molybdenum, and the question to be solved, as a field for experiment, is whether the use of alloys of iron with these constituents would not decrease the vapor tension of the iron and possibly decrease the loss by vaporization in the firing of the gun.

ARTHUR L. WALKER, New York, N. Y.—I would like to say a few words in regard to the theory mentioned by Mr. Addicks, namely, the effect of a very thin surface coating of oxide of copper—the results he found were questioned. Some time ago my attention was drawn to a case of serious defects in copper rolled from cakes cast in the regular manner. This copper cracked when subjected to the test imposed. The test was a very severe one. The copper cakes were rolled into sheets ½ in. thick and cut into blanks. The blanks were cupped, and from the cupped blanks rings were cut to be shrunk on shells, the copper ring taking the rifle of the gun. A test piece cut from the rings was bent cold in the "back-bending" test—in other words bent 180° back on itself. From the same charge, certain samples cracked while others did not. A microscopic examination showed that in every case one side of the ring had more suboxide of copper than the other, which would be the result naturally expected. When a cake is cast the surface is necessarily exposed to the atmosphere for a short time and a film of suboxide of copper

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is formed on top of the cake. In rolling, this film is rolled into the cake so that naturally one side of the sheet has for its surface a thin film of suboxide of copper. When the test piece from the cupped blank was bent so that the original surface of the cake was on the outside it was fractured, sometimes all the way through. When the test piece was bent so that the original surface of the cake was on the inside the sheets did not crack at all. This illustrates in a striking manner the effect of this thin film of suboxide of copper on the sheets. It also bears out Mr. Addick's theory as to the results which might be obtained by the removal of this oxide film. In the case mentioned, apparently the brittle eutectic of Cu_2O + Cu cracked when subjected to stress and started a fracture which extends into the copper itself. I had no personal experience with the tests just mentioned; the results were simply brought to my attention and I have samples in my collection which illustrate them.

HENRY FAY.—I would like to say a word in regard to Major Wilson's amorphous metal or allotropic metal, as he stated it; I hesitated to use the term amorphous because of the many interpretations that might be put upon it. It seems to me that it is an important term, because we have a metal which is in a markedly different condition from that in which we ordinarily consider it; *i.e.*, as crystalline metal. We all know that if a metal is cold worked at the surface, it will corrode more readily at the point where it has been put in the amorphous or cold-worked condition, that is, it has a different solution pressure from the crystalline metal. I also used it because it seemed to me that if the metal is amorphous, having a greater solution pressure than the crystalline metal, we might also expect that it would have a greater solubility effect on iron carbide. In other words, amorphous metal might promote the solution of iron carbide with a stronger tendency to hold it in the form of solid solution, than the metal which was in the crystalline condition.

In regard to Major Wilson's other question, as to motion: It seems to me that that might be explained by the fact, as Mr. Maxim has said, that the interior of the gun increased in diameter 0.001 in. on firing, and having this hard skin, it might easily, on cooling, contract and crush a small amount of metal and make a ridge.

RALPH EARLE, Washington, D. C. (communication to the Secretary*). —Prof. Fay treats of the phenomena attendant upon the firing of large caliber guns for many rounds, that is, the fact that the surface of the metal becomes first, hard and brittle; second, cracks; and third, wears away. These conditions are termed by the Navy: cementation, heat cracks, and erosion.

A very unusual set of conditions exists in the firing of a gun. A large quantity of powder is suddenly converted into gas at a very high

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temperature, and the pressure of the gas rises quickly to several thousand pounds on the square inch. The effects produced by this action are several.

If steel is heated to a high temperature, and suddenly cooled, it will be hardened, and this hardening will be more pronounced with high carbon steels. A thin film of the bore of the gun is heated at each discharge of the gun and suddenly cooled, the rate of cooling being accelerated by the cold metal of the tube. It is found that after a few rounds the bore becomes quite hard. The bores of some guns, returned from proof after five rounds, are found so hard as to resist the ordinary tools used in manufacture.

Aside from the hardening due to heating and rapid cooling, steel can also be hardened by working it at a fairly low temperature. The friction of the products of combustion and the rotating band may assist in the hardening of the bore aside from the effect of rapid cooling. If the metal of the bore absorbed carbon, the additional carbon would assist in making the bore harder.

After a number of rounds have been fired from a gun, there is found to be a network of fine cracks covering the chamber in the neighborhood of the compression slope and extending down the bore. These cracks are heat cracks and are found in closed bombs as well as in guns. Various explanations have been offered to account for them. The hardening of the metal alone sets up surface strains, as well as making the metal more brittle, and in this condition it is liable to crack when subjected to high pressures. If the metal has been superficially carburized by the action of the gas these conditions are exaggerated. The strains which exist in the metal of the bore are caused by a thin layer of heated metal expanding against metal relatively colder.

The physical change in the metal of the bore assists in generating heat cracks. The physical characteristics of the steel over a thin film are changed by firing, the metal becoming much harder, with consequent reduction in ductility, and elongation, and increase of brittleness.

In the hard state, steel will rupture quickly where there are any sharp changes of direction in its surface. Hence heat cracks are most pronounced at the angles at the bottom of the rifling grooves, or on the driving edge, as Prof. Fay states it.

One explanation of the cause of heat cracks is this: At each discharge of the gun, the metal at the surface of the bore is heated to a temperature that would cause it, if free, to expand much more than it can stretch within the elastic limit. (The coefficient of expansion of steel is 0.000075° F., and it is evident that the expansion due to 250° F. is about equal to the extension of gun steel at its elastic limit.) The free expansion being prevented by the cold outer metal, the metal at the bore is crushed. When it cools it is held in a state of tension by the outer metal. The

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surface more and more assumes a state of circumferential tension, and finally yields by developing cracks (heat cracks) for the most part longitudinal.

Inspection of rings cut from the bore of guns shows that the heat cracks are of little importance at the muzzle of the gun, and in general are most pronounced somewhat beyond the origin of rifling. It is likely that the greater erosion at the origin prevents the growth of the cracks to the extent to which they are found farther forward.

It is evident that heat cracks weaken the tube, but the extent of this weakness is not known. Thus far they have *not* been considered the cause of serious weakness, especially in guns of two or more layers. Investigations are still being made as to the maximum depth of cracks, their extent, and the increase of these cracks with repeated firings.

It is evident that heat cracks are a source of weakness in permitting ruptures to occur, but all guns that have been fired to any extent show heat cracks, and on the whole it cannot be said that their presence is a serious menace to the strength of the gun, especially in guns of two or more layers throughout.

Thus, the hardening of the bore is considered a cause of heat cracks rather than an important cause of erosion. Occurring as they do, principally in the grooves, they can hardly be called a serious cause of erosion in which the metal of the bore is worn away fastest on the lands.

Prof. Fay calls attention to the fact that this surface hardening consists of troostite and martensite. It is known that such structure can be caused by quenching and cold-working. For instance, the tires on locomotive driving wheels are often found pitted with martensitic structure as a result of the wheel sliding on the track due to an application of the brakes.

In the case of locomotive wheels, this structure causes hair cracks to proceed into the tire, oftentimes resulting in the cracking of such tire. The Navy Department has never heard of a case where this structure was worn away faster than the remaining surface. It does not show as pit marks, but is only noticed after microscopic examination, and shows up best after being etched. The bore of a gun is subjected to somewhat the working of a locomotive tire, and therefore the similar structure can easily be explained as cold working.

What is the reason that this hardened martensitic surface is, or should be, worn away faster than other parts of a gun? If the carbon is segregated close to the surface it would reduce the melting temperature, and therefore permit a great amount of molten metal to be scoured away by the rushing of the gases. It is doubtful if such segregation of carbon occurs, the only excess of carbon being absorbed from the excess of carbon in the powder gas. According to analyses of powder gases, there is very little excess carbon, and certainly not sufficient to account for this theory.

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As a result of erosion tests covering a number of years, it has been most noticeable that the greater amount of alloy a metal contains the greater the erosive effect of the gases upon it. Metals have been tested that contain as high as 43 per cent. of nickel and 14 per cent. of tungsten, and such metals are the worst that have been tested. Wrought iron or mild steel (0.100 carbon) have given the best results. These results might confirm the fact that martensitic structure or hardening of the surface is the cause of erosion, for it is practically impossible to turn wrought iron or mild steel into troostite or martensite.

A series of experiments is now being carried on with erosion plugs that are not quenched but are annealed above the Ac_3 point. It is believed that such specimens being absolutely free from troostite or martensite will have less erosion than specimens that have been quenched. This point was brought forth by the fact that certain quenched specimens that had been drawn at exceedingly high temperatures showed up somewhat better than similar specimens drawn at a much lower temperature.

It appears, therefore, that the heat cracks are not of very great importance in relation to their effect upon the erosion of guns, and they have not progressed deeply enough into a forging to be considered serious as far as the strength of the gun is concerned. Before this point is reached, the erosion has generally made the gun useless.

The Navy considers erosion in guns as being of two kinds—that caused by the escape of gas around the projectile, and that due to the rush of gas behind the projectile. As both kinds manifest themselves by a fairly uniform wear of the bore, it is not possible to state their relative value.

The necessary conditions for producing erosion are the heating of a thin film of metal together with the rush of the powder products, over this heated surface. In a given time greatest erosion will occur in the gun where these two factors have their greatest erosive relation. Erosion will occur with high temperature of metal and relatively low velocity of gas. It likewise occurs with high velocity of gas and low temperature of metal, as is shown by the erosion at the muzzle; but in all cases to produce any appreciable erosion a thin film of the metal of the bore must be heated and the products of combustion must pass over this film at considerable velocity. Friction is necessary to produce erosion—there is no erosion in closed bombs in which both the pressure and the temperature are the same as in guns—there must be motion of the products of combustion. There must be a friction, or cutting, or washing away of the weakened film of metal. In addition to the friction of the products of combustion there is the friction of the rotating band, and this is a considerable part of the total friction. The time element is of great importance, for the quantity of metal washed away increases with the time of action.

The greatest erosion occurs at the origin of rifling, where the relation between the time, temperature of the bore, and the velocity of the gas

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(aided by the friction of engraving the band) seems to be at a maximum for producing erosion.

The time taken by the projectile to travel one caliber down the bore is about 50 per cent. of the total time of travel from origin of rifling to the muzzle. It is evident, therefore, that as the bore near the origin is very much longer exposed to the destructive effects of the products of combustion greatest erosion is to be expected in this part of the gun.

Erosion is serious because it gradually wears away the rifling and enlarges the bore near the origin. This results in a reduction of both pressure and velocity with the ultimate result that the projectiles do not get proper rotation and hence give inaccurate flight.

Having been recognized for more than 30 years, a large number of investigations and experiments have been made in an attempt to prevent the erosion. The methods utilized and the results and conclusions reached thereby are beyond the scope of this paper, which is intended solely as a discussion of Prof. Fay's paper.

It is sufficient to remark that while the evil effects of erosion have been nullified to a great extent by the Navy, no method to prevent its occurrence has yet been developed. The design of the gun in particular reference to its powder chamber dimensions has much to do with the rapidity of erosion. One of the recent high-powered navy guns of 14 in. in caliber has already been fired one and six-tenths times the number of rounds that its recognized life permits, and does not show appreciable loss in velocity or accuracy.

In conclusion, it is to be hoped that Prof. Fay will continue his researches and eventually reach a conclusion as to the relation between the hardening of the surface and the erosion of the guns, and point out a new direction of research for the Navy to pursue in its endeavor to lessen this phenomenon.

HENRY FAY (communication to the Secretary*).—In reply to the discussion of my paper, I wish to take this opportunity to express my appreciation to the Chairman and the various gentlemen who have taken part. I regret that others have not offered criticism, for honest differences of opinion often lead to the truth, and suggest new lines of work.

An experiment has been made which bears on Prof. Sauveur's discussion and which helps to emphasize the conclusion that cementation is not a factor in the hardening of the surface. Dr. Langenberg, of the Watertown Arsenal, has heated for me, *in vacuo* at 1,000° C., a piece of the metal containing a hard surface. The microstructure shows uniform distribution of carbon throughout the metal, showing that no carbon has been added to the surface and heating *in vacuo* has prevented any oxidation of the surface.

* Received Apr. 24, 1917.

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Notes on the Heat Treatment of High-Speed Steel Tools

BY A. E. BELLIS,* S. B., SPRINGFIELD, MASS., AND T. W. HARDY,† S. B., NEW GLASGOW, N. S.

(New York Meeting, February, 1917)

THE problem of heat treating high-speed steel becomes more and more important as the design of cutters becomes more and more complicated in increasing the efficiency of mechanical operations. Hundreds of dollars are spent in the design and manufacture of milling cutters of special form for rapid production of duplicate or interchangeable parts, and then, as the heat treatment is faulty on the one hand, or scientifically executed on the other, the tool fails after a few operations, or its efficiency is greatly increased. The practical operation of giving these complicated tools the right temperature necessary to bring out the best cutting qualities, and at the same time bring the tool out "clean," is a difficult one and calls for no small amount of skill. In order to be on the safe side the average tool hardener uses a temperature much too low to give the best results with the high-speed steel he uses. In the case of cutters which are finished to a given diameter before hardening, it is impossible to grind the tool after hardening, so that it is essential that the surface be protected from oxidizing or de-carbonizing. It is the aim of this paper to describe some experiments on hardening high-speed steel, in which metallographic means were used to determine the correct hardening temperatures.

The accompanying table and plates show the results of hardening experiments on five different high-speed steels. The analyses for carbon, tungsten, chromium and vanadium of each steel are given in the accompanying table. Six specimens from the same bar of each kind of steel

Analyses of High-Speed Steels Used in Hardening Tests

	C. Per Cent.	W. Per Cent.	Cr. Per Cent.	V. Per Cent.
A	0.58	17.4	3.11	1.14
B	0.60	13.3	3.32	3.58
C	0.53	13.0	4.69	2.45
D	0.75	17.7	3.30	0.85
E	0.60	16.5	3.55	0.70

* Metallurgist, New England Westinghouse Co.
† Metallurgist, Nova Scotia Steel & Coal Co.

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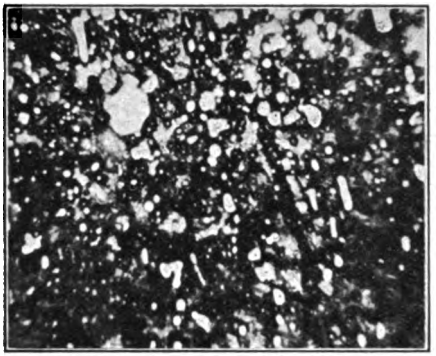
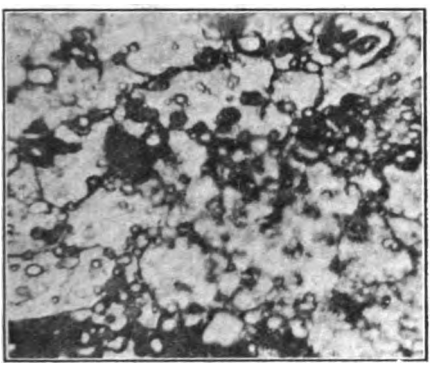
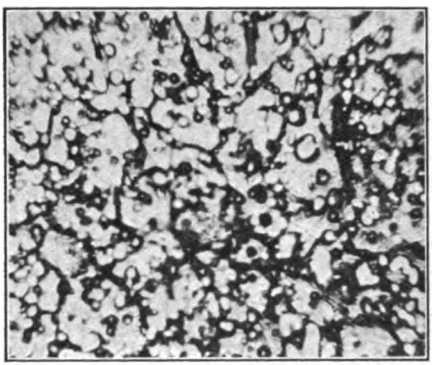
2,050° F.



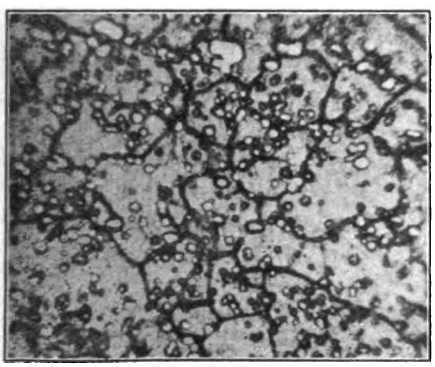
2,100° F.



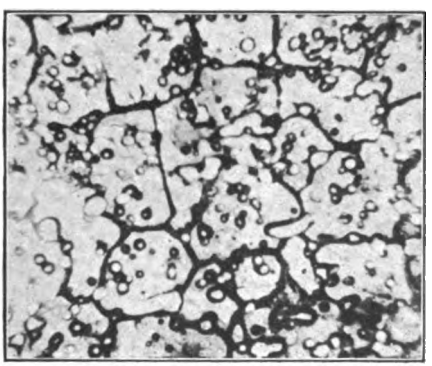
2,200° F.



2,250° F.



2,300° F.



2,350° F.

PLATE A.—STEEL *A* HARDENED AT DIFFERENT TEMPERATURES.

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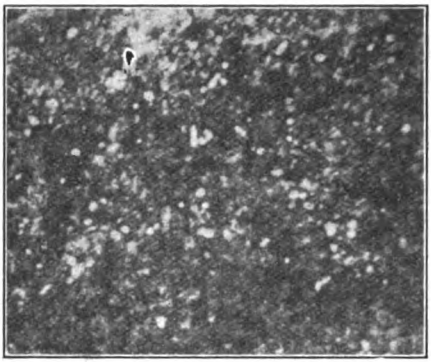
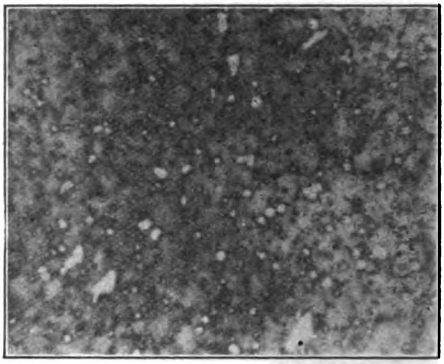
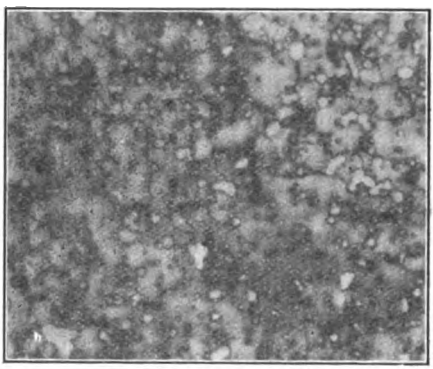


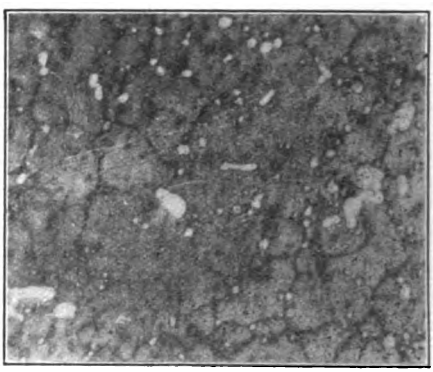
2,050° F.



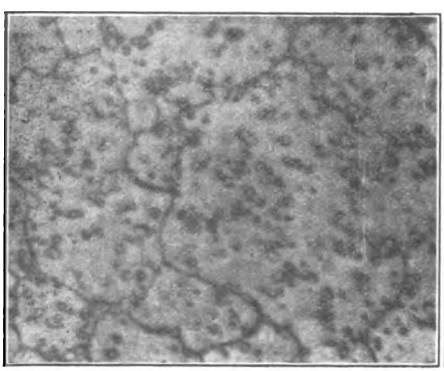
2,150° F.



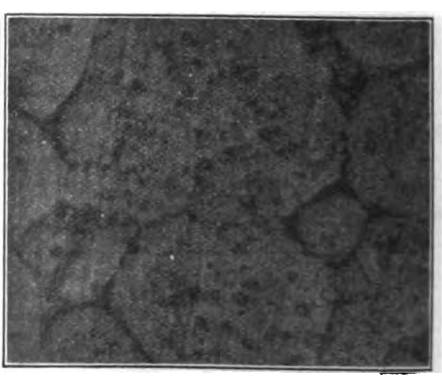
2,200° F.



2,250° F.



2,300° F.



2,350° F.

PLATE D.—STEEL *D* HARDENED AT DIFFERENT TEMPERATURES.

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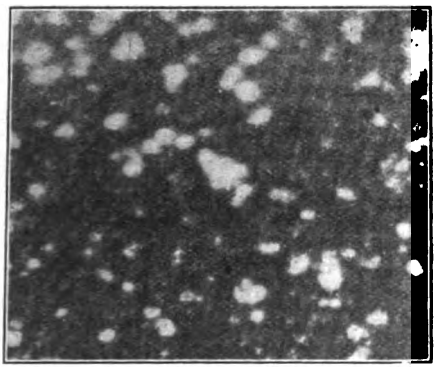
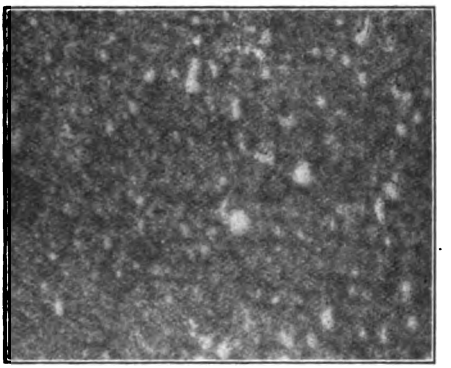
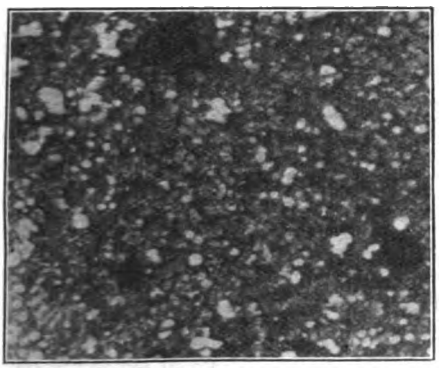


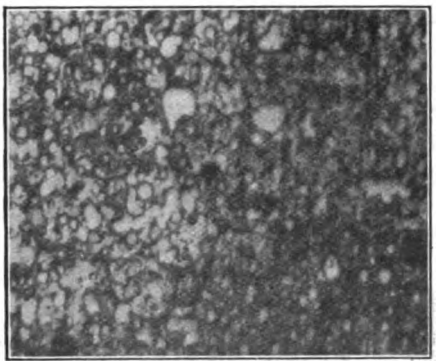
2,050° F.



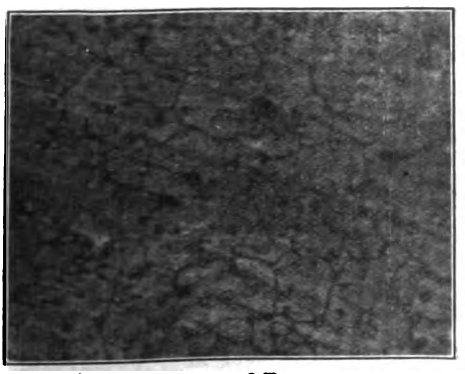
2,100° F.



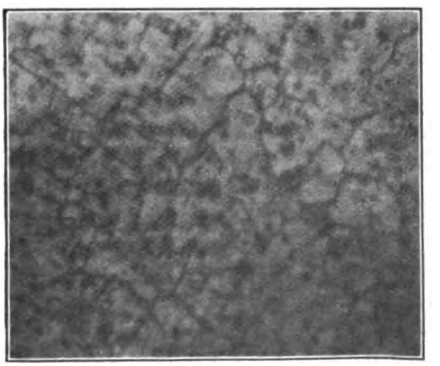
2,150° F.



2,200° F.



2,250° F.



2,300° F.

PLATE E.—STEEL *E* HARDENED AT DIFFERENT TEMPERATURES.

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were hardened from different temperatures, and photomicrographs made. The temperature from which the piece was hardened is given under the photomicrograph in each case. Thus the illustration marked A-2,100 is steel giving analysis under A, and hardened from 2,100° F. The six samples were taken from the same bar in the annealed condition regularly furnished the tool maker. Photographs were made of the longitudinal section, care being taken to grind off the outer surface. The specimens, ¼-in. square in section, were first preheated at 1,500° F., and then quickly placed in the high-speed furnace already heated to the desired temperature, left at this temperature for 1 min. and quenched in oil. The temperature was controlled by a standard pyrometer, consisting of a rare-metal couple and potentiometer. An optical pyrometer of the Holborn-Kurlbaum type gave excellent checks with the standard pyrometer, and proved more convenient and durable. A precision of 10° F. was attained throughout. The polished specimens were etched for 15 min. in 4 per cent. alcoholic solution of nitric acid, and photographed under a magnification of 1,000 diameters (750 diameters as reproduced).

In interpreting the micrographs, it is convenient to regard A-2,100 as a dark matrix showing a large excess of a free carbide; as the temperature is raised, more and more of the carbide dissolves, and the network structure of austenite is more noticeable (A-2,200). The overheated structure shows a coarse grain with black (burnt?) spots and wide intergranular spaces. We have refrained from drawing definite conclusions regarding variations of structure with analysis and hardening temperatures.

In general, the steels that show some excess carbide even at the maximum hardening heat are the most efficient. These, as a rule, are the steels with high tungsten content; they harden from a higher temperature and over a wider range than the lower-tungsten steels. For this reason they do not require as careful treatment and are, therefore, more popular than the lower-tungsten steels. The steels with lower tungsten and higher vanadium give better results when hardened at the lower temperatures than do the higher-tungsten steels when these are hardened at the same low temperatures, but the comparison is not so advantageous to the lower-tungsten steels when the steel with higher tungsten is given the proper hardening heat.

The importance of carefully controlling the hardening temperature, and of varying it for the particular steel used, cannot be overemphasized. The custom of using one "high-speed temperature" for all tools is very poor practice, for, as shown by the photomicrographs, the best structure may be obtained with one steel at a temperature which will "burn" another, or not harden a third. Thus 2,300° F. or over is necessary to give A or E a good structure, but this temperature gives a coarse grain in the other steels, or "burns" them. Again, a temperature as low as 2,150° F. can satisfactorily harden B or D, but tools made of other steels

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would not stand up if hardened at this temperature. More extreme examples could have been shown, but the samples chosen are typical of the most widely used brands of high-speed steel.

The average hardener rarely obtains the best result from his steel. The reason for this is, especially in the cases of tools that cannot be ground after hardening, that oxidation becomes a serious problem at higher temperatures. The use of a barium chloride bath to eliminate this difficulty has the disadvantage that the surface of the tool becomes decarbonized. A method that has proven satisfactory is to place the tools after preheating in the reducing atmosphere of a carbon resistance electric furnace already heated to the required temperature. The very short time necessary to get the tool to the temperature of the furnace eliminates deleterious surface effects. Pack hardening often gives good results but, owing to the great affinity of iron for carbon at high temperatures, care must be taken to reduce this carbonizing action to a minimum. This may be done by selecting a packing material of little or no carbonizing power, and by cutting down the time during which the metal is in contact with the packing material.

The increased efficiency and cutting power of tools that have received the proper heat treatment is out of all proportion to the time given to the study of the particular steel involved, and to the care given the work.

Discussion

Robert J. Anderson, Cleveland, Ohio (communication to the Secretary*).—The paper by Messrs. Bellis and Hardy was interesting to me and has led me to make a few remarks concerning some of the points brought out.

The question of employing correct hardening temperatures is one that is sadly neglected, particularly in many large plants that forge and treat their machine tools for private use. As is most aptly remarked in this paper, "The custom of using one 'high-speed temperature' for all tools is very poor practice;" still, such practice is more common than otherwise. An instance came to my attention some time ago, in connection with some work with the standardization of high-speed steels for machine tools, where there were no less than six different brands of alloy steels being used for machine tools such as lathe, shaper, planer, boring mill and cutters of like nature. All of these different steels received identical heat and forging treatment—or at least that was the aim of the tool hardener. That the results were highly unsatisfactory goes without saying. That the hardening department of many plants is conducted on a rule-of-thumb fashion is the reason for a great deal of the trouble and disappointment which seems to be the lot of many tool-steel users.

* Received Jan. 11, 1917.

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Exceedingly crude execution is the rule rather than the exception, and it is no small wonder that the results are as good as they sometimes happen to be.

It would seem that at the present price of alloy steels the subject should receive a closer scrutiny by purchasers than it does. One not familiar with the workings of some plants that disdain to lend an ear to scientific heat treatment would scarcely give credence to some of the methods employed, not only in the treatment of the steel but also in its selection and purchase.

Heating for hardening should be conducted under close pyrometric control only after the correct hardening temperature has been determined. However patent all this may be to most people, it is an exceedingly difficult matter to impress upon some, in spite of the facts of experimental evidence and those gleaned from the apparently trustworthy literature.

Relative to the matter of the decarbonizing effect of $BaCl_2$; potassium ferrocyanide is supposed to prevent decarbonization of cutting edges while heating. Just what reaction takes place here or how this happens is not known to me.

The photomicrographs show in clear-cut fashion the existence and position of the critical temperature of hardening and the importance of the same.

With the rapid progress in metallography, it is hoped that every plant either manufacturing or using high-speed steel in quantity will soon find the services of the metallographist invaluable.

F. C. LANGENBURG, Watertown, Mass. (communication to the Secretary*).—The authors of this paper have taken great care to state precisely the reagents used in etching the samples of steel described in this note. They have also stated definitely the time of etching. Anyone who has had much to do with the microstructure of high-speed steel will appreciate their care in covering this point, as it is one of the most important items in judging of the character of the structure produced by any treatment.

Several specimens have come under my observation which gave well-defined austenitic structure after a few seconds' etching, but after a longer attack the specimens resembled very much those shown on Plate D where the steel was hardened at 2,150° F., which is, evidently, not a desirable condition for a properly hardened high-speed tool.

To more clearly illustrate this point and to show also that the structure is a great help in determining the actual cutting efficiency, two photomicrographs are introduced. Both samples were etched 2 min. in a 4 per cent. alcoholic solution of HNO_3 and photographed at 500 diameters.

The specimens which these illustrations represent were taken from

* Received Jan. 30, 1917.

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twist drills that had been subjected to a competitive test. The drill having the structure shown in Fig. 1 was far more efficient than the drill having the structure shown in Fig. 2. Fig. 1 certainly shows a structure more nearly approaching austenite than Fig. 2.

If these samples had been etched 15 min. in the same reagent as above, very little difference would be shown in their structure and one might be led to believe that microscopic examination was without value in the examination of high-speed tool steel. Quantitative etching will,

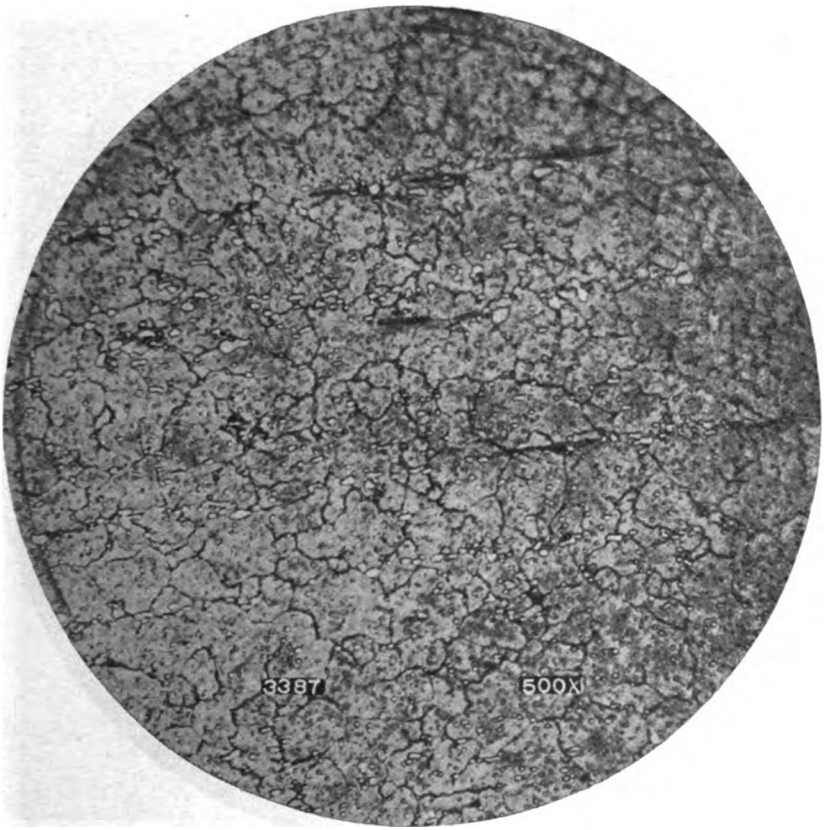

FIG. 1.—× 500.

if properly carried out, be of very great assistance in forming an opinion concerning hardening temperatures and other high-speed steel problems.

The statement made by Mr. Bellis and Mr. Hardy, that the custom of using one high-speed temperature for all tools is very poor practice, is not clearly understood. If they refer to the use of one high-speed temperature for all steels it would seem more reasonable, and it is inferred that this is their meaning. Often it is not the custom to use the same high-speed temperature on the same steel for all tools. Many hardeners

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are afraid that too high a temperature will cause cracking and checking in complicated cutters with sharp angles, and for that reason they use a lower temperature on such pieces. This practice very often arises from the fact that the management will criticise the tool hardener for cracking tools in the hardening operation, whereas a proper viewpoint would show that a few cracked tools brought about by the use of a higher hardening temperature would in the end result in great economy, as one properly hardened cutter will outlast many that are improperly hardened.

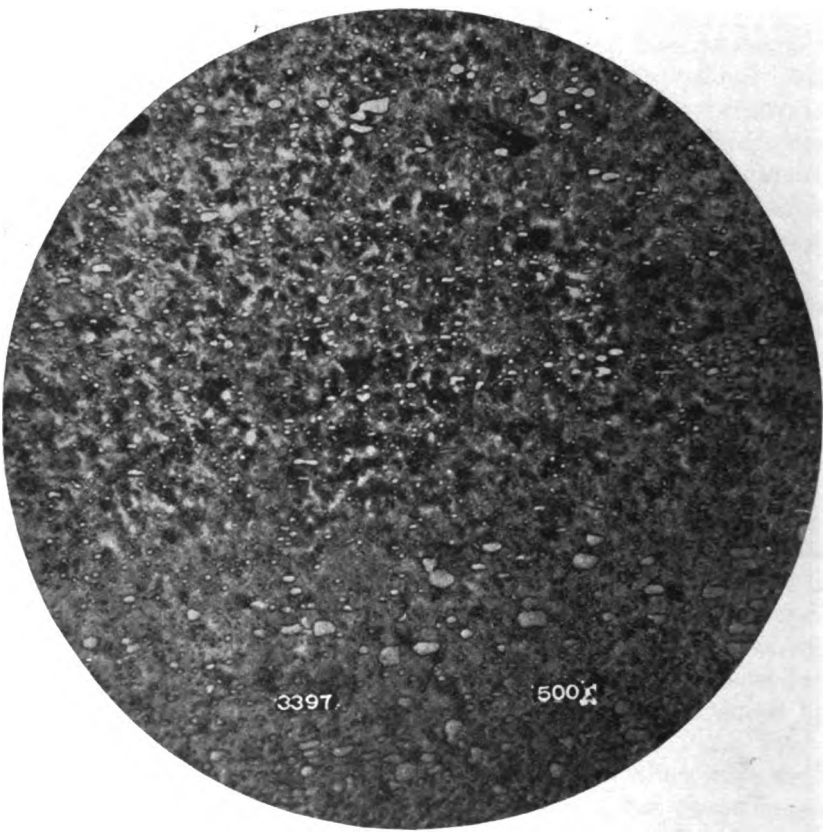

FIG. 2.—× 500.

It is not altogether clear what is meant by the term "burning" in their paper. There is introduced here a photomicrograph at 500 diameters, Fig. 3, which shows the appearance of an intergranular eutectic. This intergranular eutectic becomes evident when the hardening temperature has been too high. It is probable that the formation of this eutectic is brought about by heating above the solidus point, which may be separated by 100° C. or more from the liquidus. It does not seem

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that the eutectic formation can properly be called "burning," although its effect on the tool may be the same.

If the term "burning" is to be extended to cover all causes of intergranular brittleness, the metallurgist will lose at once a concise word with a definite meaning, and confusion will result. A number of overheated steels have come under my observation, but the intergranular brittleness was always traceable to the eutectic shown above.

FIG. 3.—× 500.

One of the most important items in tool-hardening, and an end that is fully as difficult of attainment as the proper hardening temperature, is the hardening of a tool so that the surface is not affected in composition to a depth greater than a few thousandths of an inch. If the tool is hardened in a strongly oxidizing atmosphere, it is of course necessary to grind not only to remove the scale but to remove the soft skin which results from decarburizing. If, to avoid this result, a reducing atmosphere is used, serious troubles will very likely be present, but not so

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apparent. The tool may come from the furnace perfectly bright and be soft to a depth of 1/16 in. Fig. 4 shows such a condition. When this tool was removed from the furnace it was perfectly bright and absolutely no trace of oxidation was present. Decarburization had taken place, however, to a very marked degree, as can be seen from an inspection of the edges of the tool.

The micrograph shown in Fig. 5, 50 diameters, was taken at the junction of the decarbonized edge and the inner portion of the tool not affected by reducing gases. Fig. 6, 50 diameters, shows the interior structure of

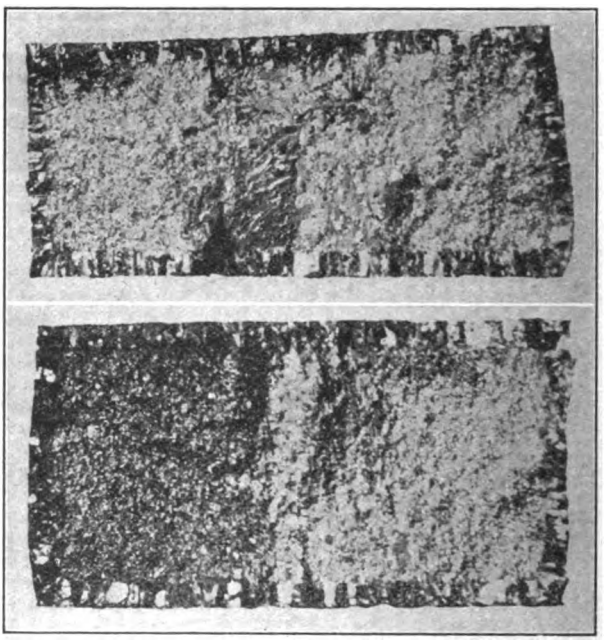

FIG. 4.—× 2½.

the same tool. The specimen from which these photographs were taken was intentionally overheated to illustrate the coarsening of the grain and the appearance of the intergranular eutectic previously discussed.

This decarburization noted above is easy of explanation. The gases in the furnace were CO and CO_2, which will, if the temperature and pressure are fixed, come to an equilibrium condition represented by $\frac{CO}{CO_2} = K$.

If a piece of steel is introduced into an atmosphere of CO and CO_2, which is under the equilibrium conditions, $\frac{CO}{CO_2} = K$, cementation or decarburization may occur. If the concentration of CO is greater than that for equilibrium, cementation will ensue, whereas if the concentration

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is less, the result will be decarburization. At elevated temperatures a very small amount of CO_2 is sufficient to make $\frac{CO}{CO_2} < K$, which will limit cementation or cause decarburization.

The authors of the paper under discussion say that their hardening was done in a reducing atmosphere of a carbon resistance electric furnace. The principal advantage to be obtained from such a furnace is that the tool is not scaled, but this does not imply that serious surface softness

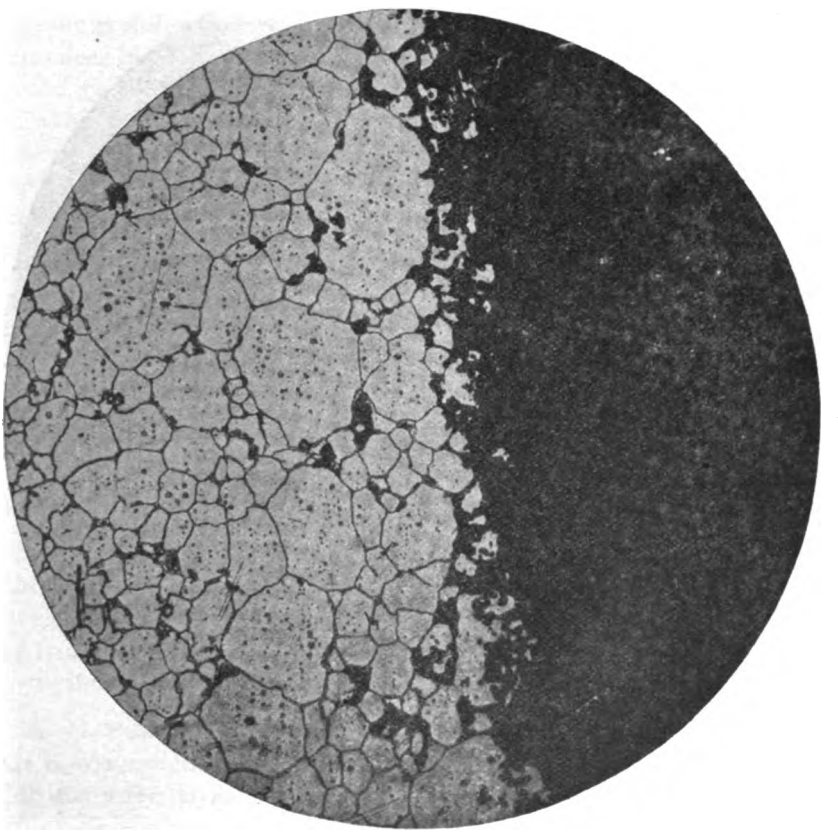

FIG. 5.—× 50.

may not result from this type of apparatus, as the illustration given above will show.

Pack hardening is never very satisfactory, due to the difficulty of adjusting the ratio $\frac{CO}{CO_2}$ so that neither decarburization nor carburization will ensue.

It is the opinion of the writer that a satisfactorily constructed oil

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or gas furnace will give good results if proper attention is given to the regulation of the air and fuel supply.

Two very important points have not been mentioned by the authors; namely, the effect of time at the hardening temperature and the subsequent tempering of hardening high-speed tools. There is a great deal of work to be done on both of these points, but it is evident that the time at the hardening temperature is of vital importance because the coarsening

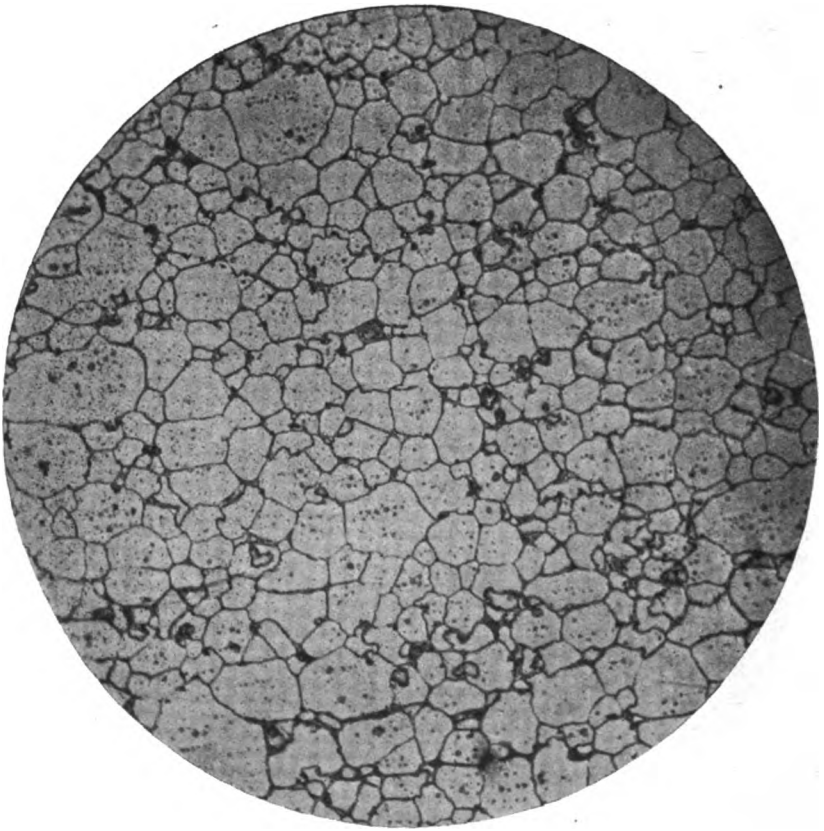

FIG. 6.—× 50.

of the grain is a function of the time as is also very probably the solution of the complex carbides.

The question of subsequent tempering of high-speed steels is of relatively great importance in certain classes of work. In heavy cuts where the tool will be tempered by the heat generated in cutting, the necessity of a second treatment is not so important. On twist drills and a great deal of light work where the tool itself will not generate enough heat to cause tempering, the previous tempering operation might be of considerable advantage as it has been shown conclusively by Edwards and

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Kikkawa,[1] that a decided increase in hardness results from tempering properly hardened high-speed steel. Whether this increased hardness will result in longer life is the question still open for investigation.

THE CHAIRMAN (ALBERT SAUVEUR, Cambridge, Mass.).—Any information likely to throw light on the constitution and proper treatment of high-speed steel in order to obtain maximum results, should surely be welcomed. All users of high-speed steel, I believe, will agree with the authors that, in order to obtain satisfactory results, it is not sufficient to heat all brands of high-speed steel to one and the same temperature; apparently very slight differences of chemical composition seem to call for marked differences in temperature. I think that all of us agree that a properly treated piece of high-speed steel should have a fine austenitic or polyhedral structure and that an appreciable quantity of free carbide may be present in the case of very high tungsten steels. What we seem to lack is a satisfactory method, quick and reliable, of ascertaining when the proper structure has been obtained. I think Mr. Bellis' paper is a step in the right direction. To the untrained eye, however, it is not an easy matter to select from some of his photomicrographs the one representing the best structure; for instance, in the case of steel A, we wonder why temperatures of 2,250 and 2,300 do not yield the same results, seeing that both structures are apparently alike, and the same might apply to some of the other samples. I have no doubt, however, that the experienced observer is capable of detecting structural differences that are not made plain here and by which he is able to select the proper quenching temperature.

COLIN G. FINK, East Orange, N. J.—On page 503 the authors say that "the use of a barium chloride bath to eliminate this difficulty has the disadvantage that the surface of the tool becomes *decarbonized*." I would like to ask whether they actually found that the surface was decarbonized, or whether they base this conclusion on softening alone?

A. E. BELLIS, Springfield, Mass.—No, we have never demonstrated that it was a decarbonization, it is just the softening of the steel. The addition of ferrocyanide to the barium chloride bath has been suggested. I think that is not altogether satisfactory, because the ferrocyanide bath changes composition very rapidly at these high temperatures, and it would be just a question of luck whether or not it had too much or too little ferrocyanide in the bath giving equilibrium with the carbon content.

COLIN G. FINK.—I would suggest that the authors take a piece of tungsten and put it into the barium chloride bath, weighing it before and after they put it in. If they want to use a barium chloride bath for

[1] C. A. Edwards and H. Kikkawa: *Journal of the Iron and Steel Institute*, vol. 92, p. 6 (1915).

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high-speed steel, it is best to saturate the bath with tungsten; they will then find that the steel is not "decarbonized."

J. A. MATHEWS, Syracuse, N. Y.—There is no doubt of the practical bearing and importance of this paper to users of high-speed steel. It is perhaps slow and expensive, making a series of photomicrographs of each steel you get in the shop, but a visual examination would be sufficient after a complete series of standard photographs had been made.

A. E. BELLIS.—It is not necessary to make photographs.

J. A. MATHEWS.—I did not get clearly which structure you consider the best one, or do you have to sacrifice sometimes the maximum cutting efficiency to preserve your clean surface?

A. E. BELLIS.—I will repeat that the sample, if not heated hot enough, will show the black background with the excess carbide spots after the 10 min. etching. The best structure is the finest austenitic network with the excess carbide. As the sample is overheated, a coarser and coarser network results; as in orthodox metallography, the "grain" is coarsened by overheating. If overheated very much, a very coarse network results and the carbide spots become blackened. The best treatment, according to our experience—it should be modified with different forms of tools—is that which gives the smallest network structure.

I would like to remark that we can take, say, four samples from each bar of, say, six bars of steel, and have them sawn, polished and viewed under the mieroscope in an hour or so; after one is experienced and has the laboratory boy do the polishing and etching, the actual work is not very great. I think the greatest advantage of the test is its simplicity.

J. A. MATHEWS.—In connection with lathe tools, it has generally been considered the best practice to bring them to a sweating heat, and that is far beyond the point at which you get your minimum grain size. Is that on the assumption that you grind away most of the metal where fusion has started in?

A. E. BELLIS.—No, there seem to me to be two problems: One is obtaining the best structure, and the other is to maintain the surface.

H. M. BOYLSTON, Cambridge, Mass.—I would like to say that I have examined some of Mr. Bellis' specimens. The structures under the microscope are clearer than his photographs show. It is much easier to judge from the actual specimens than it is from a set of photographs such as he has shown.

MARSHALL H. MEDWEDEFF, Springfield, Mass. (written discussion).—The subject of heat treatment of high-speed steel tools is of exceeding importance to the large users of such steels, and any knowledge brought to

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light as to their successful treatment would be of help in unraveling the difficulties that arise in the art. Any discussion on this subject can only be with the hope of bringing such to light.

It is difficult at the present state of our knowledge to establish a standard practice in hardening tools made from such steels. It is apparently true that every "brand" of high-speed steel has a definite maximum as well as minimum hardening temperature which will impart to it the desired qualities. At the same time, a consideration of the design of the tool, its size, and one may add the function it is to perform, are of utmost importance in gaging the temperature from which it is to be quenched. Thus, while a given brand "A" for a roughing tool or a large milling cutter will give excellent results when quenched from its maximum temperature, say 2,300° F., a form cutter of ¼-in. diameter, will be ruined by so high a heat; 2,150° F. would probably be a safe temperature, at the same time imparting to it the desired cutting qualities. The reasons for it one can perhaps attach to the very design of the cutter. In all probability the cutting edges, being relatively fine, heat up very quickly and by the time the main body of the tool is heated up to the same temperature, the edges develop a coarse structure and brittleness results. The higher the temperature, the greater the danger of overheating the edges. In a large cutter, the cutting edges, being comparatively large, are not exposed to the same danger.

The successful hardening of high-speed steel tools will depend to a great extent upon the proper manipulation in the furnace and also on the manner of quenching, *i.e.*, the position of the particular tool as it is immersed in the quenching medium, and at times upon the quenching medium itself. An ordinary cutter is not difficult to heat-treat, but it is the intricate form cutters that tax the mettle of the hardener. The writer has had occasion to examine a considerable number of cutters failing in service; some with their cutting edges either broken or worn off. In the former case the failure might be due either to overheating with its consequent coarse grain causing brittleness, or to underheating, where the brittleness is due to the excessive free carbides which were not given the opportunity to diffuse. Where the edges are worn off, the cause can easily be traced to surface decarbonization. But the hardening room is not always responsible for failure of tools. Careless adjustment in the machine, subjecting it to undue stress or careless grinding will frequently ruin a good cutter.

Uniform heating is of utmost importance, and the hardener overcomes the defects in the design of the furnace by manipulating the object heated with his tongues, so that no part be overheated. Surface decarbonization is largely prevented by properly preheating to about 1,500° F. and then placing in the high-temperature furnace, as the absorption of heat after the above temperature is reached is very rapid. The tool

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should be taken out of the high-temperature furnace as soon as the desired temperature is recorded.

The writer at one time started an investigation to determine the combined effect of time and temperature in bringing out the desired structure in high-speed steels. A number of test pieces, ¼ in. in diameter, after being preheated in an electric furnace were placed in a high-temperature furnace heated to 2,100° F. One of these was quenched as soon as it reached the furnace temperature. Three others were held for 1, 2 and 3 min. respectively after reaching the furnace temperature and then quenched. No conclusions are drawn for the present, but it appears as if a lower temperature and a longer time in the furnace produce the same results structurally as on heating to a higher temperature and allowing little or no soaking time in the furnace as soon as that temperature is reached. Thus, a sample held in the furnace for 2 min. at 2,150° F. and quenched in oil seems to have the same structure microscopically as one heated to 2,250° F. and quenched at once. From theoretical considerations this might well be expected. The object of heating such steels to high temperatures is to diffuse the carbides in the austenitic matrix, producing the well-defined network structure. The longer the metal is left in the furnace, the greater opportunity there would seem for this diffusion to take place, and the characteristic structure to appear. As the structure under these conditions appears at a lower temperature, it will necessarily be finer. But it is evident that the above procedure can only be followed on such tools as allow for regrinding after hardening, as the dangers of decarbonization are considerably increased.

The writer hoped to be able to carry out determinations of the critical temperatures of a number of high-speed steels, but other duties prevented their accomplishment. It is his conjecture that at the temperatures at which diffusion of the carbides takes place there must be a retardation in the temperature analogous to the retardation in the melting of solids. M. Yatsevitz, in Prof. Sauveur's laboratory, made a number of determinations of the critical temperatures of such steels, but his working temperatures were much too low, not exceeding 1,050° C. or 1,921° F. At those temperatures the structure of these steels do not appear to assume any definite form, being largely cementitic. The above remarks refer to the regular high-speed steels of high-tungsten content.

A few words about pack hardening high-speed tools. In the writer's opinion, pack hardening of tools made of such steels should be resorted to only in case of very delicate cutters which cannot be exposed to the hazards of the high-temperature furnace. The disadvantage of pack hardening is the danger of carbonization, unless the packing material be noncarbonizing, such as charcoal ashes. The practice of using a mixture of burned leather and charcoal is not advisable, as this in itself is an excellent carbonizing material. Charcoal is used successfully, but the

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temperature must not be over 1,900° F., as at higher temperature carbon is evidently absorbed. A carbonized pack-hardened cutter behaves like an overheated carbon steel, becoming brittle. The writer has had occasion to examine a number of such failures. Upon regrinding and hardening in the usual way they usually give excellent results.

HENRY M. HOWE, Bedford Hills, N. Y. (communication to the Secretary†).—The authors' valuable results as to the effects of the air-hardening temperature on high-speed steel may be summed up thus:

Influence of Rising Air-Hardening Temperature on High-Speed Steel

Air-hardening temperature	Microstructure			Cutting Properties
	Austenite Grains	Grain Boundaries	Free Carbide	
Too low	Small or undeveloped	Narrow	Maximum	Too soft
Intermediate, correct	Well-developed	Intermediate	Little or even none	Best
Too high	Coarse	Wide, even with three-rayed stars	Minimum	Weak, brittle, crumbling

Here the suggestion is that as the air-hardening temperature rises it causes two prominent effects, one helpful and the other harmful, the increase of the former exceeding that of the latter up to a certain point, at which the steel is most useful, and then in turn being outrun by it. The first is the progressive dissolving of the free carbide in the austenite, increasing the stability of the red-hard state. The second is the progressive coarsening of the grains, and thickening of the intergranular cement, developing at last into spandril-like three-rayed stars of the 1,288° specimen (2,350° F.) of Plate *D*, thus giving clear proof of incipient fusion, that is of crossing the solidus. This causes intergranular weakness. The individual grains may have great red-hardness, but they do not cohere. They approach the condition of a bag of emery grains.

May we not sum this up by saying that we should, by raising the temperature, increase the red-hardening effect of dissolving the carbide, as far as we can without causing undue intergranular brittleness through the simultaneous grain coarsening, and the thickening of the intergranular cement?

† Received Apr. 29, 1917.

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A. E. Bellis (communication to the Secretary*).—The valuable discussion of our paper has brought out many interesting points regarding the heat treatment of high-speed steel. In reviewing it we feel that we should have been more emphatic in our general conclusion that the best performance results from tools having a fine-grained austenitic structure obtained in the shortest time possible. In cases of cutters where the surface must be preserved, the reducing atmosphere of a gas or electric furnace is more reliable than packing or the use of salt baths. The tools should be quenched the instant they reach the desired temperature. The effect of time on the coarsening of the grain is illustrated by two microphotographs presented herewith.

Cutters that require great strength, even at the expense of hardness, such as small profiling tools and saws, should be given the lowest possible

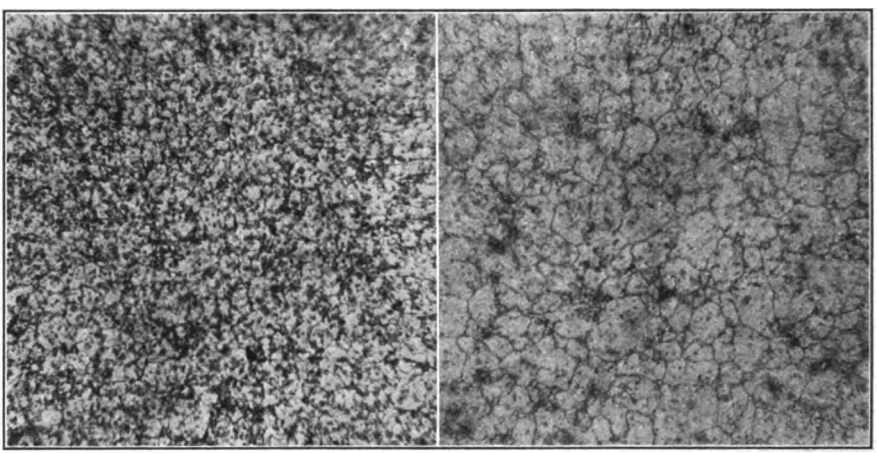

Fig. *A*.—In Furnace at Proper Temperature, 1 Min. × 560.

Fig. *B*.—Same Steel as Fig. *A* in Furnace at Same Temperature, But Left There for 15 Min. ×560.

hardening temperature and finest possible austenitic grain. Cutters that have given phenomenal life, on inspection after failure, have shown an infinitesimally small network, which was recognized as austenite only by the brightness of the specimen after etching.

Tools that have been tempered give a gray or black "background" after a short etching, even if the structure is austenitic; the higher the drawing temperature, and the longer the etching, the darker the "background." It is especially desirable, therefore, to use the method of "quantitative etching" advised by Mr. Langenberg in examining tools after the tempering. But when we come to measuring degrees of color and seconds of time we approach the borderland of science and assume the prerogatives of artists in heat treatment.

* Received Apr. 18, 1917.

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Effect of Time in Reheating Hardened Steel below the Critical Range

BY CARLE R. HAYWARD,* S. B., AND S. S. RAYMOND,† M. S., CAMBRIDGE, MASS.

(New York Meeting, February, 1917)

IN reheating quenched steel to remove part of the hardness, the softening effect has generally been considered to be a function of temperature and time. The temperature effect is well known, and long before pyrometers were heard of the blacksmith was able to do a good job of tempering, by rule-of-thumb methods and experience in judging temperatures.

Modern conditions, which demand steel that will withstand the severest tests, require that the heat treatment be carried out with every possible precaution to secure the best results obtainable. Pyrometers and heat-treatment furnaces of many types are on the market and improvements are continually being made so that it is now possible to regulate the temperature in heat treatment very accurately. There is still a question regarding the time that the steel should be subjected to treatment. It is customary with all operators to insure the desired temperature throughout the specimen and it is generally supposed that a longer treatment of a hardened steel produces a further softening but there are few published figures showing the exact effect of time of treatment at constant temperature.

A search of the literature disclosed several brief statements that the tempering effect was a function of both time and temperature, but in no case were any figures given. The opinion is very generally held that the well-known tempering colors are accurate indicators of the degree of tempering. This is disputed by Brearley (*The Heat Treatment of Tool Steel*, page 102) who states:

> It has been said that the result would be the same in respect to both hardness and other properties whether the colors were obtained by a shorter heating at a higher temperature or a longer heating at a lower one. This, to say the least, is a very doubtful conclusion, and is certainly not borne out by mechanical tests on oil-hardened and tempered motor car steels, which, after tempering for periods varying from 15 min. to 2 hr., show no very great differences.

The present investigation had for its object the obtaining of some definite data regarding the effect of time, which might serve as a guide to those engaged in the heat treatment of steel.

The Rhode Island Tool Co. coöperated by furnishing the steel and machining the test specimens.

* Assistant Professor of Mining Engineering and Metallurgy, Massachusetts Institute of Technology.

† Graduate Student, Massachusetts Institute of Technology.

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Steel Used

The steel furnished came from three rolled bars $1\frac{3}{16}$ in. in diameter. The analyses are given in Table 1.

Table 1

Mark	C	Si	S	Mn	P
	Per Cent.	Per Cent.	Per Cent.	Per Cent.	Per Cent.
C	0.45	0.03	0.049	0.056	0.016
D	0.45	0.03	0.048	0.053	0.017
F	0.44	0.03	0.049	0.053	0.013

Heat Treatment

Outline of Operations

The procedure decided upon was to heat all specimens to 800° C., quench at this temperature, and reheat them in sets of three for 15 min., 30 min., 1 hr., 2 hr. and 4 hr. at 300° C., 400° C., 500° C. and 600° C.

Preparation of Specimens

The bars were sawed into $5\frac{1}{2}$-in. lengths and marked as follows: 301, 302, 303, 304, 305, 401, 402, etc., corresponding to the five different periods of heating at each temperature. In each case there were three specimens with the same number. Three specimens marked 800 were not reheated after quenching. Before treatment, the ends of these were threaded to fit the testing machine, as they would be too hard to thread after treatment.

Furnace

The furnace used was made in the metallurgical laboratory of the Massachusetts Institute of Technology and has been described and illustrated in another paper.[1] The heating chamber is an alundum muffle 16 in. long, 9 in. wide and 2 in. high, wound with No. 15 excello resistance wire. The muffle is surrounded by 2 in. of powdered magnesia inclosed in an outside case of galvanized iron. The specimens were supported midway between the top and bottom of the muffle by an asbestos rack. Temperatures were measured by a platinum platinum-rhodium thermocouple connected to a Siemens & Halske recording galvanometer.

Heat Treatment

The procedure in heat treatment was as follows: The furnace was heated to 800° C. and nine specimens were introduced, which caused the

[1] Carle R. Hayward: The Effect of Sulphur on Low-carbon Steel. See p. 535.

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temperature to fall to about 500°. When the temperature had again reached 800°, which took about 40 min., the current was regulated to hold the heat uniform for 5 min., after which the specimens were withdrawn and quenched in water.

To insure uniformity in quenching, each specimen was dropped into a separate pail of tap water, the temperature of which was 4° C.

The above procedure was continued until all the specimens had been hardened.

In the reheating operations, the furnace was brought to the desired temperature and nine specimens were introduced. This caused a fall in temperature and about 40 min. was required to bring it back to the desired point. When this was reached it was maintained for 1 hr., three specimens being removed after 15 min., three after 30 min. and the remaining three at the end of the hour. Six more specimens were then introduced and after the furnace had regained the desired temperature the heat was kept uniform for 4 hr., three specimens being withdrawn after 2 hr. and the remaining three at the end of the 4-hr. period. When the specimens were removed, each was dropped into a pail of water at 4° C.

The above procedure was carried out for 300°, 400°, 500° and 600°.

Mechanical Testing

After a ½-in. length had been sawed from the end of each piece for microscopic examination, the heat-treated specimens were turned into

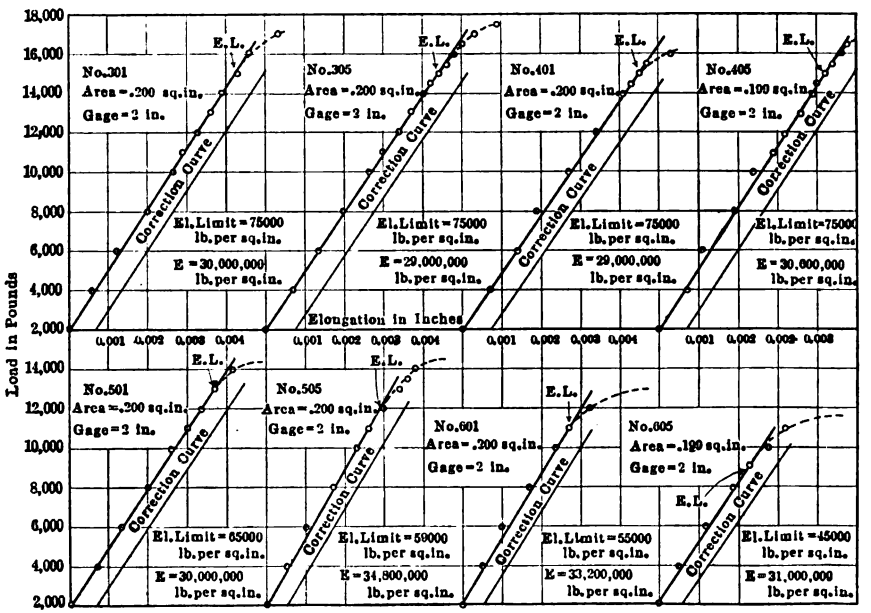

FIG. 1.—DIAGRAM SHOWING THE ELASTIC LIMIT.

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standard tensile specimens 0.505 in. in diameter and 2-in. gage length with threaded ends.

The tensile tests were made on an Olsen machine in the Mechanical Testing Laboratory of the Massachusetts Institute of Technology. A Berry strain gage was used for determining the elongation under increasing load, but because of the time required and the large number of specimens to be tested this instrument was used only on one specimen heated for 15 min. and one for 4 hr. at each temperature. In the remainder of the specimens the yield point was determined by the drop of the balance arm.

Table 2 gives the elongations recorded by the Berry strain gage for the different loads applied. Specimens 301 and 305 are those heated to 300° C. for 15 min. and 4 hr. respectively; 401 and 405 those heated to 400° for 15 min. and 4 hr. respectively. Similarly, the 501 and 505 specimens are the 500° heats and 601 and 605 the 600° heats.

The results in Table 2 are plotted in the curves shown in Fig. 1.

TABLE 2.—*Results of the Elastic-Limit Tests*

Load in Pounds	Elongation in Inches							
	301	305	401	405	501	505	601	605
2,000	0	0	0	0	0	0	0	0
4,000	0.00060	0.00070	0.0007	0.0007	0.0007	0.0005	0.0005	0.0005
6,000	0.00120	0.00134	0.0014	0.0011	0.0013	0.0010	0.0010	0.0012
8,000	0.00200	0.00200	0.0019	0.0019	0.0020	0.0017	0.0017	0.0019
10,000	0.00264	0.00264	0.0027	0.0024	0.0026	0.0023	0.0023	0.0028
11,000	0.00290	0.00300	.....	0.0029	0.0030	0.0026	0.0027	0.0032
12,000	0.00328	0.00340	0.0034	0.0032	0.0034	0.0030	0.0032	0.0140
13,000	0.00360	0.00370	.....	0.0036	0.0037	0.0034	0.0178	
14,000	0.00390	0.00400	0.0041	0.0039	0.0042	0.0038		
14,500	.....	0.00420	0.0043	0.0040	0.0173	0.0226		
15,000	0.00430	0.00440	0.0045	0.0042				
15,500	.....	0.00460	0.0047	0.0044				
16,000	0.00460	0.00480	0.0053	0.0046				
16,500	.....	0.00500	.....	0.0048				
17,000	0.00530	0.00530	.....	0.0222				
17,500	.....	0.00590						

The figures obtained for the yield point, breaking load, elongation and reduction of area for all the specimens are given in Table 3. The first figure in each number denotes the temperature of reheating, 300°, 400°, 500° and 600°, while the last figure represents the period of heating: 1 = 15 min., 2 = 30 min., 3 = 1 hr., 4 = 2 hr. and 5 = 4 hr. The specimens numbered 800 were quenched at 800° and not reheated. Those marked 000 are from the original untreated bars.

The average results in Table 3 are plotted in Fig. 2.

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TABLE 3.—*Results of Tensile Tests*

No.	Load, Pounds per Square Inch		Elongation of Gage, Per Cent.	Reduction of Area, Per Cent.	No.	Load, Pounds per Square Inch		Elongation of Gage, Per Cent.	Reduction of Area, Per Cent.
	Yield Point	Ultimate				Yield Point	Ultimate		
301	100,400	128,900	18.5	57.8	501	79,140	110,000	22.0	59.3
301	92,500	126,400	20.0	56.0	501	79,470	115,600	24.0	60.2
301	92,500	128,350	20.0	55.5	501	76,500	116,600	22.5	60.5
Av...	92,500	127,900	19.5	56.4	Av...	78,370	114,100	22.8	60.0
302	93,700	130,000	19.5	55.6	502	76,850	111,500	22.5	60.5
302	92,500	126,500	18.5	55.5	502	78,640	110,500	20.5	60.3
302	91,200	117,300	18.0	54.8	502	76,300	112,500	22.0	61.2
Av...	92,470	124,600	18.7	55.3	Av...	77,260	111,500	23.0	60.7
303	91,000	126,500	19.5	58.0	503	71,600	108,500	24.0	61.0
303	92,250	116,800	19.0	56.3	503	78,040	117,600	22.5	61.3
303	93,800	128,200	19.0	55.3	503	77,375	108,300	22.0	60.2
Av...	92,350	123,800	19.2	56.5	Av...	75,670	111,500	22.7	60.8
304	91,000	128,000	24.0	55.5	504	75,250	118,700	23.0	60.6
304	91,750	110,800	18.5	58.3	504	75,000	105,200	24.5	61.2
304	92,500	128,000	20.5	59.8	504	75,100	106,800	23.0	59.9
Av...	91,750	122,200	21.0	57.9	Av...	75,120	110,200	23.5	60.6
305	95,250	127,500	20.5	55.5	505	79,750	107,400	24.0	60.3
305	94,200	123,800	19.5	58.8	505	77,490	106,000	23.5	60.3
305	.....	127,500	20.5	56.5	505	80,000	102,400	24.0	60.8
Av...	94,720	126,200	20.0	56.9	Av...	79,080	105,300	23.8	60.5
401	89,400	118,250	21.5	60.0	601	65,177	96,000	26.5	64.8
401	85,700	118,500	21.5	59.8	601	75,205	96,900	24.5	59.4
401	87,500	116,500	21.0	58.5	601	80,000	95,000	25.0	61.0
Av...	87,510	117,800	21.3	59.4	Av...	70,190	96,000	25.3	61.7
402	87,500	117,000	22.0	59.5	602	70,203	95,400	25.0	63.2
402	88,800	117,500	20.5	60.2	602	69,000	95,350	26.5	64.0
402	85,800	117,400	22.0	60.2	602	69,350	95,750	24.5	63.3
Av...	87,370	117,300	21.5	60.0	Av...	69,520	95,500	25.3	63.5
403	86,910	117,000	20.0	60.0	603	65,850	89,750	27.0	63.5
403	87,700	118,100	20.5	58.3	603	69,106	91,600	25.0	64.8
403	85,700	116,100	22.0	59.8	603	68,180	90,300	26.0	63.6
Av...	86,770	117,100	20.8	59.4	Av...	67,710	90,600	26.0	64.0
404	84,000	116,700	21.0	58.5	604	66,850	88,500	28.0	66.5
404	85,200	117,000	20.5	58.5	604	68,350	90,100	26.5	63.8
404	91,200	117,200	21.0	59.4	604	66,600	90,450	27.0	63.8
Av...	84,600	117,000	20.7	58.8	Av...	67,260	89,680	27.2	64.7
405	82,500	116,000	21.0	60.2	605	68,000	89,850	26.5	64.5
405	83,000	128,200	21.0	59.8	605	65,800	90,450	27.0	64.8
405	85,400	116,600	22.0	58.8	605	67,300	89,200	27.5	65.8
Av...	83,630	116,300	21.3	59.6	Av...	67,030	89,830	27.0	66.0
000	55,000	76,400	31.0	51.8	800	91,000	122,200	17.0	50.3
000	49,750	76,300	31.0	53.3	800	.....	129,500	18.0	48.7
000	52,250	76,800	31.0	52.8	800	101,300	129,000	17.0	49.7
Av...	52,330	76,500	31.0	52.6	Av...	96,150	126,900	17.3	49.6

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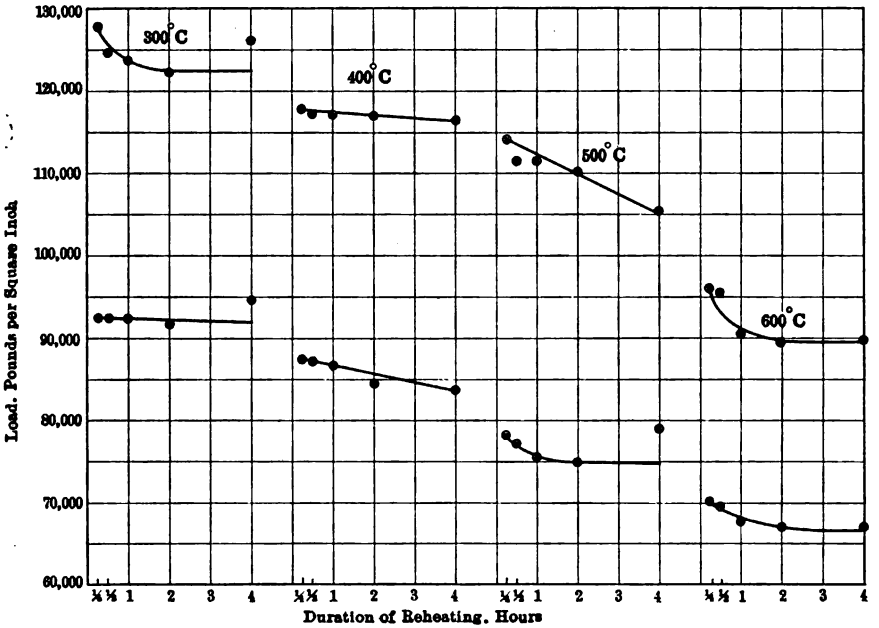

FIG. 2.—UPPER CURVES SHOW BREAKING LOAD. LOWER CURVES SHOW YIELD POINT.

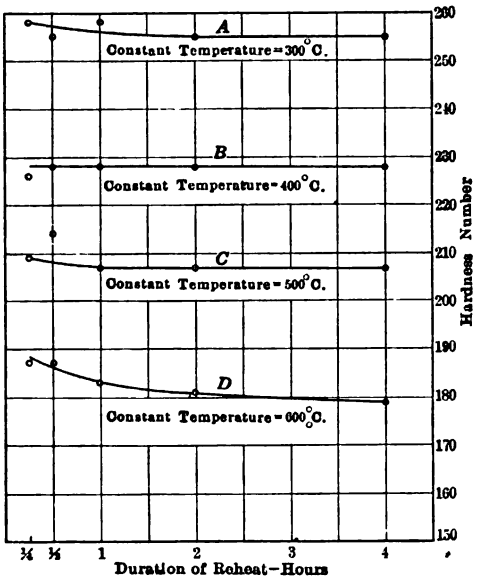

FIG. 3.

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Microscopic Examination

A piece sawed from the end of each specimen was polished and examined under the microscope. No difference could be detected between the structure of the 15-min. and 4-hr. specimens for any given temperature. A gradual change was noticed as the temperature increased from 300° to 600°, as would naturally be expected. Because of the uniformity of the photographs, they are not included in the paper.

Brinell Hardness Tests

After completing the microscopic examination, two of the polished specimens from each set were tested in an Alpha Brinell machine. The results are given in Table 4 and the averages are graphically represented by the curves in Fig. 3.

TABLE 4.—*Results of Hardness Tests*

Specimens No.	Diameter of Ball Impression (Mm.)	Hardness No.		Specimens No.	Diameter of Ball Impression (Mm.)	Hardness No.	
301	3.75	262	258	501	4.15	212	209
301	3.78	255		501	4.20	207	
302	3.80	255	255	502	4.10	217	214
302	3.80	255		502	4.15	212	
303	3.75	262	258	503	4.20	207	207
303	3.80	255		503	4.20	207	
304	3.80	255	255	504	4.20	207	207
304	3.80	255		504	4.20	207	
305	3.80	255	255	505	4.20	207	207
305	3.80	255		505	4.20	207	
401	4.05	223	226	601	4.40	187	187
401	4.00	228		601	4.40	187	
402	4.00	228	228	602	4.40	187	187
402	4.00	228		602	4.40	187	
403	4.00	228	228	603	4.45	183	183
403	4.00	228		603	4.45	183	
404	4.00	228	228	604	4.45	183	181
404	4.00	228		604	4.50	179	
405	4.00	228	228	605	4.50	179	179
405	4.00	228		605	4.50	179	
000	5.15	134		800			

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Discussion of Results

The elastic limit as determined by the Berry gage is identical for the 15-min. and 4-hr. treatments at 300° and 400°. At 500° the elastic limit of the 4-hr. specimen is about 10 per cent. lower than the 15-min. specimen, and at 600° the falling off is about 18 per cent.

The curves showing the breaking load and yield point are somewhat erratic. For some reason the breaking load in the 300° specimens fell slightly up to 2 hr. and rose again after 4 hr. Since the yield point remained constant, it is probable that further tests would have shown

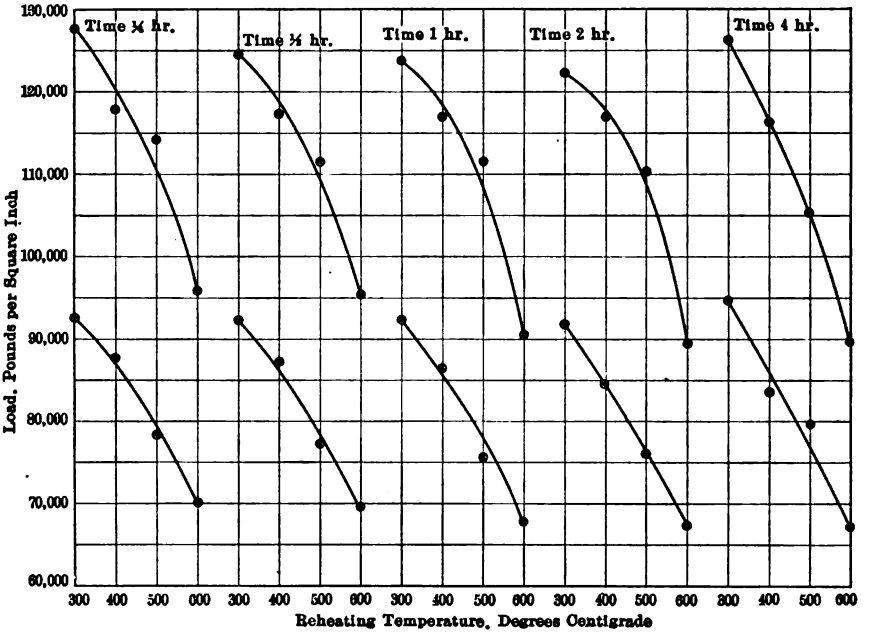

FIG. 4.—UPPER CURVES SHOW BREAKING LOAD. LOWER CURVES SHOW YIELD POINT.

that actually there is no falling off in tensile strength with increasing time of treatment. This would accord with the results obtained with the Berry gage for elastic limit.

In the 400° specimens the ultimate strength remained constant while the yield point fell off about 5 per cent. as the time increased from 15 min. to 4 hr.

In the 500° specimens there was a drop of about 8 per cent. in the ultimate load as the time increased from 15 min. to 4 hr. The yield point fell about 5 per cent. in 2 hr. and then for some unexplained reason rose again to about the original figure.

The 600° specimens showed a gradual falling off in both ultimate load

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and yield point as the time increased. The total fall in the first case was about 6 per cent. and in the second about 3 per cent.

It is not fair to consider the question settled when only three specimens were used for each treatment, but the results obtained indicate that for reheating quenched medium carbon steel to temperatures below 500° it is only necessary to heat it through. Longer heating has little or no effect on the tensile strength, ductility or hardness. At temperatures above 500° increasing the time of treatment causes a slight falling off in hardness and tensile strength with a corresponding increase in ductility.

Although not intended as a part of this investigation, the curves in Fig. 4, plotted from the data of Table 3, are of passing interest. They show that for the 15-min. heating the ultimate strength falls about 25 per cent. and the yield point about 20 per cent. as the temperature increases from 300° to 600°, and the same figures hold approximately true for the ½-hr., 1-hr. and 2-hr. treatments. Heating for 4 hr. causes the ultimate strength to fall about 30 per cent. and the yield point about 25 per cent. as the temperature increases from 300° to 600°.

It is unfortunate that the heat capacity of the furnace used was so small that the introduction of the specimens caused such a large drop in temperature and a relatively long time in bringing it back to the desired point. However, in view of the fact that time was found to be of so little importance, it is not probable that the results were influenced appreciably by the time the specimens were in the furnace below the desired temperatures.

Discussion

Carle R. Hayward.—I do not want it understood that I think that the conclusion that the time of tempering temperature is immaterial has been definitely proven, but since these are the first definite figures put out, I thought before continuing on more work ourselves, it would be well to present them as progress and for information.

J. A. Mathews, Syracuse, N. Y.—I think that if Prof. Hayward would consult the paper of Profs. Barus and Strouhal,[1] 1885, he would find about 250 pages, mostly figures, on this subject, in which they investigated the tempering effect by electrical and magnetic methods. There are so many figures in the paper that it is very difficult reading. One point particularly brought out was that even at a low temperature, such as the boiling point of alcohol, about 60° C., there is a tempering effect, increasing up to 4 hr. One reason they obtained those results, with which the present authors are at variance, was that they started with a high-carbon steel which hardens thoroughly, whereas the steel Prof.

[1] *U. S. Geological Survey, Bulletin* No. 14 (1885).

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Hayward and his collaborator used was a comparatively mild steel which does not take a great deal of hardening. About 3 years ago Mr. Stagg and I published[2] a few figures bearing on the same subject, but we used a low-carbon alloy steel of high tempering qualities and found a great deal of improvement, particularly in ductility, by continuing the drawing up to 4 hr. We used a chrome-vanadium steel showing after heat treatment 250,000 tensile strength. On short drawing, we got almost no ductility, but by continuing to 4 hr., we raised it to 12 per cent. I believe Prof. Hayward's results are due to using comparatively mild steel, whereas Prof. Barus used drill rod and carried out a wonderfully exhaustive investigation, using electrical conductivity methods for measuring the softening.

BRADLEY STOUGHTON, New York, N. Y.—No one will deny the very great scientific and practical importance of the subject treated by Prof. Hayward and Mr. Raymond. The results are astonishing to anyone familiar with the tempering and annealing of steel, but the reputation of Prof. Hayward, the confidence he expressed in the work of the graduate student working under him, and the circumstance that the paper offers an opportunity for the discussion of a very timely and important subject, decided the Committee on Publications to accept the paper. A preliminary study of the literature by the Committee, as well as the searches made by the authors, as announced on the first page of their paper, failed to disclose any definite evidence in contradiction of their conclusions. After the paper was accepted, however, time was available for a much more extensive search, which has brought out data which, if not generally known, will alone justify the publication of this paper and its presentation here.

In the appended bibliography are given articles dealing with the effect of time on the tempering of steel at various temperatures. A study of these articles throws very important light upon the conclusions of Prof. Hayward and Mr. Raymond, and we may say that they corroborate the following conclusion given near the end of the paper: "for reheating quenched medium carbon steel to temperatures below 500° it is only necessary to heat it through. Longer heating has little or no effect on the tensile strength, ductility or hardness. At temperatures above 500° increasing the time of treatment causes a slight falling off in hardness and tensile strength with a corresponding increase in ductility." This conclusion seems to be justified not only for hypoeutectoid steels investigated by these authors, but also for hypereutectoid steels. But in the last paragraphs of their paper, under the heading "Discussion of Results," the authors give other conclusions which seem to be at variance with previous investigations, and I do not think that these are warranted.

[2] *Transactions of the American Society of Mechanical Engineers*, vol. 36, p. 845 (1914).

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I do not think the conclusion is warranted that there is no important difference in the results as to elastic limit, breaking load, yield point, and hardness, between hardened steels which have been reheated for 15 min. and those which have been reheated for 4 hr. at temperatures of 300°, 400°, 500° and 600°. Nor do I think that the authors are warranted in saying, as they do in the last paragraph of their paper, that "time was found to be of so little importance, it is not probable that the results were influenced appreciably by the time the specimens were in the furnace below the desired temperatures."

The authors say that they placed nine bars in the reheating furnace, which was unfortunately of so small a capacity that the introduction of the specimens caused a large drop in temperature, and it required about 40 min. to bring it back to the desired point. It would appear from the authors' own results and from the results of previous investigators, that the heating which the steels underwent during the 40 min. while they were being heated to the desired point had an important annealing effect upon them. Therefore, when the authors heated the steel 40 min. to the desired point and then continued the heating for 15 min., instead of giving the time of heating at the desired temperature as 15 min., it would actually be 15 min. plus the equivalent at that temperature of the 40 min. required to heat the steel to the desired point. We have no way of knowing what the equivalent of that 40 min. of heating at various lower temperatures is, but we may assume it to be 10 min., 20 min., 30 min., or even 40 min. Let us for the sake of argument assume it to be equivalent to 20 min., then the reheating of the steel at the desired point would be equivalent not to 15 min., but to 35 min. Now the results of other investigators have shown that 35 min. is practically sufficient to completely temper steels of $^{13}/_{16}$-in. diameter at 300°, 400° and so on. In other words, the authors practically completed all the tempering effect they were capable of producing at that temperature on the steels when heating them a length of time which they call 15 min. It is, therefore, natural to expect that the reheating for a longer period would not have any appreciable effect, and this is in fact what is shown by the paper under discussion. The paper is important in corroborating the results of others, and in indicating that for reheating steels to temperatures below 500° it is only necessary to heat them through. This fact has been indicated by the results of previous investigators also, but the authors of this paper have added confirmatory evidence.

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H. M. BOYLSTON, Cambridge, Mass.—I would like to offer another possible explanation in part of the failure to show greater differences at the times mentioned. I would like to ask Prof. Hayward whether, when he examined, under the microscope, the steel after the first quenching, it was all martensitic in structure?

CARLE R. HAYWARD.—It was not entirely martensitic.

H. M. BOYLSTON.—Did it not show white spots on a dark background?

CARLE R. HAYWARD.—Yes.

H. M. BOYLSTON.—I thought so, because the manganese is very low, 0.056, and I feel that 800° C., the quenching temperature he used, is practically in the critical range, instead of being above it. I believe he used, not a martensitic steel to begin his tempering on, but a troostite-martensitic. He thus had less tempering to perform. It seems to me that the three explanations given all work together to account for the failure to show greater differences.

J. A. MATHEWS.—I would like to inquire of Prof. Hayward if that chemical analysis is right, if it is 0.05 manganese it must be steel of special character, because commercially they are rarely made that way.

CARLE R. HAYWARD.—I did not make that chemical analysis; it was done by the chemist and I accepted his figures as the report made.

WILLIAM CAMPBELL, New York, N. Y.—I have read this paper with a great deal of interest, and especially so because I did some work along the same lines. I started out with a steel of 45 carbon, 1 per cent. manganese, and I got the result that I expected. I had another steel of 45 carbon and 0.1 per cent. manganese, and I went at it the same way and the results were not at all what I had expected, and on making an examination under the microscope, as Mr. Boylston mentioned, I found a good deal of the ferrite was either unabsorbed or had precipitated out; in other words, the small amount, or the lack of manganese, had such a great effect on the hardening qualities of the steel. In regard to the question of heating, I mean the time that it takes to heat up the steel, I think the

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paper would be made of somewhat more value if Prof. Hayward would take a sample of the hardened material with a thermocouple in the center, and repeat the heating in order to get a time temperature curve to show the heating of the steel itself, and in that way we would be able to judge what the effect of the time heating was. I do not think that the explanation that it took so long to heat up can be the only one to give such smooth curves as are given on page 283. I would also like Prof. Hayward to add the hardening figure for the sample quenched at 800, which appears blank on page 523, because I think that would tell us also something about the amount of softening brought about.

THE CHAIRMAN (ALBERT SAUVEUR, Cambridge, Mass.).—I should like to ask Prof. Hayward if he noticed the tempering colors of the steel at different times?

CARLE R. HAYWARD.—The colors were affected because of the fact that the work was done in a muffle furnace and no attempt was made to keep the atmosphere neutral. A scale of perhaps $\frac{1}{32}$ in. was formed on the specimens; all were blue. When this scale came off, as it did in the quenching, the color underneath was usually about a straw color; it varied somewhat because part of the scale stuck and we did not take the trouble to chip it all off and study the color. I would like to add that we found it practically impossible to get the hardness with that hardened specimen; the ball on the Brinnell machine broke and we finally gave it up. It seemed to be very hard. I would like to call attention to my last paragraph, which I will read, if I may. I have recognized the fact that this heating for a long time in the furnace to bring the specimens up to temperature was unfortunate, and in doing the experiment again, I think I should do it in a little different way.

"It is unfortunate that the heat capacity of the furnace used was so small that the introduction of the specimens caused such a large drop in temperature and a relatively long time in bringing it back to the desired point. However, in view of the fact that time was found to be of so little importance, it is not probable that the results were influenced appreciably by the time the specimens were in the furnace below the desired temperature."

I think that it is very important to get the specimens heated through. I do not know how we are to determine just how long that does take. The temperature, 800°, was determined by getting a cooling curve to decide as to the upper critical point and we found that that was just below 800°, and that was the reason it was taken. We assume that, with specimens of that size, it was impossible to get a martensitic structure throughout. I recognize the fact that in the center it was troosto-martensitic; at the surface it was martensitic, and I doubt if it is possible, by ordinary water quenching, to take a specimen the size of a standard tensile specimen, and get it martensitic throughout.

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M. H. MEDWEDEFF, Springfield, Mass.—One gathers from Prof. Hayward's statements that manganese has no influence upon the time and drawing temperatures. Actual experience in large-scale production demonstrates the contrary. Manganese has indeed a decided influence on both the drawing temperature and time in the furnace. An actual case in illustration: In tempering heat-treated gun barrels, it is found that to obtain certain desired results, *i.e.*, with reference to their physical tests, a variation in the manganese content of 0.10 per cent. necessitates a change in both the time and drawing temperature. In general, for any steel the time element is of utmost importance in heat-treatment work, depending largely on size and to a considerable extent on composition.

CARLE R. HAYWARD.—Of course, I recognize that the figures do not apply to any alloy steels, but that they have to have a special heat treatment. I think it is also undoubtedly true that high-carbon steel would be affected by time, and the title of the paper was therefore not made general, but applied to the effect of time on reheating. The paper applies to medium-carbon steel, but we do not wish to make it general and covering high-carbon steel or alloys.

J. A. MATHEWS.—I am afraid that some of the users of steel will make it general and assume that the time of tempering does not make much difference. I would hate to have some of our customers do that.

O. A. KNIGHT, Cambridge, Mass. (communication to the Secretary*).—It is needless to say that with the great industrial importance of steel at the present time, and the variety of properties which may be obtained from a steel of given composition by varying the heat treatment applied, too much cannot be learned in connection wth the subject presented by Prof. Hayward. A subject so practical deserves the most careful study and research. In conducting our researches, however, we should approach as nearly as possible the conditions best suited to actual practice, and new experiments should be conducted with at least an accuracy equal to the older ones.

Let us consider briefly the method of drawing the quenched specimens that was applied in this investigation. The specimens, after having been quenched in water at 4° C., were plunged immediately into a furnace whose temperature was the maximum to be used in the drawing operation, viz., 300° C. 400° C., 500° C. and 600° C. When we consider the severely strained condition of a steel after quenching in water at 4° C. it becomes apparent that this is a more or less drastic treatment to be applied. True enough, it may produce no visible defects with pieces of uniform dimensions, such as were used in this research, but with pieces whose di-

* Received Mar. 3, 1917.

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mensions are less uniform, as is the case with many commercial articles, we might find, and in many cases do find, that the piece will crack even under the most suitable conditions. Even though no visible defects occur, the internal strains which might be set up will most certainly work to no good advantage.

From these considerations it seems as though a more suitable method would have been to place the hardened specimens in the furnace at atmospheric temperature, and bring about the desired temperature more gradually, thus easing up the internal strains already present, due to the quenching operation.

It might be argued that these experiments were carried out primarily to determine the effect of the exact time at the temperature of drawing. This being the object, it seems that a better method of procedure would have been to plunge the hardened specimens into a salt bath already at the desired temperature, and sufficiently large to insure its temperature not being appreciably lowered by introducing the specimens. This method, by agitating the specimens, would bring about the maximum temperature in a few minutes instead of 40. Portevin[1] shows that by this method of heating, the desired temperature of specimens of the diameter of those used in the present research may be obtained in a period of only a few minutes. He also gives the various salt mixtures used to make up the bath.

Another part of the paper by Prof. Hayward and Mr. Raymond to which I should like to call attention is paragraph three of the first page, the first sentence of which reads as follows:

"A search of the literature disclosed several brief statements that the tempering effect was a function of both time and temperature, but in no case were any figures given."

I would like to refer the authors of this paper to an extended research performed by Barus and Strouhal, *Bulletin* 14, *U. S. Geological Survey*, 1885, entitled "On the Physical Characteristics of Iron Carburets." They would find on page 43 a sub-title, "On the Bearing of the Time of Exposure on the Efficacy of Annealing." Under this heading vast numbers of figures are given on the effect of time of annealing at different temperatures.

These experiments were conducted with steel wire in the form of rods 30 cm. long and from 0.03 to 0.01 cm. in diameter. The steel was known as "English silver-steel." The wires were heated by being made part of an electrical circuit, and sufficient current was used to bring about the desired temperature, after which they were water-quenched by turning on a sudden flow of water, the specimens remaining in the heating device,

[1] Influence Du Temps De Chauffage Avant La Trempe. *Revue de Métallurgie*, vol. 13, pp. 9 to 63 (Jan.–Feb., 1916).

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and were surrounded by a thin glass cylinder. CO_2 gas was passed through the tube during heating to prevent excessive oxidation.

After hardening the steel, it was connected thermo-electrically to a copper wire which itself had previously been compared to that of silver. The thermocouple thus formed was the source of electromotive force subsequently determined. The thermo-electric power measured was supposed to bear a direct relation to the hardness of the sample.

After hardening the wires and making thermocouples of them as above described, their "hot junction" was inserted in the various mediums used for drawing (steam, vapor of boiling methyl alcohol, vapor of boiling analine, and molten lead) and the value of the thermo-electric constants noted from time to time. The constant whose value is recorded in the following data is the thermo-electric constant "a" of Avenarius. The "glass-hard" steel gave negative values to "a" (with one exception which was a low positive value) which approached zero and increased in positive value as the drawing temperature and time were increased, this phenomenon giving a relation for comparing the relative degrees of hardness.

The following table, taken from the paper of Barus and Strouhal, gives the variation in the value "a" of two hardened samples heated in steam at atmospheric pressure for varying lengths of time.

Sample No.	0 Hr.	1 Hr.	2 Hr.	3 Hr.	4 Hr.	5 Hr.	6 Hr.
24 — a = ...	−2.83	+0.61	1.26	1.61	1.76	1.70	1.92
25 — a = ...	+0.13	2.75	3.64	3.55	3.77	3.90	4.02

The wide difference in the two samples is clearly seen, but the relation of time is likewise apparent.

They then say: "From this grouping of parallel results, or, more evidently still, from a graphic representation (time as abscissæ, thermo-electric constant as ordinate, mean values of No. 24 and 25, Fig. 9)[2], it will be seen that hardness varies continuously with the time of exposure of the glass-hard rod to the given annealing temperature; that the amount of thermo-electric change rapidly decreases as the time increases, until finally a definite and superior limiting value is asymptotically reached."

Similar results were obtained in the other investigations. The factor *time* plays a less important part as the temperature of annealing the hardened metal increases. A fair understanding of their work may be gained from a study of the mean value of results which are given under the title of "General Discussion of the Results of This Annealing." The following is taken from page 54 of their paper. "The results thus far given adequately exhibit the general physical character of the process of

[2] *U. S. Geological Survey, Bulletin* No. 14, p. 46 (1885).

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tempering. For the sake of clearness and with a view to partially eliminating such discrepancies as are due to incidental errors, the three individual values of thermo-electric power and specific resistance for each of the temperatures of annealing will be combined and their mean chosen for discussion. We thus arrive at the following relations:

TABLE 28.—*Mean Results*

1. For annealing in vapor of boiling methyl-alcohol (66°)

Time of annealing =	0 hr.	1 hr.	2 hr.	3 hr.
a =	−1.62	−1.07	−0.80	−0.60

2. For annealing in steam (100°)

Time of annealing =	0 hr.	⅙ hr.	½ hr.	1 hr.	2 hr.	3 hr.
a =	−0.91	+0.17	0.95	1.50	2.08	2.36

3. For annealing in vapor of boiling analine (185°)

Time of annealing =	0 hr.	⅙ hr.	½ hr.	1 hr.	2 hr.	3 hr.
a =	−0.95	+4.19	4.71	5.00	5.18	5.40

4. For annealing in molten lead (330°)

Time of annealing =	0.hr.	1/60 hr.	½ hr.	½ hr.
a =	−0.37	8.11	8.26	8.30

"*Deduction.*—If these functionalities are constructed graphically, as has been done in Fig. 10 (time as abscissa, thermo-electric constant as ordinates)[1], we obtain a family of typical curves, the general character of which is distinctly pronounced, and may be thus expressed:

"The degree of hardness retained by a glass-hard rod, after having been subjected to the operation of annealing, is dependent both on the temperature to which it has been exposed and on the interval of time during which this exposure has taken place, in such a way that the effect of time, though of predominating importance in the case of small values of temperature, is more and more negligible in proportion as these values increase. The operation is always most effective in the earlier stages, and this efficiency decreases very slowly where the temperatures are low—very rapidly, indeed almost suddenly, where they are high. If the action of any temperature be indefinitely prolonged, the rod under its influence ultimately reaches an inferior and limiting degree of hardness, characteristic both of the temperature chosen and the type of steel under experiment."

Again I should like to call attention to a paper by J. A. Mathews and

[1] *Op. cit.*, p. 55.

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H. J. Stagg, Jr., published in the *American Society of Mechanical Engineers, Transactions* of 1914, the subject being "Factors in Hardening Tool Steel." On page 861 we find paragraphs 43 and 44 reading as follows:

43. "Regarding the effect of time on drawing the temper, we submit the following: Standard ½-in. round A. S. T. M. test pieces were quenched from constant temperature into the same medium, and the temper drawn in the same salt bath at constant temperature for 5 min., 15 min., etc.

Table of Results

Elastic Limit	Maximum Strength	Elongation	Reduction	Brinell Hardness	Remarks
228,750	260,137	2.5	.....	425	1,550-Oil-800° F. 8 min.
201,125	214,562	11.6	45.4	390	1,550-Oil-800° F. 20 min.
175,000	183,187	12.0	49.35	340	1,550-Oil-800° F. 40 min.

44. "Each of these results is the average of four closely agreeing checks. A study of the above table shows that time at the drawing temperature has a marked effect. The act of breaking down the martensite is progressive and not sharply defined. Both time and temperature have their effects."

That time plays a less important part as the drawing temperature is augmented is not only logical but seems already well established. The temperatures employed in the research under discussion were sufficiently high in all cases to minimize the effect of time. Undoubtedly the reason that time had no more marked effect is that owing to the temperatures employed, a great deal of tempering took place during the period of heating.

I hope that I may not be misunderstood in this discussion, as I do not advocate that further carefully conducted research along this line might not prove of value, but I hope the researches above referred to will be of benefit in correcting the erroneous statement of the authors, and also to reveal the fact that as early as 1885 it was quite definitely proven that the time factor is of minimum value in the range of tempering temperatures employed in current practice.

In conclusion, I wish to say that the points which I hope my discussion will bring out are (1) that the authors' conclusions are based upon a method which is not as accurate as methods employed by earlier investigators, and (2) their statement concerning the literature reveals the fact that they made an inadequate search.

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The Effect of Sulphur on Low-Carbon Steel

BY CARLE R. HAYWARD,* S. B., CAMBRIDGE, MASS.

(New York Meeting, February, 1917)

SULPHUR has long been one of the banes of the steel manufacturer and often no effort and expense have been spared in order to reduce it to a small per cent. in the finished product. This condition is due to a general conviction that in many cases where steels have failed in service, sulphur has been the cause. But there has been a growing feeling in recent years that the verdict against sulphur has been unnecessarily severe. In cases of segregation it was present in augmented amounts along with other impurities, but it had not caused the segregation. High sulphur in pig iron is caused by poor furnace conditions and the sulphur is merely one indication of an iron that has not been properly reduced. No amount of subsequent treatment under oxidizing conditions in the open-hearth furnace can remedy the defects, although the per cent. of sulphur may be considerably reduced. In other words, the causes of bad steel can frequently be traced back to bad pig iron, and sulphur is merely one indication that the pig iron is bad. The writer recently visited a steel plant where a mass of evidence had been accumulated which substantiated this fact, and the superintendent was emphatic in stating that high sulphur was not harmful provided the steel was not otherwise poor due to insufficient reduction in the blast furnace.

The presence of a moderate amount of sulphur is desirable from the standpoint of the man who machines the steel. The low-sulphur material drags and the production of a smooth surface is very difficult. A slight increase in sulphur enables the machinist to produce a smooth surface without difficulty.

Since, therefore, such large quantities of steel are subjected to machining, it becomes highly important that the sulphur controversy should be settled, and if its presence is proved to be harmless the ban on it should be lifted.

Among the recent papers on the effect of sulphur on steel is one by Dr. J. S. Unger, Manager of the Central Research Bureau, Carnegie Steel

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Co.[1] The results of an exhaustive series of tests are given and the conclusion states: "The author does not advocate paying no attention whatever to sulphur content in steel but believes firmly that a steel containing less than 0.100 per cent. is not necessarily bad, and that it will show little, if any, difference in quality when compared with the same steel of much lower sulphur, other conditions being the same."

The present investigation was undertaken at the suggestion of A. H. Annan of the Rhode Island Tool Co., who coöperated by furnishing the steel and machining the specimens. The results are presented as a contribution to the general knowledge on the subject.

Steels Used

It was planned to use three steels of different sulphur content but with the other elements the same. The manganese was an exception, however, for with this element part is in the form of MnS existing free in the steel and the remainder is dissolved in the steel. It is evident, therefore, that the manganese should vary but that the amount in excess of MnS should be constant in the different steels.

The steels finally selected were in the form of ¾-in. round bars. Two bars of each grade were required to furnish sufficient specimens. The analyses are shown in Table 1.

TABLE 1

Mark	Carbon, Per Cent.	Total Manganese, Per Cent.	Excess Manganese, Per Cent.	Phosphorus, Per Cent.	Silicon, Per Cent.	Sulphur, Per Cent.
1	0.18	0.55	0.48	0.007	0.01	0.038
1A	0.18	0.57	0.50	0.009	0.02	0.041
2	0.17	0.67	0.52	0.008	0.01	0.086
2A	0.18	0.70	0.55	0.010	0.03	0.087
3	0.18	0.80	0.54	0.006	0.02	0.152
3A	0.17	0.80	0.55	0.011	0.03	0.148

Heat Treatment

In order to make a comparison of the steels under different conditions it was decided to heat all the specimens to a temperature just above the critical range, quench in water, and reheat different lots to 300°, 400°, 500° and 600°C. respectively. For this purpose the bars were cut into 7-in. lengths, which was sufficient for tensile specimens and specimens for microscopic examination.

The furnace used is shown in Fig. 1. The muffle is made of alundum

[1] *Iron Age*, No. 97, pp. 146–150 (1916).

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and is 2 in. high, 9 in. wide and 16 in. long. It is wound with No. 15 excello resistance wire.

In order to obtain uniform heating of the specimens, they were supported on an asbestos rack, as shown. Asbestos shields were placed on the sides and ends of the rack so that the specimens were practically in a muffle within a muffle.

A platinum platinum-rhodium thermocouple was introduced through the back of the muffle into the center of the heating chamber and connected to a Siemens and Halske recording galvanometer.

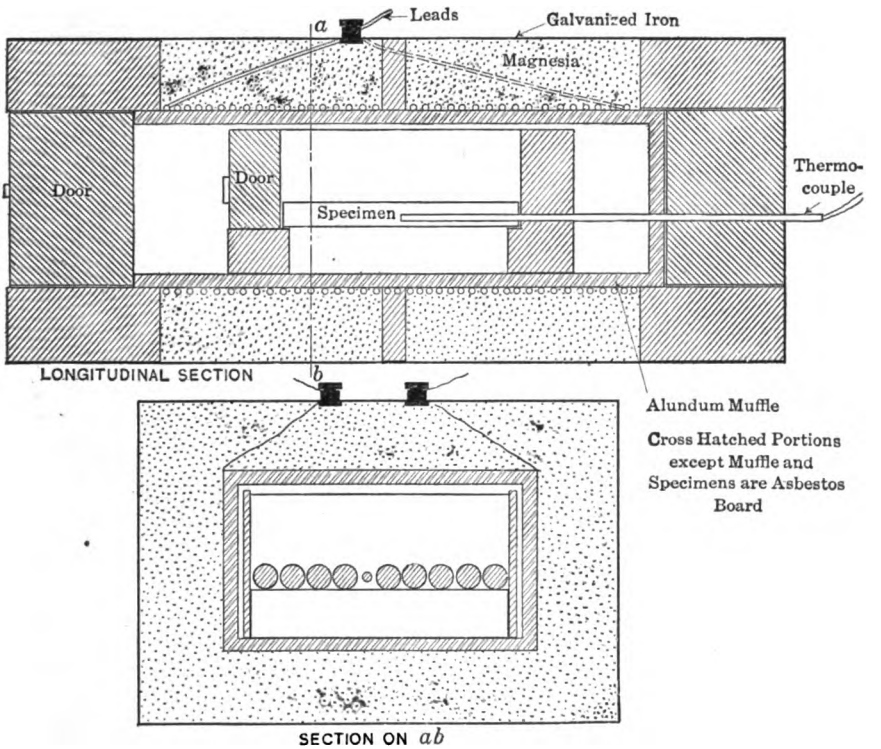

FIG. 1.—HEAT-TREATMENT FURNACE.

The procedure in heat treatment was as follows: The furnace was heated to 880° C. and nine specimens introduced. This caused the temperature to fall to 550° C. and it took about 40 min. to again reach 880°. When the latter temperature was reached it was maintained constant for 15 min., after which the specimens, except as noted below, were quenched in water and a new lot introduced into the furnace. This was continued until all but 18 of the specimens had been treated. Nine of these, consisting of three high-, three medium- and three low-sulphur steels, were removed from the furnace and allowed to cool in air. The

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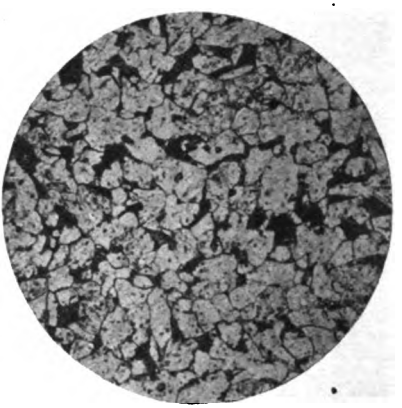
a.—High-Sulphur Steel.

b.—Low-Sulphur Steel.

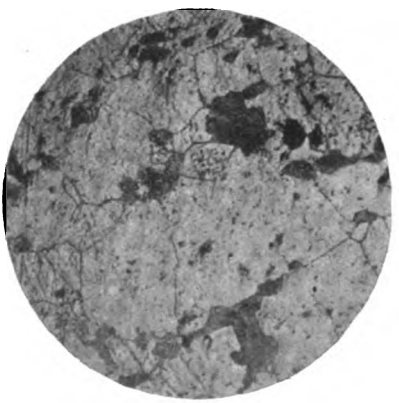
Quenched in Ice Water.

c.—High-Sulphur Steel.



d.—Low-Sulphur Steel.

Cooled in Air.



e.—High-Sulphur Steel.

f.—Low-Sulphur Steel.

Cooled in Furnace.

PLATE I.

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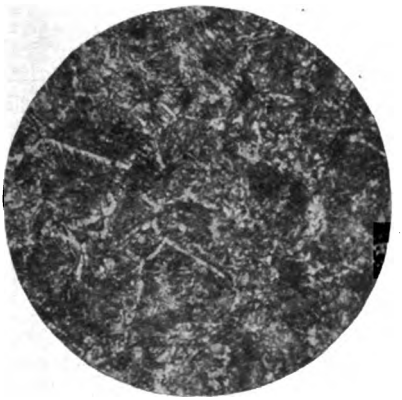

a.—High-Sulphur Steel.

b.—Low-Sulphur Steel.

Reheated to 300°.

c.—High-Sulphur Steel.

d.—Low-Sulphur Steel.

Reheated to 500°.

PLATE II.

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TABLE 2

No.	Mark		Yield Point, Lb. per Sq. In.	Breaking Load, Lb. per Sq. In.	Elongation, Per Cent. in 2 In.	Reduction of Area, Per Cent.
1	1A	Heated to 880° C. and quenched in ice water.	58,500	86,250	23.0	64.7
2	1A	Heated to 880° C. and quenched in ice water.	58,000	85,250	23.5	62.4
3	1A	Heated to 880° C. and quenched in ice water.	57,250	85,500	23.5	62.4
Average			57,900	85,600	23.3	63.1
4	2A	Heated to 880° C. and quenched in ice water.	53,000	79,750	26.0	69.3
5	2A	Heated to 880° C. and quenched in ice water.	56,250	77,000	29.5	71.4
6	2A	Heated to 880° C. and quenched in ice water.	51,500	76,250	27.5	69.3
Average			53,600	77,700	27.7	70.0
7	3A	Heated to 880° C. and quenched in ice water.	56,500	84,250	22.0	59.9
8	3A	Heated to 880° C. and quenched in ice water.	79,250*	89,250	22.5	59.9
9	3A	Heated to 880° C. and quenched in ice water.	57,300	84,500	22.0	62.4
Average			56,900	86,000	22.2	60.7
10	1A	Heated to 880° and cooled in still air.	41,000	58,500	41.0	67.0
11	1A	Heated to 880° and cooled in still air.	40,750	58,500	41.5	69.3
12	1A	Heated to 880° and cooled in still air.	41,750	58,250	41.5	67.0
Average			43,150	58,400	41.3	67.8
13	2A	Heated to 880° and cooled in still air.	45,750	58,000	41.0	67.0
14	2A	Heated to 880° and cooled in still air.	46,250	58,000	40.5	69.3
15	2A	Heated to 880° and cooled in still air.	39,750*	58,000	41.5	69.3
Average			46,000	58,000	41.3	68.5
16	3A	Heated to 880° and cooled in still air.	40,000	59,500	38.0	62.4
17	3A	Heated to 880° and cooled in still air.	41,500	59,500	39.5	64.7
18	3A	Heated to 880° and cooled in still air.	46,250	59,250	39.5	64.7
Average			42,600	59,400	39.0	63.9
19	1A	Heated to 880° and cooled in furnace.	38,000	52,500	40.5	62.4
20	1A	Heated to 880° and cooled in furnace.	37,750	52,750	40.5	64.7
21	1A	Heated to 880° and cooled in furnace.	33,750	52,500	40.0	62.4
Average			36,500	52,600	40.3	63.2
22	2A	Heated to 880° and cooled in furnace.	37,000	53,000	40.0	62.4
23	2A	Heated to 880° and cooled in furnace.	38,750	53,000	40.0	62.4
24	2A	Heated to 880° and cooled in furnace.	34,500	53,000	41.0	59.9
Average			36,750	53,000	40.3	61.6
25	3A	Heated to 880° and cooled in furnace.	37,500	55,000	37.5	59.9
26	3A	Heated to 880° and cooled in furnace.	35,500	55,000	38.5	59.9
27	3A	Heated to 880° and cooled in furnace.	32,250	55,000	38.0	69.3*
Average			35,080	55,000	38.0	63.0
301	1	Quenched at 880°. Reheated to 300°.	53,750	76,000	25.5	69.3
302	1	Quenched at 880°. Reheated to 300°.	58,000	76,000	26.0	69.3
303	1	Quenched at 880°. Reheated to 300°.	56,000	75,000	28.0	71.4
Average			55,900	75,700	26.5	70.0
304	2	Quenched at 880°. Reheated to 300°.	57,500	73,250	28.5	71.4
305	2	Quenched at 880°. Reheated to 300°.	61,250	78,000	26.5	71.4
306	2	Quenched at 880°. Reheated to 300°.	59,750	73,750	25.0	71.4
Average			59,500	75,000	26.7	71.4
307	3	Quenched at 880°. Reheated to 300°.	57,000	80,250	23.5	62.4
308	3	Quenched at 880°. Reheated to 300°.	56,500	83,000	21.5	59.9
309	3	Quenched at 880°. Reheated to 300°.	59,000	85,000	19.5	59.9
Average			57,500	82,750	21.5	60.7
401	1	Quenched at 880°. Reheated to 400°.	63,750	76,500	28.5	71.4
402	1	Quenched at 880°. Reheated to 400°.	58,500	76,000	29.5	73.5
403	1	Quenched at 880°. Reheated to 400°.	58,750	76,450	28.5	71.4
Average			60,300	76,300	28.8	72.1
404	2	Quenched at 880°. Reheated to 400°.	62,750	75,000	26.5	69.3
405	2	Quenched at 880°. Reheated to 400°.	63,750	80,000	24.0	69.3
406	2	Quenched at 880°. Reheated to 400°.	61,250	74,750	28.5	71.4
Average			62,600	76,600	26.3	70.0
407	3	Quenched at 880°. Reheated to 400°.	64,000	84,000	21.5	62.4
408	3	Quenched at 880°. Reheated to 400°.	62,000	82,750	21.5	62.4
409	3	Quenched at 880°. Reheated to 400°.	61,250	82,750	24.0	62.4
Average			62,400	83,150	22.3	62.4
501	1	Quenched at 880°. Reheated to 500°.	55,250	70,750	33.5	73.5
502	1	Quenched at 880°. Reheated to 500°.	58,250	73,250	31.5	73.5
503	1	Quenched at 880°. Reheated to 500°.	57,500	74,000	31.0	73.5
Average			57,000	72,650	32.0	73.5
504	2	Quenched at 880°. Reheated to 500°.	60,750	73,250	29.0	71.4
505	2	Quenched at 880°. Reheated to 500°.	61,250	74,000	32.0	71.4
506	2	Quenched at 880°. Reheated to 500°.	57,000	72,500	26.0	71.4
Average			59,650	73,250	29.0	71.4
507	3	Quenched at 880°. Reheated to 500°.	61,500	78,750	26.0	67.0
508	3	Quenched at 880°. Reheated to 500°.	60,500	77,500	26.0	67.0
509	3	Quenched at 880°. Reheated to 500°.	60,750	77,750	26.0	67.0
Average			60,900	78,000	26.0	67.0
601	1	Quenched at 880°. Reheated to 600°.	53,500	69,250	35.0	75.5
602	1	Quenched at 880°. Reheated to 600°.	51,000	67,750	35.0	75.5
603	1	Quenched at 880°. Reheated to 600°.	52,000	69,500	32.5	75.5
Average			52,150	68,850	34.2	75.5
604	2A	Quenched at 880°. Reheated to 600°.	54,500	70,500	35.5	73.5
605	2A	Quenched at 880°. Reheated to 600°.	52,500	72,000	32.5	73.5
606	2A	Quenched at 880°. Reheated to 600°.	53,750	70,750	31.0	73.5
Average			53,550	71,100	33.0	73.5
607	3	Quenched at 880°. Reheated to 600°.	55,000	74,250	31.0	71.4
608	3	Quenched at 880°. Reheated to 600°.	55,000	73,750	30.5	71.4
609	3	Quenched at 880°. Reheated to 600°.	55,500	75,750	30.0	69.3
Average			55,150	74,600	30.5	70.7

* Results abnormal. Not included in average.

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remaining nine, consisting of three of each sulphur content, were allowed to cool in the furnace.

In drawing the quenched specimens at the various temperatures, the procedure was as follows: The furnace was heated to 600° C. and nine specimens (three of each sulphur content) were introduced. The temperature fell and it required about 30 min. to come back to 600°. It was maintained here for 10 min. and then the specimens were withdrawn and quenched in water. The furnace was cooled to 500° C. and another set of nine specimens was introduced. When the temperature had regained 500° it was maintained constant for 10 min. and then the specimens were withdrawn and quenched. Following a similar procedure, sets of nine specimens were treated at 400° and 300° C. respectively.

Tensile Tests

After sawing off ¾ in. from each piece for microscopic examination, standard test specimens were prepared with 2-in. gage length, 0.505-in. diameter and threaded ends. These were pulled in an Olsen machine in the testing laboratory of the Massachusetts Institute of Technology.

The results obtained are given in Table 2.

For more ready comparison, the averages are retabulated in Table 3. The designations *L*, *M* and *H* refer to low, medium and high sulphur content.

TABLE 3

	Yield Point, Lb. per Sq. In.	Breaking Load, Lb. per Sq. In.	Elongation, Per Cent. in 2 In.	Reduction of Area, Per Cent.
L Heated to 880° C. and quenched in ice water.....	57,000	85,600	23.5	63.1
M Heated to 880° C. and quenched in ice water.....	53,600	77,700	27.7	70.0
H Heated to 880° C. and quenched in ice water.....	56,900	86,000	22.0	60.7
L Heated to 880° C. and cooled in still air.....	43,200	58,400	41.5	67.8
M Heated to 880° C. and cooled in still air.....	46,000	58,000	41.3	68.5
H Heated to 880° C. and cooled in still air.....	42,600	59,400	39.0	63.9
L Heated to 880° C. and cooled in furnace.....	36,500	52,600	40.5	63.2
M Heated to 880° C. and cooled in furnace.....	36,800	53,000	40.3	61.6
H Heated to 880° C. and cooled in furnace.....	35,100	55,000	38.0	63.0
L Quenched at 880° in ice water. Reheated to 300°...	55,900	75,700	26.5	70.0
M Quenched at 880° in ice water. Reheated to 300°...	59,500	75,000	26.7	71.4
H Quenched at 880° in ice water. Reheated to 300°...	57,500	82,750	21.5	60.7
L Quenched at 880° in ice water. Reheated to 400°...	60,300	76,300	28.8	72.1
M Quenched at 880° in ice water. Reheated to 400°...	62,600	76,600	26.3	70.0
H Quenched at 880° in ice water. Reheated to 400°...	62,400	83,200	22.3	62.4
L Quenched at 880° in ice water. Reheated to 500°...	57,000	72,700	32.0	73.5
M Quenched at 880° in ice water. Reheated to 500°...	59,700	73,300	29.0	71.8
H Quenched at 880° in ice water. Reheated to 500°...	60,900	78,000	26.0	67.0
L Quenched at 880° in ice water. Reheated to 600°...	52,200	68,800	34.2	75.5
M Quenched at 880° in ice water. Reheated to 600°...	53,600	71,100	33.0	73.5
H Quenched at 880° in ice water. Reheated to 600°...	55,200	74,600	30.5	70.7

There is not sufficient variation in the results to make an effective plot, but Table 4 summarizes the figures by giving the order in which they occur from high to low.

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TABLE 4

	Yield Point			Breaking Load			Elongation			Reduction of Area		
	L	*M*	*H*	*L*	*M*	*H*	*L*	*M*	*H*	*L*	*M*	*H*
Quenched in ice water..	1	3	2	2	3	1	3	1	2	3	1	2
Cooled in still air.....	2	1	3	2	3	1	1	2	3	2	1	3
Cooled in furnace	2	1	3	3	2	1	1	2	3	2	3	1
Reheated to 300°.....	3	1	2	2	3	1	3	1	2	3	1	2
Reheated to 400°.....	3	1	2	3	2	1	1	2	3	1	2	3
Reheated to 500°.....	3	2	1	3	2	1	1	2	3	1	2	3
Reheated to 600°.....	3	2	1	3	2	1	1	2	3	1	2	3
Totals.....	17	11	14	18	17	7	11	12	19	13	12	17
Order of totals.....	3	1	2	3	2	1	1	2	3	2	1	3

Photomicrographs

One specimen from each set was polished, etched in a solution of 10 per cent. HNO_3 in alcohol and examined under the microscope. The photographs show a representative spot on each specimen magnified 300 diameters.

The specimens were studied at higher magnifications than those given in the micrographs, but no further information was obtained. The lower magnifications gave a better idea of the general structure and, therefore, only these were reproduced in the paper.

Shock Tests

After concluding the tensile tests, it was thought desirable to determine the effect of varying sulphur content on specimens subjected to shock.

Through the kindness of the testing department of the Watertown Arsenal, the use of their Charpy machine was obtained for this purpose.

The remaining ¾-in. stock of bars marked 1, 2 and 3 was sawed into 2-in. lengths and the specimens subjected to heat treatment in sets of three under the same conditions that obtained in the case of the tensile specimens. After heat treatment, the specimens were machined to conform to the following specifications:

Length 55 mm., cross-section 10 by 10 mm., notch across one side, midway between the ends, 1 mm. wide. Radius of cutter edge, ⅔ mm.

The Charpy machine consists of a heavy pendulum which drops from a fixed height, strikes the specimen supported at each end and breaks

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TABLE 5

Treatment	No.	Mark	Breaking Shock, Ft.-Lb. per Sq. In.	Average
Heated to 880° and quenched in cold water.....	1	1A	494	
Heated to 880° and quenched in cold water.....	2	1A	545	
Heated to 880° and quenched in cold water.....	3	1A	540	526
Heated to 880° and quenched in cold water.....	4	2A	523	
Heated to 880° and quenched in cold water.....	5	2A	488	
Heated to 880° and quenched in cold water.....	6	2A	542	518
Heated to 880° and quenched in cold water.....	7	3A	432	
Heated to 880° and quenched in cold water.....	8	3A	507	
Heated to 880° and quenched in cold water.....	9	3A	470	470
Heated to 880° and cooled in still air.....	10	1A	356*	
Heated to 880° and cooled in still air.....	11	1A	460	
Heated to 880° and cooled in still air.....	12	1A	452	456
Heated to 880° and cooled in still air.....	13	2A	512	
Heated to 880° and cooled in still air.....	14	2A	521	
Heated to 880° and cooled in still air.....	15	2A	524	519
Heated to 880° and cooled in still air.....	16	3A	418	
Heated to 880° and cooled in still air.....	17	3A	398	
Heated to 880° and cooled in still air.....	18	3A	433	416
Heated to 880° and cooled in furnace.....	19	1A	258	
Heated to 880° and cooled in furnace.....	20	1A	284	
Heated to 880° and cooled in furnace.....	21	1A	295	279
Heated to 880° and cooled in furnace.....	22	2A	353	
Heated to 880° and cooled in furnace.....	23	2A	357	
Heated to 880° and cooled in furnace.....	24	2A	259*	355
Heated to 880° and cooled in furnace.....	25	3A	283	
Heated to 880° and cooled in furnace.....	26	3A	256	
Heated to 880° and cooled in furnace.....	27	3A	279	273
Quenched at 880°. Reheated to 300°.....	301	1A	633	
Quenched at 880°. Reheated to 300°.....	302	1A	447*	
Quenched at 880°. Reheated to 300°.....	303	1A	578	605
Quenched at 880°. Reheated to 300°.....	304	2A	562	
Quenched at 880°. Reheated to 300°.....	305	2A	537	
Quenched at 880°. Reheated to 300°.....	306	2A	530	543
Quenched at 880°. Reheated to 300°.....	307	3A	462	
Quenched at 880°. Reheated to 300°.....	308	3A	422	
Quenched at 880°. Reheated to 300°.....	309	3A	434	439
Quenched at 880°. Reheated to 400°.....	401	1A	613	
Quenched at 880°. Reheated to 400°.....	402	1A	597	
Quenched at 880°. Reheated to 400°.....	403	1A	582	597
Quenched at 880°. Reheated to 400°.....	404	2A	540	
Quenched at 880°. Reheated to 400°.....	405	2A	541	
Quenched at 880°. Reheated to 400°.....	406	2A	556	546
Quenched at 880°. Reheated to 400°.....	407	3A	474	
Quenched at 880°. Reheated to 400°.....	408	3A	447	
Quenched at 880°. Reheated to 400°.....	409	3A	429	450
Quenched at 880°. Reheated to 500°.....	501	1A	439*	
Quenched at 880°. Reheated to 500°.....	502	1A	694	
Quenched at 880°. Reheated to 500°.....	503	1A	670	682
Quenched at 880°. Reheated to 500°.....	504	2A	560	
Quenched at 880°. Reheated to 500°.....	505	2A	572	
Quenched at 880°. Reheated to 500°.....	506	2A	552	561
Quenched at 880°. Reheated to 500°.....	507	3A	495	
Quenched at 880°. Reheated to 500°.....	508	3A	456	
Quenched at 880°. Reheated to 500°.....	509	3A	469	473
Quenched at 880°. Reheated to 600°.....	601	1A	726	
Quenched at 880°. Reheated to 600°.....	602	1A	728	
Quenched at 880°. Reheated to 600°.....	603	1A	726	727
Quenched at 880°. Reheated to 600°.....	604	2A	584	
Quenched at 880°. Reheated to 600°.....	605	2A	604	
Quenched at 880°. Reheated to 600°.....	606	2A	602	597
Quenched at 880°. Reheated to 600°.....	494	3A	494	
Quenched at 880°. Reheated to 600°.....	502	3A	502	
Quenched at 880°. Reheated to 600°.....	521	3A	521	506

* Abnormal. Not included in average.

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it at the notch. The pendulum then continues its swing and the height it reaches is registered. Knowing the weight of the pendulum, the height it falls and the height it rises, a simple calculation gives the energy consumed in breaking the specimen.

The results obtained are given in Table 5.

Conclusions

Table 4, which expresses the summary of the tensile tests, shows that the high-sulphur steel has for each treatment the highest breaking load while the yield point ranks first for two treatments, second for three and third for two. From this we may conclude that the sulphur does not lower the tensile strength.

The figures for elongation and reduction of area show that there is little difference in ductility between the low- and medium-sulphur steels, but the ductility of the high-sulphur steel is slightly lower than the other two for most of the treatments.

The average figures for the shock tests, except for the air- and furnace-cooled specimens, are highest for each treatment in the case of the low-sulphur steels and lowest for each treatment for the high-sulphur steels. The widest difference appears in the steels which have been quenched and reheated.

It is difficult to draw definite conclusions from the results because of the newness of the shock test and the difference of opinion among engineers regarding its value. The tensile tests are not unfavorable to steels with moderate amounts of sulphur, while the shock tests show a decided falling off in strength as the sulphur increases. Until the interpretation of the results from the Charpy machine is more fully understood, it is impossible to say to which set of tests the most importance should be attached.

Further light might be thrown on the subject by making alternate stress or fatigue tests. It would be important to learn whether the results would confirm the tensile or shock tests. Unfortunately, however, the stock of steels used in the previous work was exhausted, and whatever the results of the fatigue tests there would be an uncertainty in their interpretation because of difference in stock. It was, therefore, decided not to include this series in the present investigation.

Discussion

The Chairman (Albert Sauveur, Boston, Mass.).—I am sure we are indebted to Prof. Hayward for his addition to our knowledge of the influence of sulphur on steel. As he has said in this paper, sulphur has had many defenders in recent years. It seems to me, however, that,

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while it may be old school to believe in the detrimental action of sulphur, it is not yet old school to believe in segregation; that while it may be that 0.1 per cent. of sulphur has no very great effect on the physical properties of steel, a ladle analysis of 0.1 per cent. of sulphur may readily result in 0.2 or 0.3 per cent. sulphur being present in the segregated portion of the ingot, and that, it seems to me, might be a rather serious matter.

J. S. UNGER, Pittsburgh, Pa.—Prof. Hayward has presented the results of an investigation along a line to which I have devoted considerable time and attention, and I am particularly pleased to note that his results are in accord with my own and further tend to prove the fact that the old established prejudice against sulphur is based more on belief than actual facts.

I have felt for years that too much stress was placed on the harmful influence of certain elements in steel such as sulphur, phosphorus, copper, etc., but it has only been within recent years that efforts have been made to establish the truth. Practically all the investigations made to date have proven that these elements within reasonable limits are harmless and, in fact, that for certain purposes the addition of these elements is beneficial.

In referring to copper, J. E. Stead, Vice-President of the British Iron and Steel Institute, said at their last meeting: "Even today one comes across steel specifications in which copper is barred, which can only be regarded as an indication of ignorance, if not stupidity, of those who prescribe the composition of steel, for it has been long ago proved that copper in steel, instead of being an evil, is quite harmless, and is sometimes distinctly beneficial."

Sulphur owes its evil name to the early days of the manufacture of steel. Chemical analyses were crude, and failure due to either poor raw materials or metallurgical treatment were many. Sulphur being a comparatively easy element to determine with reasonable accuracy, particular attention was called to the amount of sulphur, and the failure was attributed to it, without giving any consideration to other causes. A prejudice against sulphur was thus formed, which has existed to this day. As a result, the permissible sulphur content in steel is so low in some cases that it is questionable if the excessive purification necessary to produce such results is not detrimental to the steel.

It has been my experience in the working of steel, that occasionally a high-sulphur steel would roll or work much better than a lower-sulphur steel, yet the steels would be apparently the same in composition, excepting the sulphur. At other times the reverse would be true, and the lower-sulphur steel would roll better than the high-sulphur steel. If a certain amount of sulphur produces a particular effect, to be consistent it should always produce the same effect.

My investigation of the influence of sulphur in steel covered tests of

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considerable quantities of several kinds of steel in various sizes ranging from small roofing nails to 8-in. channels and railway axles, with sulphur contents of from 0.025 to 0.254 per cent.

The products tested were rivets, machine- and hand-made chains, sheets, wire, nails, tubes, pipe, drop forgings, channels, plates, axles, rails and wire cable. Owing to the variety of products investigated, it was sometimes necessary to use steel a little softer or harder than that ordinarily used for the same purpose. This, however, had no influence on the comparative results.

In the fabrication of these products, the usual works practice was followed in the production of the finished article, no preference being given in the treatment of steel of one sulphur content over that of another. The material was subjected to the tests regularly prescribed for steel products of the varieties mentioned and, in addition, special tests were made on most of the products.

It was not my object to study the state in which the sulphur existed in steel, or its microstructure. What I wanted to learn was what effect different amounts of sulphur had on the working of the steel and its effects on the properties of the finished article. This is what concerns the user of the steel.

A paper read before the Society of Automobile Engineers and published in their *Proceedings* for 1916 and in the *Iron Age*, January 13, 1916, gives the results of a great many of these tests, particularly those of interest to the automobile industry. Another paper presented before the American Boiler Manufacturers' Association and published in their *Proceedings* for 1916 and in the *Blast Furnace and Steel Plant*, July, 1916, describes an extensive series of tests on the influence of sulphur in rivet steel.

I will not review these results here, but will briefly refer to some of the tests which were not covered in these papers; such as tubes, plates, rails and wire products.

Referring first to the tests on tubes, a series of fifty 2-in. boiler tubes were made of each sulphur content, and all met the regulation hydraulic pressure test without failure. The welding practice on the tubes of 0.050 and 0.090 per cent. sulphur was better than the practice on the tubes containing 0.030 per cent. sulphur. A series of 4-in. seamless tubes were also made and subjected to different tests. The tubes of 0.068 sulphur as a whole gave better results than the 0.032 per cent. sulphur tubes. In the higher-sulphur steels there was a slight falling off in tensile strength, but an increase in ductility as shown by the elongation.

The investigation of plates consisted of an extensive series of longitudinal and transverse tensile, torsion and bend tests, welding and hot and cold flanging tests. The results of these tests were practically the same for plates of all sulphur contents.

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Steel of 0.51 per cent. carbon with varying sulphur contents was rolled into 100-lb. rails. Tensile and drop tests were made. There was a slight decrease in the tensile strength in the high-sulphur rails, but an increase in ductility as shown by the elongation in both the tensile and drop tests. Rails of each sulphur content were then put into actual service in a railroad track, and although lower in carbon than steel ordinarily used for rails, which reduced the life of the rails, all of them, regardless of the sulphur content, showed about the same amount of wear.

In the investigation of wire products, one of the studies made was the influence of sulphur in the manufacture of roofing nails with a very large, thin head. The upsetting of the head is a particularly severe operation, yet the steel of the highest-sulphur content made as perfect a head as the lower-sulphur steels.

Another study was the electrical welding of wire. When welding the wires for continuous drawing or in the manufacture of fencing, where the wires are welded at right angles to each other, it was found that steels of all sulphur contents could be perfectly welded.

In making the regulation twist and bend test of galvanized wire, the wires all met the requirements of this test, and the coating adhered perfectly, regardless of sulphur content.

The tests of wire cable showed that cables containing up to 0.10 per cent. sulphur gave practically the same results in the tensile, twist and life tests.

Attention has been called by other investigators to the fibrous condition of high-sulphur steels. This fact was particularly noticeable in our study of case-hardening. After the steels had been case-hardened and quenched, all showed the same depth of case and hardness. When fractured, the low-sulphur steels would snap off square and show a crystalline fracture, while the higher-sulphur steels, instead of snapping off square, had to be bent backward and forward before they would break, and the fracture would be irregular and have a fibrous appearance.

The results of this investigation confirmed my belief that steel could contain a great deal more sulphur than that usually permitted and still be of good quality. The work of Wahlberg, Arnold, Stead, Cooper, Hayward and other investigators has done much to strengthen this belief. There is still a large field for investigation and a much larger work in distributing the knowledge thus gained to all those interested. This seems to be the only way by which any erroneous beliefs can be replaced by actual facts, which later find a practical application.

GEORGE F. COMSTOCK, Niagara Falls, N. Y. (written discussion).—The results of tests given in Prof. Hayward's paper are interesting, and are worthy of serious attention by those who advocate an appreciable increase in the permissible limits of sulphur content in steel. It does not seem, however, that Prof. Hayward in his conclusions has rendered an entirely

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impartial verdict. The beginning of the paper would indicate that he started out with the intention of proving if possible that sulphur up to 0.1 or 0.15 per cent. does no harm to steel, and the tensile tests were extremely encouraging to such a conclusion. It should be noted, however, that the high-sulphur steels were no better on the whole in the tensile tests than the low-sulphur steels.

The impact tests, on the other hand, do show a marked difference in favor of the low-sulphur steels, but Prof. Hayward is apparently unwilling to admit in his conclusions that any particular importance attaches to this fact. The excuse is given that the shock test is new and its value not thoroughly appreciated by all engineers. This, I suppose, is a matter of opinion, but it surely would seem that a glance through the published records of technical societies interested in metals or their testing, both in this country and Europe, should be sufficient to show that the shock test is not new but is well known and extensively used. Looking at the matter from the viewpoint of the designing engineer, it might be asked how metals usually fail in service, whether by slow, gradually increasing tension, or from small repeated stresses which cause fatigue, or from sudden shocks? It seems obvious that failure from pure slow tension practically never occurs in service, and that fracture from shock is far more common, although probably not so common as failure from fatigue. When looked at in this light it is hard to see why Prof. Hayward's shock test results should not be given serious attention in judging of the practical value of high-sulphur steels.

The actual results of the work described in this paper may be summarized as follows: Tensile tests showed on the whole no difference between high-sulphur and low-sulphur steels; impact tests showed a distinct difference in favor of low-sulphur steels; microstructure not discussed. In view of this summary, where nothing is shown in favor of the high-sulphur steels, but one point of decided superiority is brought out for the low-sulphur steels, is there any logical escape from the conclusion that steel low in sulphur is better and safer to use than steel with high sulphur?

The failure of the tensile tests to show a difference between these classes of steel is very similar to its results on steels with varying amounts of phosphorus, or on steels heat-treated differently. It has been shown by very eminent authorities that steel high in phosphorus may give good tensile strength and elongation, and still show great brittleness in shock tests. This is a case directly in line with Prof. Hayward's results with sulphur, and if high-phosphorus steels are considered unsafe to use in spite of fairly good tensile results, then Prof. Hayward's paper can only be regarded as showing that high-sulphur steels are equally unsafe.

I notice that Prof. Hayward does not discuss the microstructures of these samples, although photomicrographs were made and are repro-

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duced in the paper. I would like to ask what conclusions are to be drawn from them? It always seems unfortunate to me that photomicrographs should be considered as proving anything in themselves in cases like this, for of course no two photomicrographs even of the same sample would be exactly alike. The true use of a photomicrograph is, I believe, in illustrating points that would be less clear without the actual views, and I would like to ask whether Prof. Hayward considers that these photomicrographs show the structures of high-sulphur and low-sulphur steels to be alike, or, if not, what differences are shown?

If photomicrographs had been made before the samples were etched, there is no doubt that much greater differences would have been shown in the cleanness of the metal. The excessive amount of non-metallic manganese sulphide in the high-sulphur steel could be easily seen in an unetched sample, and would illustrate well the reason why such steel is easy to machine, and also why it is brittle and unfit for severe service. Sulphide inclusions are as harmful as any other non-metallic impurities in steel, and more so than some because they tend to segregate. Unless we disagree entirely with the eminent authorities who maintain that non-metallic inclusions are harmful in steel, we cannot logically admit that high-sulphur steel is all right.

It is unfortunate that Prof. Hayward did not continue the investigation to include alternating stress tests, even if new stock would have been required. Surely he does not consider his results of no general value and inapplicable to any but the identical steels on which he worked, and it is not clear why the results of fatigue tests on high-sulphur and low-sulphur steels should have any "uncertainty in their interpretation" merely because the stock was different from that used in the impact and tensile tests. It is to be hoped that such an investigation will soon be made, in order to show, more clearly even than Prof. Hayward's present results show it, why high-sulphur steel is unsafe to use for severe service.

M. H. MEDWEDEFF, Springfield, Mass.—I recollect when Dr. Ungers' paper on the "Effect of Sulphur in Steel" appeared in the *Iron Age*, I intended to offer a criticism more with the hope of having some more competent authority challenge the rather revolutionary statements contained therein. It was, however, never published, as I hoped to secure substantiating figures in defense of the current specifications for sulphur limits.

While connected with a steel works producing merchant open-hearth bars, splitting ingots in the process of rolling were repeatedly found to be caused by the high sulphur in the steel, the other constituents being normal. The ingots at the same time appear to have a fibrous appearance. This may perhaps be ascribed to the rolling out of the sulphides into filaments. Sulphur exists in steel as MnS, or perhaps both as FeS

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and MnS, particularly when the manganese content is low. The melting points of these sulphides are considerably below the melting points of hypoeutectoid steel. Now if the steel is high in sulphur, it contains a considerable amount of MnS or MnS plus FeS; it is reasonable to suppose that as these impurities are of lower melting point, they would be thrown out to the grain boundary of the metal on cooling. Granting that high sulphur may even favor an increase in the tensile strength and elongation, the question rises in one's mind, will such steel stand transverse strains or shocks? Prof. Hayward answers that question himself in his Charpy tests figures. From the above considerations of the discontinuity in the structure of such steels, one may well expect it.

One cannot help but wish that Dr. Unger would give us the secret of how to weld high-sulphur steel; it is the general experience that such steels are stringy and unweldable.

CHAIRMAN SAUVEUR.—I would like to ask Dr. Unger about those different sulphur steel tests. Do I understand they were all less than 1 per cent. sulphur?

J. S. UNGER.—I had steels of about 0.09 per cent. carbon that ranged in sulphur from 0.030 to 0.254 per cent.; of 0.32 per cent. carbon, from about 0.032 to 0.230 per cent. sulphur; and of 0.51 per cent. carbon, that ranged from 0.025 to 0.230 per cent. sulphur. Those figures can, however, be obtained in my former paper. I do not recollect the exact figures, but in making these tests I want to say, if you are selling to a customer steel that he proposes to weld into chain, the man that finally uses the chain is not interested to a very large extent as to what tensile strength a bar may possess. He is interested, however, in the strength of that chain.

In making this investigation I found that there was a greater possibility of making a poor weld in a chain, whether it was of low sulphur or high sulphur, than any slight change that might exist in the physical properties of the steel.

If we make a beam, channel or an angle, the usual method of testing is to make a tensile test. A beam is very rarely subjected to a tensile strain; it is either used as a column or as a girder and it is subjected to compressive or transverse loading. In testing the channels we made, we took full-sized channels, 6 ft. long, and tested them transversely to determine at what point they would take a permanent set, which imitated service conditions.

In testing the rails, we did not pay a great deal of attention to what they might show on tensile or drop tests; but we put them in a track that had to carry 2,000,000 tons of freight a month, and ran them for a year until they were practically worn out. We then compared them with other rails of a normal composition installed at the same time and location to

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find out whether they gave the same results. I assure you that until I learned they were safe, I watched the high-sulphur rails very carefully to observe the least signs of failure; so that they might be removed before an accident happened.

In making tests, the thing you must consider is, what is the customer going to do with the steel? You may make tensile, hardness, impact and all sorts of tests, but what you want to know is whether the steel is suitable for the purpose for which you are going to use it, not whether it is going to pass a particular physical or mechanical test.

G. AERTSEN, Philadelphia, Pa.—As I understood Dr. Unger, some of these tests on high and low steel were made on plate steel?

J. S. UNGER.—Yes.

G. AERTSEN.—Did you notice any greater difference between longitudinal and transverse tests on the high-sulphur steel than on low-sulphur steel?

J. S. UNGER.—We know, of course, that there is a pronounced difference between the longitudinal and the transverse tests on all plates, but there was not any greater difference on the high-sulphur steels than on the low-sulphur steels between the longitudinal and transverse tests.

G. AERTSEN.—Was there very much difference noticeable in the surface appearance of the high-sulphur plates? In other words, were they cleaner or less clean plates with low sulphur than with high?

J. S. UNGER.—I will answer that in this way: I kept on adding sulphur to the steel until I got to a point at which the steel was difficult to roll. I believe I could have rolled the highest-sulphur steels that I had, had I been willing to lower the rolling temperature, but my object was to roll these plates at a regular working temperature, not to make a special provision on account of their being high in sulphur. I am satisfied that I could have rolled them if I had reduced the rolling temperature about 100°. But to answer your question, after the sulphur had exceeded about 0.150, I obtained a surface that was inclined to be slivery, it was what we call snakey in the mill.

M. H. MEDWEDEFF.—I believe you said it did not make any difference on the welding properties of the steel?

J. S. UNGER.—You are correct. I brought out in my paper why it seems that sulphur was the only possible explanation, the reason being that if we cannot find any other reason, we pick out the sulphur and blame it on that.

LEONARD WALDO, New York, N. Y.—There is another connection in which the paper Dr. Unger published a year or so ago and this paper of

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Prof. Hayward are papers of great importance. The fuel near the Atlantic seacoast for open-hearth steel practice in the future will be Mexican crude oil. Mexican crude oil runs 3½ to 4 per cent. of sulphur; in the manufacture of many thousands of tons of steel, I found it practically impossible to prevent the addition of perhaps 0.01 per cent. of sulphur to the ingot steel, from the fuel alone. The education which has been persistently spread—whether because of gas fuel existing in certain localities (which has no sulphur) or not, I do not know—makes open-hearth superintendents hesitate over the addition of 0.01 per cent. of sulphur to a steel, which, in its ordinary product, would give 0.03. Dr. Unger has pointed out the error in this point of view, and he has only stated in words what every maker of steel rails has known for several years back, that the slight addition of sulphur has no practical effect on the product. I think, therefore, that we ought to be especially grateful to gentlemen like Dr. Unger and Prof. Hayward for putting this thing in a professional paper so that the facts and the reasons may be stated to inquirers who otherwise would be face to face with a difficult fuel situation as the years go on.

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A Method for Distinguishing Sulphides from Oxides in the Metallography of Steel

BY GEORGE F. COMSTOCK, A. B., MET. E., NIAGARA FALLS, N. Y.

(New York Meeting, February, 1917.)

IT seems a common opinion among metallographists that all light-gray inclusions seen with the microscope in polished sections of steel are manganese sulphide. Examples of this belief are continually appearing, as for instance in the paper by Dr. Henry Fay on manganese sulphide as a source of danger in steel rails,[1] and in Lieutenant-Commander Cook's paper on Metallography of Steel for U. S. Naval Ordnance.[2] Slate-

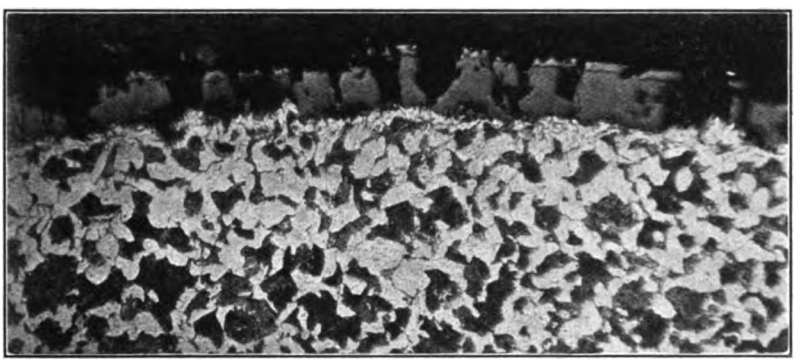

FIG. 1.—TRANSVERSE VIEW OF THE EDGE OF A SMALL HOT-ROLLED STEEL ROD. ETCHED WITH PICRIC ACID. × 200.

colored inclusions are considered to be silicates, and dove-gray inclusions, manganese sulphide. To show the danger in the latter unqualified assumption, it should be sufficient to examine the edge of any piece of steel that is covered with a fairly thick scale, and that has been polished in such a way as to prevent the scale from breaking away entirely below the polished surface. This can be done by protecting the scaled edge of

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* Metallographist, Physical Testing Laboratory, Titanium Alloy Manufacturing Co.

[1] A Microscopic Investigation of Broken Steel Rails: Manganese Sulphide as a Source of Danger. *Proceedings of the American Society for Testing Materials*, vol. 8, p. 74 (1908).

[2] *Trans.*, vol. 53, p. 238 (1915).

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the specimen with some soft metal, or fusible metal applied molten, or ordinary red fiber such as electricians use for insulating. An examination of this scale, if the polishing has been done well, and the sample is preferably unetched, will show it to be light gray in appearance, and apparently "manganese sulphide." As a matter of fact, however, we know that scale is chiefly iron oxide, and contains sulphur only as an impurity. Its similarity in appearance to manganese sulphide is shown in Fig. 1, which is a transverse view of the edge of a small hot-rolled steel rod, magnified 200 diameters. The steel has been etched with picric acid, but the gray scale can be plainly seen at the edge of the metal. The very dark material beyond the scale is the red fiber in which this specimen was clamped to prevent the scale from falling away during polishing.

In the course of several years' work with the microscope on rail steel and other commercial steels the writer has not infrequently seen light-gray inclusions that, from evidence given by sulphur prints, or for other reasons, did not seem likely to be sulphides, and in some cases these were known without any doubt to be oxides, like the scale mentioned above. Finally, a case of some importance arose, where it was desired to know definitely whether certain light-gray inclusions were sulphides or not, and it was not possible to decide the question from their color alone. Reference to the standard textbooks on metallography did not help much, for the only real test described for distinguishing oxides from sulphides involved heating the polished sample in a current of carefully purified hydrogen, and this method was considered as too complicated and necessitating too much preparation and practice. Etching with weak organic acids was not found satisfactory, and neither was the method of using a drop of sulphuric acid and watching for bubbles of hydrogen sulphide gas. Tried with known sulphide inclusions, both these methods attacked the steel itself too strongly, or else, if the acids were weakened, had no effect either on steel or sulphides. It was finally decided to get together a few specimens containing sulphides, and others in which oxides were known to be present, and to try various solutions on them until one was found that would attack one type of inclusion and not the other.

This work had not progressed very far when it was remembered that boiling alkaline sodium picrate, which is used to darken cementite, having no effect on ferrite and very little on pearlite, was always found to attack the sulphide inclusions in a sample, leaving black pits instead of the light-gray spots seen before etching. Fig. 2, for instance, shows some sulphide inclusions in a polished section, unetched, cut from a segregated streak in a steel rail, where sulphur prints indicated the presence of high sulphur. This same spot etched with boiling alkaline sodium picrate is shown in Fig. 3, where the sulphides are seen to have been blackened by this reagent. The darkening seems to be due to an actual solution of the manganese sulphide in the etching liquid, so that instead of a smooth

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polished surface, we now have a surface full of pits or hollows, where the sulphides were, and as these hollows do not reflect any light back into the microscope with vertical illumination, they appear black in the field of view. Figs. 4 and 5 show some other sulphide inclusions in a shell-steel billet, before and after etching, respectively. In this lower-carbon steel

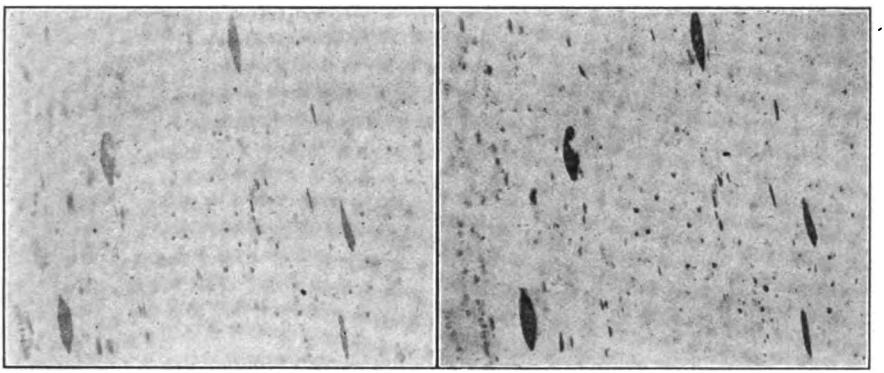

FIG. 2.—UNETCHED. FIG. 3.—ETCHED WITH BOILING ALKALINE SODIUM PICRATE.

Sulphide Inclusions in a Polished Section Cut from a Segregated Streak in a Steel Rod. × 130.

the ferrite-pearlite structure is brought out faintly by the etching, but not enough to interfere with the distinct blackening of the sulphides.

When this etching with boiling alkaline sodium picrate was tried on steel known to contain oxides, the problem of distinguishing oxides from

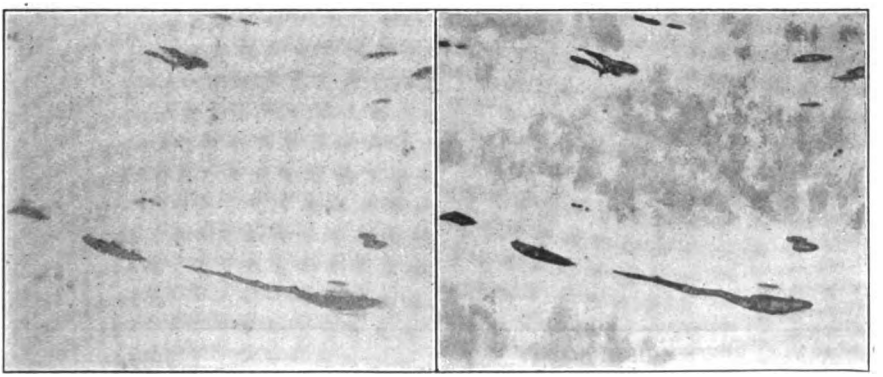

FIG. 4.—UNETCHED. FIG. 5.—ETCHED.

Sulphide Inclusions in a Shell-Steel Billet. × 130.

sulphides was solved at once, for the oxide inclusions were found to be absolutely unattacked. Fig. 6 shows some oxides in a polished unetched sample of clean rail steel treated while molten in a crucible with nickel oxide. The steel undoubtedly reduced the nickel, forming iron oxide.

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Fig. 7 shows this same spot after etching, and looks exactly like the unetched view. Fig. 8 shows a cross-section, unetched, of a seam in the base of a steel rail. These surface seams are always filled with scale, or oxide, although the light-gray color has sometimes led observers to believe that manganese sulphide was present. The fact that such seams give

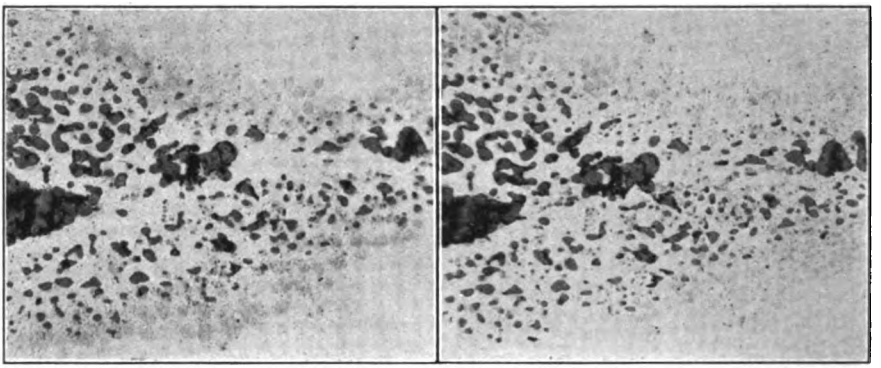

FIG. 6.—UNETCHED. FIG. 7.—ETCHED.
Iron Oxides in Rail Steel. × 130.

light streaks instead of black spots on sulphur prints and etched sections, however, shows that oxide and not sulphide is really present in them. This seam, etched with boiling alkaline sodium picrate, is shown in Fig. 9. The oxide is seen to be entirely unattacked.

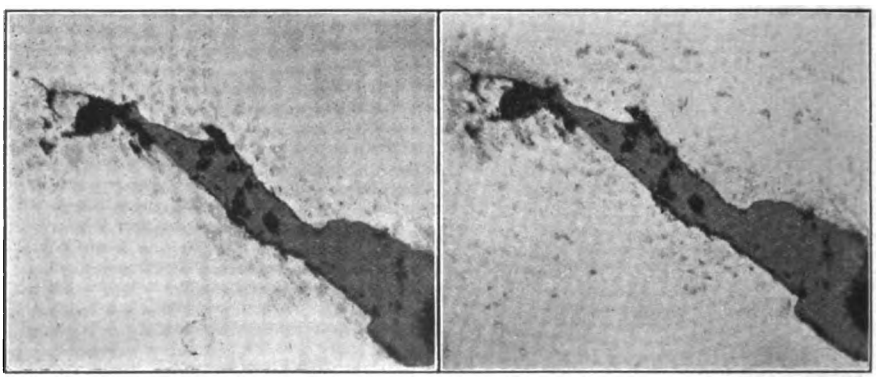

FIG. 8.—UNETCHED. FIG. 9.—ETCHED.
Cross-section of a Seam in the Base of a Steel Rail. × 130.

The application of this solution to etching all grades of steel for distinguishing between oxides and sulphides has not been described before, so far as the writer is aware. In ease of application and certainty of results it is much superior to other methods that have been suggested for this purpose. One advantage is that the solution used is one that every

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metallographist should have on hand for etching cementite in high-carbon steel. Thus this same solution can be made to serve two purposes. It was first discovered by Kourbatoff, who recommended it for darkening cementite because it does not attack ferrite or pearlite. The writer makes up the solution according to the formula given in Sauveur's *Metallography and Heat Treatment of Iron and Steel*, the procedure being about as follows: 25 g. NaOH are dissolved in 60 to 70 c.c. of water, 2 g. of picric acid are added, and the solution is heated until the picric acid is dissolved, when the volume is brought up to 100 c.c. by adding more water. In using this solution to etch polished-steel specimens, it is brought to boiling in a beaker on a hot plate, the specimen is immersed in the solution, and boiling is continued for 10 min. Then the specimen is removed, washed, and dried. The polished and etched surface may be wiped dry with chamois skin without affecting the results. If the speci-

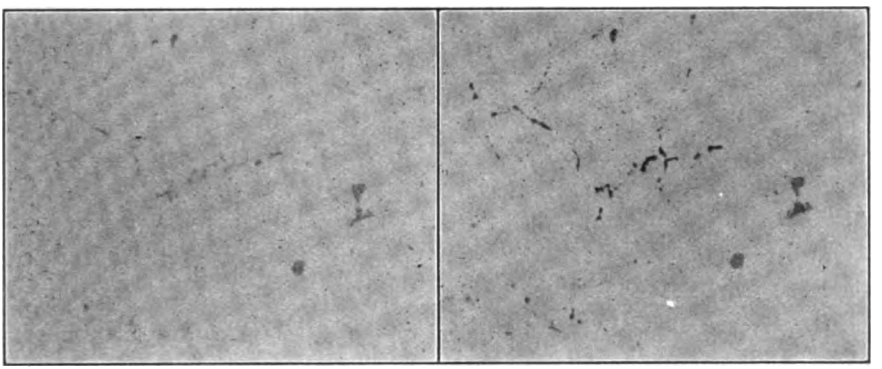

FIG. 10.—UNETCHED. FIG. 11.—ETCHED.
Polished Section of a Steel Casting which Cracked while Hot. × 130.

men contains cracks, this wiping is necessary, and repeated washing and drying may also be necessary, to remove the last traces of the sodium picrate from the surface to be examined. The solution may be used many times, but as the boiling makes it more concentrated water should be added occasionally to keep the volume about 100 c.c.

Before closing, some illustrations will be given of the practical use of this method for distinguishing oxides from sulphides. Fig. 10 shows a polished section of a steel casting which cracked while hot and in which the presence of oxides might therefore be expected. An experienced eye could perhaps tell which of these inclusions are sulphides and which are oxides, but the distinction at best would not be convincing. After etching by the method described above, however, the difference between the two kinds of inclusions becomes evident, as shown in Fig. 11. The arrangement of these sulphides is noteworthy, as a network is strongly suggested, and by this method of etching it can easily be shown that the

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small inclusions often found in a network arrangement in the ferrite of well-deoxidized steel castings are merely finely divided sulphides. An instance of such inclusions was described by Dr. Henry Fay in a paper on Some Causes of Failures in Metals,[3] and well illustrated by his Figs. 16 and 17 on page 450. These are said to be "slag" by Dr. Fay, but their appearance is exactly similar to numerous occurrences of sulphides which have come to the writer's attention.

A large complex slag inclusion, also in a cracked steel casting, is shown unetched in Fig. 12. The light-colored rough-looking part of this inclusion at the center of the field of view was bright yellow, and hence considered to be sulphide of iron. The other light-gray portions, including the spots in the dark-gray part, were supposed at first to be manganese sulphide, and the darker part, manganese silicate. But when etched with boiling alkaline sodium picrate, only the yellow constituent was

FIG. 12.—UNETCHED.

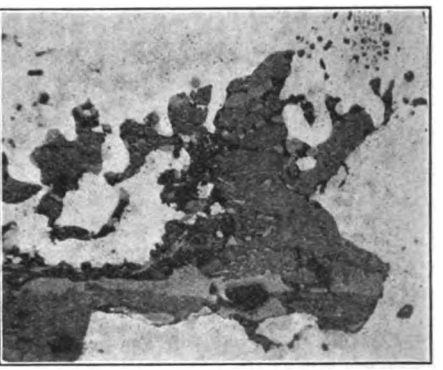

FIG. 13.—ETCHED.

Large Complex Slag Inclusion in a Cracked Steel Casting. × 130.

attacked, showing that it alone was sulphide, and the light-gray constituent was oxide instead of manganese sulphide. The appearance after etching is shown in Fig. 13.

A section cut from a bar of "ingot iron" that was red-short and cracked in rolling is shown in Fig. 14, taken before etching. Most of the inclusions found here were light gray in color, but many of them had yellow parts attached to them at one or both ends. The yellow substance, which is very rarely seen in steel, was taken to be sulphide of iron, and the gray inclusions were at first supposed to be manganese sulphide. But the manganese content of this ingot iron was found to be very low, and the sulphur also was not high enough to allow the presence of so many sulphides. Consequently, it was doubtful whether these gray inclusions were sulphides or oxides, until the sample was etched with the boiling alkaline sodium picrate solution, when it was apparent at once

[3] *Proceedings of the American Society for Testing Materials*, vol. 11, p. 439 (1911).

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that they were oxides, and only the yellow substance was sulphide. The inclusions of Fig. 14 are shown after etching in Fig. 15, where the distinction between oxide and sulphide is very plain. One of the largest particles of the yellow constituent was noticed, before etching, to have

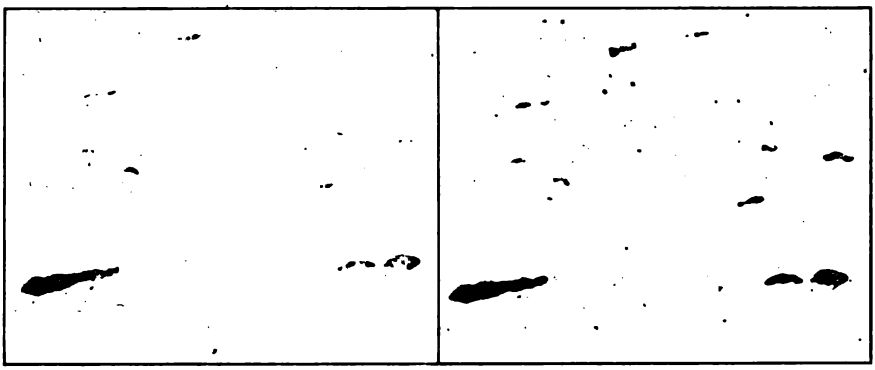

FIG. 14.—UNETCHED. FIG. 15.—ETCHED.
Section Cut from a Bar of "Ingot Iron" that was Red-short and Cracked in Rolling.
× 130.

a spotted appearance, and when examined under high magnification it was seen to be full of very fine gray spots. This is evidently a eutectic of iron oxide and iron sulphide. It is shown in Fig. 16, magnified 570

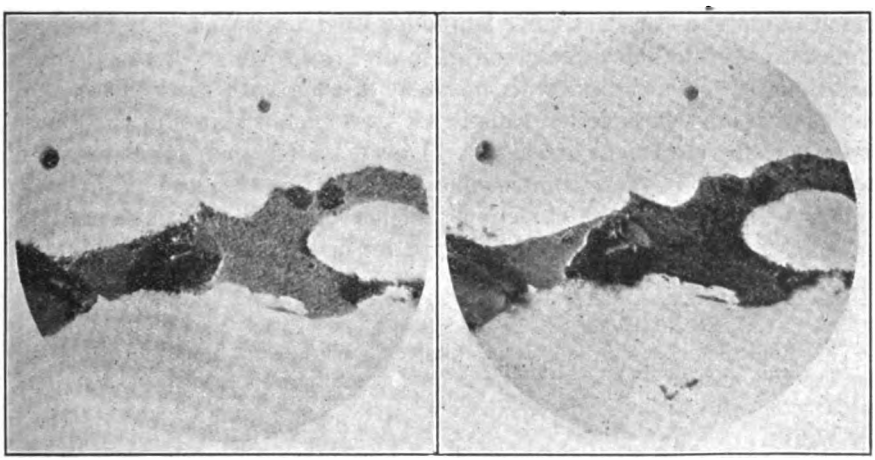

FIG. 16.—UNETCHED. FIG. 17.—ETCHED.
One of the Large Inclusions in Fig. 14. ×570.

diameters, the large unspotted dark-gray areas, with the black pits due to fracture in polishing, being oxide, and the spotted area being the eutectic. The oxide is here seen to be darker than the sulphide. Fig. 17 shows this same spot after a short etching with boiling alkaline sodium

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picrate, and it is apparent that the eutectic, but not the oxide, has been attacked, for the former is now darker than the latter, and its structure is blurred and indistinct.

These notes are presented in the hope that the simple method here described will appeal to metallographists in general as being worthy of trial in cases where the identity of light-gray inclusions in steel is in question, so that in future sulphides need not be called "slag" and the error of calling oxide or scale inclusions "manganese sulphide" merely because their color is light gray may also be avoided.

Discussion

H. M. Boylston, Cambridge, Mass.—It might be asked, since this test is so simple, why it was not discovered before. It seems to me the explanation is that when we have used the sodium picrate solution, it has generally been on a very high-carbon or hypereutectoid steel, to detect the presence of free cementite, and when we have looked for sulphides, it has generally been in the strictly hypoeutectoid steels. We have never applied this reagent to low-carbon steels, and where it was applied, the sulphides were not examined with any degree of care.

George F. Comstock, Niagara Falls, N. Y. (communication to the Secretary*).—Since writing this paper I have discovered in the literature a reference[1] to a paper by Siesching in *Metallurgie*, 1910, page 566, in which a method for darkening sulphides is given, which is somewhat similar to my method. Siesching gave a dark-brown color to sulphide inclusions by etching first with picric acid, then with nitric acid, and finally with hot concentrated caustic soda. It is not stated in the reference that this treatment attacks no inclusions other than sulphides, and it would seem to be more cumbersome in application than my method, as well as being open to the objection that pearlite would be darkened by the acid treatment, thus hiding to a greater or less extent any sulphides that might be embedded in it.

* Received Feb. 20, 1917.

[1] G. Rohl: The Constitution of the Sulphide Enclosures in Iron and Steel and the Desulphurization Process. *Iron and Steel Inst., Carnegie Scholarship Memoirs*, vol. 4, p. 39 (1912).

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Recrystallization after Plastic Deformation

BY HENRY M. HOWE, LL. D.,* BEDFORD HILLS, N. Y.

(New York Meeting, February, 1917)

THIS paper is a discussion of the extremely valuable one of Mathewson and Phillips, The Recrystallization of Cold-Worked Alpha Brass on Annealing,[1] which not only gives us a wealth of important data reached with great intelligence, but also shows both uncommon powers of imagination and a perfectly fair spirit.

Let me try to analyze some of their results, and to enunciate some of the laws to which these results point. For brevity I refer to them as "The Authors." The page numbers in parentheses refer to their paper.

1. *The Visible Aspects of Recrystallization.*—Plastic deformation in a single direction, as in cold-rolling, draws the grains out in that direction, that is, inequiaxes them, without destroying their apparent individuality. Each grain seems to endure the drawing out without changing either its volume or its orientation materially, for the etching tint of each grain seems to be as uniform after as before the drawing out, save for the "etch bands,"[2] shown in Figs. *A* to *D* of the Authors' Plate I.

As the drawn-out metal is heated progressively, on reaching a certain "disintegration temperature" these old grains seem to break up, probably into submicroscopic fragments, and this apparent disintegration is followed by a coalescence of these fragments, so that their number decreases and their size increases, till they may become very much coarser than before the drawing out. Ruder[3] coarsened the grains of silicon steel in this way till single grains reached an area of 50 cm. (7.75 sq. in.).

The deformation itself probably breaks each grain up into many very small fragments, but the fact that each grain continues to have a uniform etching tint different from that of its neighbors indicates that these fragments retain the initial common orientation of the grain. The apparent disintegration which we observe, then, is rather an abandonment of community of orientation for individuality of orientation, by the fragments formed earlier during the deformation. Hence we

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[1] *Trans.*, vol. 54, p. 608 (1916).

[2] Rosenhain calls these "slip bands," and I have called them "X bands," to avoid any preconception as to their nature (*The Metallography of Steel and Cast Iron*, p. 452).

[3] *Trans.*, vol. 47, p. 569 (1913).

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should speak rather of a "reorientation" than of a "disintegration" temperature. The reorientation and regrowth may jointly be called "recrystallization."

The recrystallization is accompanied by softening, that is to say, though the first raising of the temperature after the cold-rolling may increase the hardening which the cold-rolling caused, yet with further rise of temperature the hardness falls progressively to that of the normal metal.

While the Authors would explain the nature of disintegration and regrowth, their main objective seems to be to trace the connection between these two processes and this softening.

2. *Our natural conception of disintegration and grain growth* I take to be that the plastic deformation itself breaks each grain up into many fragments as already pointed out, of which the number increases and the size decreases as the deformation increases; that each fragment retains for the time the initial orientation of the grain from which it is broken, as is shown by the uniform etching tint of all these fragments of a given grain; but that each is sheathed completely with metal made amorphous by the friction during the act of deformation. This amorphous metal cements the whole together. On this hypothesis when the deformed metal is heated, five distinct changes occur side by side, though not necessarily simultaneously or in any fixed proportion to each other. These are, first, a progressive reabsorption of the amorphous sheaths by the crystalline fragments which they inclose; second, a suspected progressive but undescribed change in the condition of so much of these sheaths as has not yet become reabsorbed, or of the crystalline grain fragments thus sheathed, or of both; third, an apparent disintegration of the initial grains, which is really only a reorientation of the grain fragments already formed; fourth, their progressive growth by coalescence, with consequent lessening of their number; and fifth, the progressive release of stress as the mobility increases and the existing elastic limit falls correspondingly. The first three may be grouped together as "recrystallization." Let us in (3) to (7) glance at the evidence that these five agencies are at work.

3. *The reabsorption of the sheaths* with rising temperature is inferred from the observation that, by heating before etching, the etch bands, which seems to represent an accumulation of this same amorphous metal along the slipping planes, are made first fainter and finally undetectable.

4. *The reorientation of the grain fragments* is shown by the apparent disintegration which the grains undergo, for this disintegration means only that the several fragments of a given grain cease to have common orientation and hence to etch alike, and instead adopt each a new orientation, so that they etch differently and hence appear as distinct grains or grain fragments. It is not clear that this distinction between grains

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and grain fragments need be insisted upon in what follows, for indeed each fragment seems for all intents and purposes to be an independent grain, once its reorientation has occurred.

5. *Grain Growth.*—The progressive growth of the grain fragments is familiar to all through the coarsening that occurs on heating cold-rolled or otherwise plastically deformed metal.

6. *The progressive release of stress* could no doubt be proved by noting how much, after reheating to various temperatures, the specimen bends out of shape spontaneously on cutting away a fixed part of it, so as to destroy the balance of internal stresses remaining after the initial cold-rolling itself. We may assume confidently that the degree of this spontaneous bending decreases progressively as the temperature of annealing, prior to cutting away, rises.

7. *A progressive change in the condition of the amorphous sheaths* or of the crystalline fragments is neither shown nor known. I have inferred it as a probable cause of the perturbations which the cohesion, as indicated by the elastic limit and hardness of cold-rolled iron, both pure and cementite-bearing (mild steel), undergoes on gentle heating. On either slight heating or mere rest such iron may soften materially or may harden greatly, according to the intensity of the previous deformation.[4]

8. *In what Proportion is the Softening Caused by Annealing due to Each of These Five Influences?*—Though the present evidence does not enable us to give a full answer to this question, some inferences concerning it may be drawn. Let us see, in (9) to (14), what importance we are inclined to assign each of these five supposed agencies.

9. *Stress.*—The relief of stress is not likely to cause a very large fraction of the softening induced by annealing; first because it is incompetent to explain the great hardening and embrittlement which gentle heating causes in cold-worked iron; and second because the progress of the soften-

[4] *The Metallography of Steel and Cast Iron*, pp. 365 and 452 to 458. The Authors' explanation *Trans*, vol. 54, p. 614 (1916) that the strengthening represents the removal of stress is not competent to explain either the tripling of the hardness which, as I find, mere rest after deformation may cause, or the simultaneous great loss of ductility. Thus Stromeyer (*Journal of the Iron and Steel Institute*, vol. 75, No. 3, p. 92 (1907)) finds that the ability of the outer surface of a nicked test-piece to elongate before rupture may fall from 60 per cent. to 6 per cent. on long rest, and in one case to 6 and in another even to 3 per cent. on heating to 100° for 15 min. Nor can we accept the explanation (Gulliver, *Journal of the Institute of Metals*, No. 2, 1909, p. 220 that this hardening, strengthening, and embrittlement represent "a change from the instable Fe_3C, present in commercial steels, to the stable Fe_3C," because I find that practically carbonless steel, with only 0.01 per cent. of carbon, hardens like and perhaps even more than common mild steel of 0.147 per cent. of carbon, on heating to 100° after plastic deformation (*op. cit.*, p. 365). This strengthening and hardening which slight heating causes so markedly in iron occur only in a slight degree in brass, as is shown concordantly by the results of Bengough and the Authors (pp. 610 and 613). Indeed it is so slight as to have been overlooked by Grard (p. 610).

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ing, both absolute and relative, in the Authors' results is not such as would be expected to be caused by stress-relief. Thus, on the theory that stress-relief is the chief cause of the softening, it is hard to see why a progressive increase in the degree of cold-rolling should lead either to the progressive and marked lowering of the temperature of most rapid softening, or to the marked narrowing of the temperature range in which most of this softening occurs, as recorded by the Authors (Fig. 3), or why, after 8 per cent. reduction by rolling, the softening should be only 1.4 units between 350° and 450°, yet should rise to 2.1 units between 450° and 550° and to 2.5 between 550° and 650° (Table IV).

10. *Reorientation or Disintegration.*—If we are right in conceiving that visible disintegration represents the breaking away from the initial orientation and substituting a new one in each of the several grain fragments which thereby become visible, then before any large fraction of the fragments have thus changed, the change should become visible, substituting in the earliest of the reorienting grains finer uniformly etching areas for the initial coarser ones. On this conception, disintegration is not likely to be the chief cause of the softening, because after the Authors' 4 and 12 per cent. reductions more than one-third of the total softening occurs from 100° to 200° before the reorientation becomes visible (Table IV).

Again, it is hard to reconcile the belief that most of the softening is due to reorientation with the fact that, after their 35 per cent. reduction, 80 per cent. of the total softening occurs after reorientation has become visible, and successive softenings each of about 10 per cent. of the total are found at temperatures, 200°, 300°, and even 400° higher, though the micrographs suggest no such temperature-distribution of the reorientation.

11. *Grain Growth.*—If we conceive, as seems reasonable, that the grain fragments cannot coalesce with each other to any important degree before they reorient themselves and thereby cause visible disintegration, then the great softening which, after the 4 per cent. and 8 per cent. reductions, precedes visible reorientation, cannot be referred to grain growth. This disinclines us to refer the softening as a whole chiefly to grain growth, in spite of the very great softening that accompanies the replacement of finer by coarser-grained gold in the important experiments of Fahrenwald,[5] a softening so great that we hesitate to refer it solely to difference in grain size. The Authors clearly share this belief (p. 638).

Another indication that grain growth is not the chief cause of the softening is the discordance between the curves of grain size and of hardness in Fig. 4. To judge from Plate XI there is a great retardation of grain growth from the 350°–450° to the 450°–550° range, and Fig. 4

[5] *Trans.*, vol. 54, p. 627.

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tells us that there is such a retardation from the 450°–550° to the 550°–650° range, yet in passing these ranges the softening undergoes no corresponding retardation, nor does the tensile weakening.

12. *Reabsorption of the Amorphous Sheaths.*—Thin as these sheaths seem to be, if we refer to them the great hardening that cold work is capable of giving we must infer that their total volume forms a very appreciable fraction of the whole mass, an inference which is the more probable in view of the marked changes which cold work causes in the solution pressure, density, and other physical properties of the mass as a whole.

It is reasonable to hold that this amorphous metal is harder than the crystalline, because it lacks the cleavages and partings which enfeeble every crystalline mass. Hence its progressive reabsorption on heating and rest would itself naturally cause a corresponding softening of the mass as a whole. It may be that the rate of reabsorption of this amorphous metal on heating and rest varies in such a way as to contribute greatly to the variations observed in the rate of softening of the whole mass.

13. *Change of Condition of the Amorphous Sheaths or of the Crystalline Fragments which they Inclose.*—The great hardening, strengthening, and embrittlement which pure iron undergoes on rest or on slight heating after cold deformation are not readily referred to any of the four agencies which we have just considered in (9) to (12). In order to explain them I attempt to refer them to unknown changes in the nature of either the amorphous sheaths or crystalline fragments which they inclose, or both. If we grant that either or both these agencies are capable of causing these very striking changes, we naturally infer that they may also contribute to a very important degree to the softening which follows this hardening in iron on higher heating, and occurs without prominent hardening in the other metals.

14. *To sum up,* stress-relief and grain growth are not likely to contribute greatly to the softening caused by heating overstrained metal. Reorientation may contribute greatly, but it is not likely to be the chief cause. The reabsorption of the amorphous sheaths inclosing the grain fragments, and a progressive change in the condition of those sheaths of the crystalline metal in those fragments, are possible important causes. Beyond this preliminary speculation the present data do not justify our going.

It is probable that at least two agencies are at work, with effects which differ greatly, because, in the case of iron, rest and gentle heating, while effacing one of the effects of the cold work, the etch bands, simultaneously increase another and simultaneous effect of that same cold work, the hardening which it causes.

15. *Correspondence between the Authors' Conjectured Progress of*

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Recrystallization and Their Observed Progress of Softening.—First we must understand the Authors' term "fragmental resolution," for which I venture to suggest "limiting grain size." They hold that, for each temperature, there is a certain " limiting grain size," "limiting" in the sense that all grain or grain fragments which are smaller than it start to grow toward it, but cannot grow beyond it, nor can grains of this or any greater size grow at this temperature. As far as I understand "fragmental resolution," it means this "limiting grain size," a

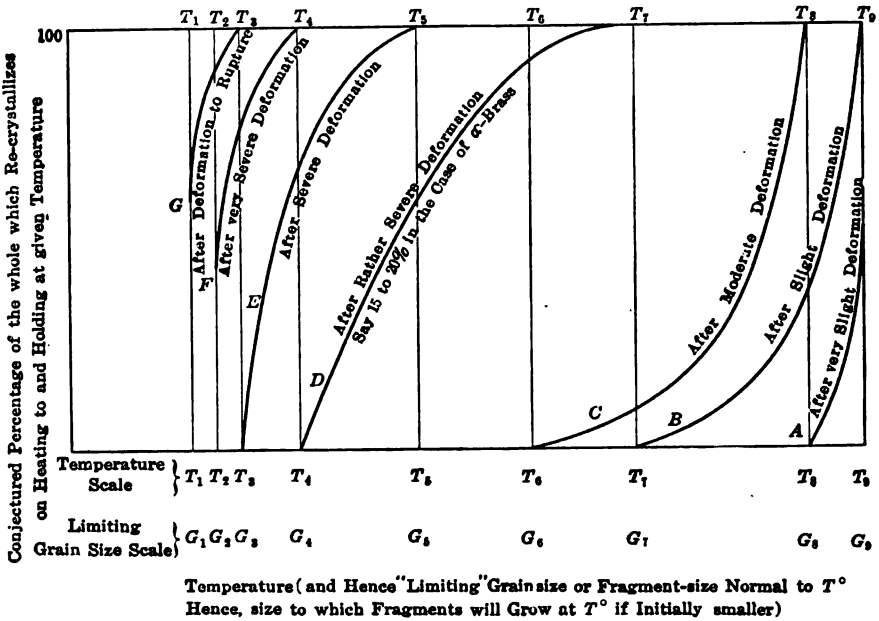

FIG. *A*.—CONJECTURED PROGRESS OF RECRYSTALLIZATION OF COLD-WORKED BRASS ON PROGRESSIVE HEATING, AFTER VARIOUS DEGREES OF PRIOR PLASTIC DEFORMATION.

term which I offer reluctantly and only because I find "fragmental resolution" confusing.

The Authors give in their Fig. 2 a most interesting generalization from their hypothesis. This, their conjecture as to the progress of recrystallization, that is of the return to the crystalline state of the metal made amorphous by the deformation, may perhaps be grasped more easily as relettered in my Fig. *A*. Here ordinates show what percentage of that part of the metal which has thus been made amorphous can recrystallize in rising to given temperature and staying there 30 min. Abscissæ measure both temperature and the limiting grain size for given temperature. Far from attempting quantitative accuracy, this sketch tries only to show the general family to which the curves are expected to belong.

According to this forecast, the greater the previous reduction by cold-rolling has been, (1) the lower is the temperature at which the curves leave the horizontal axis, that is the temperature at which recrystallization

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starts, and (2) the steeper is the beginning of the curve. Further (3), whereas after severe deformation the curves are convex upward, after slight deformation they are concave upward. Hence the curves for great reductions form a sheaf of which the top fans out to the right, whereas those of slight deformation form a sheaf of which the bottom fans out to the left.

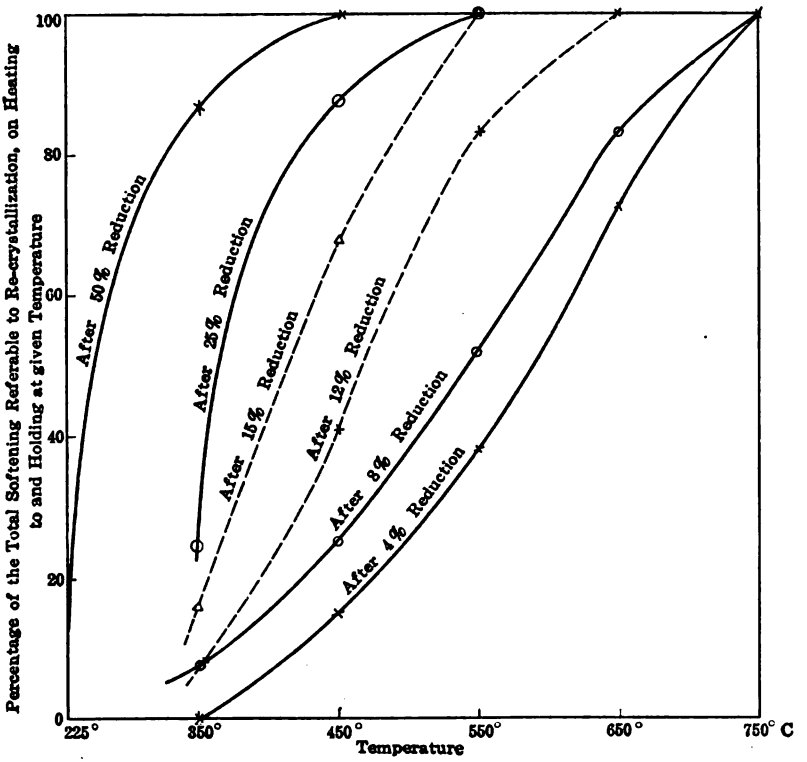

FIG. *B*.—OBSERVED PROGRESS OF SOFTENING OF COLD-ROLLED BRASS. (MATHEWSON AND PHILLIPS.)

We have no data showing the true progress of this recrystallization, and hence we cannot check their forecast as to the relation of this progress to the temperature of annealing. But in my Fig. *B*, I attempt to check it indirectly by showing how the progress of that fraction of the softening which may be referred to recrystallization is related to the annealing temperature. Any such attempt must be very rough, because we do not know what fraction of the softening is due to recrystallization and what to grain growth. In view of the relatively small effect of grain size as such on hardness, I assume that the greater part of the softening is due to recrystallization, and for the purposes of this rough check I assume

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that the temperatures given in line 2 of my Table *A* are those at which recrystallization becomes complete, on the ground that these are the ones at which the softening is nearly complete.

Because the heatings underlying their Table IV were of 30 min., because a temperature of 255° was needed in their Table II to cause an appreciable softening after 40 per cent. reduction in about this time; and because 30 min. at 200° caused a slight hardening after this reduction; I set $T°_1$, the temperature needed to cause the first softening after 50 per cent. reduction, at 255°.

The curves of Fig. *B*, plotted on these assumptions, tend to support the second and third of the Authors' predictions, that the steepness of the beginning of the curve increases with the previous reduction, and that the curves for great reduction are convex upward whereas those for light reductions are concave upward.

Their first contention, that the temperature at which recrystallization starts is the lower the greater the deformation has been, has such good direct experimental support that it is to be accepted in spite of its not being clearly indicated by these curves. Note, for instance, that the shape of the 8 and 12 per cent. reduction curves suggests that they leave the horizontal axis at temperatures rather below than above those of the 15 and 25 per cent. curves.

16. *Is There an Equilibrium Grain Size for Each Temperature?*—The Authors' assertion to this effect (p. 625) may well be taken rather cautiously in our present ignorance as to the mechanism of grain growth. That this growth accelerates with rise of temperature, and retards itself as it proceeds, seems clear; but that it ceases completely, so that the agencies resisting coalescence come in time to arrest it completely, at relatively high temperatures, has not yet been shown, I believe. A marked retardation as the process proceeds would naturally occur on almost any theory of grain growth, but that need not imply arrest. If it is true as reported that meteorites often consist of a single enormous grain, that would indicate that, at least in this case, if time is almost unlimited coalescence becomes almost unlimited, and that here the anti-coalescence agencies never become strong enough to cause complete arrest.

17. *Certain Laws of Annealing for Alpha Brass.*—The Authors' data point to the following laws:

(*A*) As the degree of previous deformation (cold-rolling) increases from a reduction in thickness of 4 per cent. to one of 40 or 50 per cent.,[6] (1) the temperature at which in subsequent heating reorientation becomes visible under a magnification of 85 diameters falls progressively from 650° to between 275° and 300° (Table III); (2) the temperature of most

[6] Both the 40 per cent. maximum reduction of Table III and the 50 per cent. reduction of Table IV and Plate XI are correct.

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rapid softening falls progressively from between 550° and 650° to below 350° (Table IV); and (3) the range of temperature in which the greater part of the softening occurs becomes progressively narrower (Table IV).[7]

(*B*) From this it follows that the annealing temperature needed to cause nearly complete softening is the lower the greater the previous reduction has been, and the longer the annealing period. For a 30-min. annealing the relation is about as follows for alpha brass:

TABLE *A*

1. Percentage of reduction by cold-rolling.....	4	8	12	15	25	50
2. Temperature needed for nearly complete softening, degrees Centigrade.....	750	750	650	550	550	450
3. Percentage of total softening at that temperature.....	90.4	98.8	90.0	90.6	79.6	79.2
4. Lowest temperature of visible disintegration, degrees Centigrade.....	650	550	550	450	350	350

(*C*) The temperature-range of most rapid softening merges into a higher range of progressively retarded further softening, which continues in every case even to beyond 750°, suggesting that the softening is due to at least two superposed changes, of which one, probably recrystallization, occurs rapidly at a relatively low temperature—the lower and narrower the greater has been the cold-rolling—and theother, probably grain growth, is spread out.

(*D*) *Decrease on Heating of the Excess of Hardness Caused by Greater Deformation.*—Here we may leave out of account the 2 per cent. reduction, because its effects are so slight compared with the accuracy of the Shore method, and the 50 per cent. reduction because this refers to a different alloy.

The excess of hardness caused by 25 per cent. reduction over that caused by 4 per cent., which is 14.7 Shore units before annealing, falls to 10.9 units on annealing at 350° and to 1.7 units on annealing at 450°. We may question whether this excess remains measurably great after annealing at 550° or any higher temperature, for though, if we consider the extreme cases, the 4 and the 25 per cent. reductions, a slight excess persists even on heating to 800°, as shown in line 4 of Table *B*, yet for

[7] This agrees with Muir's observation that the greater the hardening caused by overstrain, as shown by the rise of the yield point in steel, the lower is the temperature at which softening sets in (*Philosophical Transactions of the Royal Society of London*, vol. 193 A, p. 22, 1900), and with Chappell's that the greater the deformation the lower the temperature at which the recrystallization (disintegration and subsequent coarsening) of overstrained steel occurs (*Journal of the Iron and Steel Institute*, vol. 89, pp. 471, 496, 1914).

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TABLE B

1. Annealing temperature, degrees C....	Unannealed	350	450	550	650	750	800
2. Hardness after 25 per cent. reduction..	27.4	23.6	13.7	11.8	10.2	8.2	7.8
3. Hardness after 4 per cent. reduction...	12.7	12.7	12.0	10.9	9.3	8.0	7.5
4. Difference, Shore units[8].....	14.7	10.9	1.7	0.9	0.9	0.2	0.3

temperatures above 450° this excess hardly exceeds the limits of error. Moreover, if we compare all five of the reductions, we find great anomalies. For instance, the specimens reduced by 4 per cent. are actually reported as harder than those reduced by either 8 or 12 per cent., after the 650°, 750°, and 800° annealings severally.

Again, for each of the four annealing temperatures, 550°, 650°, 750°, and 800°, there are five reductions that we are considering, those of 4, 8, 12, 15 and 25 per cent. If for each temperature we compare the hardness after any one reduction with that after the next greater reduction, we have four such comparisons at each temperature, or 16 for the four temperatures. Though in eight of these 16 cases the hardness given by the greater reduction exceeds that given by the less, yet in seven cases the reverse is true, and in the remaining case the greater and the less reduction give the same hardness.

(*E*) *The Temperature at which Disintegration First Becomes Visible may Suffice to Efface the Excess of Hardness Given by Greater Reduction.*— This refers to the temperature at which the reorientation becomes visible under a magnification of 85 diameters. For instance, this temperature is 550° for the reductions of 8 and of 12 per cent., and at this temperature the residual hardness is actually greater after the 8 than after the 12 per cent. reduction, though the difference hardly exceeds the limits of observational error.

(*F*) As the temperature rises, the softening accelerates greatly and continuously up to a maximum rate. For instance, a softening amounting to 3 Shore units is made 6.4 times as rapidly a 50° rise, from 225° to 275°, and about 1,000 times as rapid by a 100° rise from 225° to 325° (Table II, p. 7).

Discussion of this paper on p. 589.

[8] By a "Shore unit" is meant a difference of 1 in the unit place of the Shore scale, hardness 14.3 being 2 units greater than 12.3.

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Grain Growth Phenomena in Metals

BY ZAY JEFFRIES, B. S., MET. E.,* CLEVELAND, O.

(New York Meeting, February, 1917)

THE object of the present paper is to enlarge somewhat on the general principles advanced in my discussion[1] of Mathewson and Phillips' article on The Recrystallization of Cold-Worked Alpha Brass on Annealing.[2] It will also serve as an acknowledgment of Prof. Howe's[3] most important remarks on my contribution. In this paper the writer has adopted Prof. Howe's suggestion to substitute the term "germinative temperature" for "critical temperature for grain growth." Instead of being an exact or certain temperature it should be considered that the germinative temperature phenomenon exists throughout a small temperature range.

Factors Influencing Fast Growth Phenomena

The development of grains of macroscopic size at the germinative temperature should be considered the extreme condition of a general rule. When considered in this light it is not strange that the development of these large grains should be the exception and not the rule. The principal factors influencing the operation of the laws of fast growth temperature are as follows:

1. Rate of heating.

In the original paper,[1] the germinative temperature was defined as "the minimum temperature at which two adjacent grains can coalesce to form one larger grain, provided that this larger grain will have sufficiently increased its power of attack, to enable it to absorb adjacent grains which cannot coalesce with each other. Time is always to be understood as a factor governing the first stage of grain growth." If the time of heating is short, the germinative temperature is raised. The formation of large grains in metals may occur at relatively high temperatures when the rate of heating is rapid. It is to be understood that in such cases, grain growth

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[1] *Trans.*, vol. 54, p. 658 (1916).

[2] *Trans.*, vol. 54, p. 608 (1916).

[3] See p. 582.

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must be very rapid, the germinative grains having the power to absorb the inert grains before the latter can coalesce with one another. The rapidity of absorption of the inert grains by the germinative grains is at times almost beyond comprehension. In one instance, the author calculated that 37,500,000 individual grains were absorbed by one germinative grain in 10 sec. The grain growth took place at such a temperature that the inert grains would have coalesced freely with one another, had time been given.

A further increase in rate of heating will prevent the formation of coarse grains. Other influences of change of rate of heating were considered in the original paper.

Figs. 1 and 2 show visually the effect of rate on the formation of large grains. Fig. 1 represents a section of metal held at the fast growth temperature for 30 min. This is a sample of compressed metal heated with electric current in which the temperature gradient was the cause of the selective grain growth. The photograph is at a magnification of 5 diameters and the grains can readily be seen macroscopically. Fig. 2 represents a section of the same metal in which the temperature was

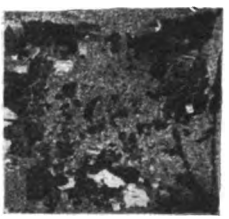

FIG. 1.

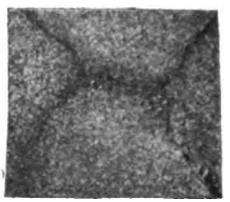

FIG. 2.

FIG. 1.—A SECTION OF METAL HELD AT THE GERMINATIVE TEMPERATURE FOR 30 MIN. × 5.

FIG. 2.—SECTION OF THE SAME METAL AS IN FIG. 1, IN WHICH THE TEMPERATURE WAS RAISED QUICKLY TO A POINT FAR ABOVE THE GERMINATIVE TEMPERATURE RANGE, AND HELD THERE FOR 30 MIN.

raised quickly to a point far above the germinative temperature range, and held at this temperature for 30 min. Some of the larger individual grains in Fig. 1 occupy an area equal to about 1,000 average grains in Fig. 2. (The lines in Fig. 2 are cracks which formed in the specimen during mounting.) The mean grain size of the fine-grained areas in Fig. 1 is very much less than the mean grain size in Fig. 2. The grain size at the center of Fig. 1 represents about the normal grain size just above the fast growth temperature. The grain size shown in Fig. 2 is approximately the equilibrium grain size for a temperature several hundred degrees above the fast growth temperature.

Fig. 3 (18 diameters) shows the large grains formed at the germinative temperature in the process of absorbing the equilibrium grains in the hotter portion of the sample.

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Fig. 4 (18 diameters) represents the same phenomenon when nearly all of the small grains have been absorbed.

2. *Resistance to grain growth* might be divided into two parts:

(*a*) The resistance to grain growth inherent in the pure predominating constituent; for example, copper in the pure state at a certain temperature has a certain inherent resistance to grain growth.

(*b*) The resistance to grain growth due to obstruction by foreign bodies; for example, copper oxide in copper or pearlite in low-carbon steel.

While it might seem anomalous, high resistance to grain growth due to the second cause greatly favors the formation of very coarse grains at the germinative temperature. As the resistance to grain growth increases in a given material the germinative temperature increases, and the time necessary for the formation of the large grains decreases rapidly. As an example of the above, a sample of metal containing 2 per cent. of non-metallic material required about 1½ hr. to form the large grains at a comparatively low germinative temperature. The same metal containing 10 per cent. of non-metallic material required but 2 or 3 min. to form the coarse grains. The coarse grains in the latter case were larger than those of the former.

In the sample containing 10 per cent. of non-metallics (by volume) the germinative temperature was only 100°C. below the fusion temperature. The addition of 12 per cent. non-metallics was sufficient to raise the germinative temperature above the fusion temperature and hence make it non-existent.

If it is considered that high resistance to grain growth favors the formation of coarse grains, then, naturally, low resistance to grain growth would defeat their formation. The explanation of this is found in the fact that as the resistance to grain growth increases, corresponding to a rise in the germinative temperature, the existing grains at the germinative temperature are very much smaller than the equilibrium grains for that temperature. Since the grains are so far out of equilibrium as regards size, when the germinative grains reach a size considerably larger than the existing grains, the latter are absorbed rapidly; first, because they are so small and, second, because the temperature is so high.

The mechanism of the fast growth phenomena operates in the reverse order for low resistance to grain growth. In the first place the germinative temperature is low and the existing grains at the fast growth temperature are more nearly in equilibrium in regard to size. This, coupled with the actual lower temperature, will materially reduce the velocity of grain growth as well as the size of the maximum grains at the fast growth temperature. In order that selective grain growth may take place at all, it is essential that the existing grains at the fast growth temperature should be smaller than the equilibrium grains for that temperature. As a general rule it might be stated that, other conditions being

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equal, the finer the existing grains at the germinative temperature, the greater will be the tendency toward selective grain growth.

3. *Influence of Grain Size Prior to Deformation.*—It is easily possible in pure metals either to help or defeat the selective grain growth at the germinative temperature. If very large grains have been formed by previous high temperature anneal, like normalizing of nearly pure iron such as Prof. Sauveur used in his experiments, it is probable that the grain fragments which formed in the cold-bent specimen after annealing were so large that the germinative temperature phenomena were largely masked. In addition to this, the germinative temperature would be so low that the velocity of grain growth would be decreased. Since the coalescence of adjacent grains in the grain growth region is a function of time, among other things, it is probable that grains not far removed from the centers of selective growth might coalesce with each other before the larger grains in their conquest would have time to absorb them.

While Prof. Sauveur observes that coarse grains did not form in nearly pure iron, it has been the writer's experience that there is nearly always a region in which the grains are larger than those on either side. Sometimes the difference is not great enough to be unmistakably observed in the ordinary microscopic examination, but grain size determinations usually show the difference. A good example of the grain-growth phenomena in a solid solution comparatively free from foreign bodies will be found in Mathewson and Phillips' micrographs.[4]

In a given pure metal with a given amount of cold deformation, the grain size before deformation will have a marked effect on the selective grain growth during annealing. If the initial grain size is large the grain fragments formed during annealing will be correspondingly large, and if the initial grain size is small the grain fragments will also be smaller than in the previous case. The latter condition is conducive to the formation of large grains at the germinative temperature. Instead of normalizing the pure iron, if Prof. Sauveur had given it an initial treatment so as to produce small grains he would have noticed a more marked recrystallization effect after cold deformation with subsequent annealing. On the other hand, if the grains are initially very large it is possible almost completely to mask the fast growth phenomenon.

4. *Temperature and Deformation Gradients.*—The formation of large grains at the germinative temperature is dependent upon a rather even grain size with a temperature gradient or an even temperature with a deformation gradient or any combination of the two. In compressed metals heated by electric current the grain size is usually quite uniform and the temperature gradient serves to start the formation of the large

[4] C. H. Mathewson and Arthur Phillips: Recrystallization of Cold-Worked Alpha Brass on Annealing, *Trans.*, vol. 54, p. 608 (1916).

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grains. The greater the temperature gradient, the greater will be the tendency toward selective grain growth.

If a metal is deformed in the cold in such a manner that the degree of deformation differs progressively from point to point—for example, in a bent metal bar (the neutral axis of the bar will have no deformation, but the edges perpendicular to the neutral axis will have a maximum deformation)—there will be a deformation gradient from the surface to the neutral axis. If a bar so bent be heated in a muffle it might be considered that there was no temperature gradient, but the deformation gradient, with temperature constant, would produce a recrystallization gradient in which the parts most severely deformed would recrystallize at the lower temperature. This difference in temperature of recrystallization furnishes the cause for the selective grain growth due to deformation gradients. Various combinations of temperature and deformation gradients might be encountered. For example, the condition might obtain in which the temperature gradient just counterbalances the effect of deformation gradient, and fast growth at the proper temperature would not occur. Local deformation gradients occur in any metal which has been cold plastically deformed, due to the variations in the directional properties and size of adjacent grains.[5]

If the total cold deformation of a piece of metal has been moderate such as would obtain after one or two passes through rolls, the local deformation gradients may give rise to a comparatively few germinative grains which during heating absorb the inert grains thus causing coarse crystallization. As the degree of cold deformation increases, the local deformation gradients will decrease. When the total average cold deformation exceeds about 30 or 40 per cent. reduction in area of the metal, the number of germinative grains formed during normal muffle heating may approximately equal the number of equilibrium grains for the maximum temperature reached during annealing, and thus the metal is said to be fine grained. The above adequately explains the cause of coarse crystallization during annealing of moderately cold deformed metals, and the formation of progressively finer grains with the same heat treatment, as the degree of cold deformation increases.

5. *Influence of Thickness of Sample.*—Stead and Carpenter[6] find that metal sheets which are less than about 0.012 in. in thickness are very much more susceptible to the formation of coarse grains than when the thickness is greater. Their experiments were conducted with electro-deposited iron and the coarse grains formed while cooling from gamma to non-gamma iron. The reasons for the formation of coarse grains are, first, a temperature gradient and, second, the thinness of the metal plates. These conditions will be recognized as ideal for the operation of the tempera-

[5] Mathewson and Phillips: *loc. cit.*

[6] J. E. Stead and H. C. H. Carpenter: The Crystallising Properties of Electro-Deposited Iron, *Journal of the Iron and Steel Institute*, vol. 88, p. 119 (1913).

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ture-gradient law, since the progressive change of gamma to non-gamma iron would produce very small grain fragments and even a slight temperature gradient would be sufficient to cause selective growth.

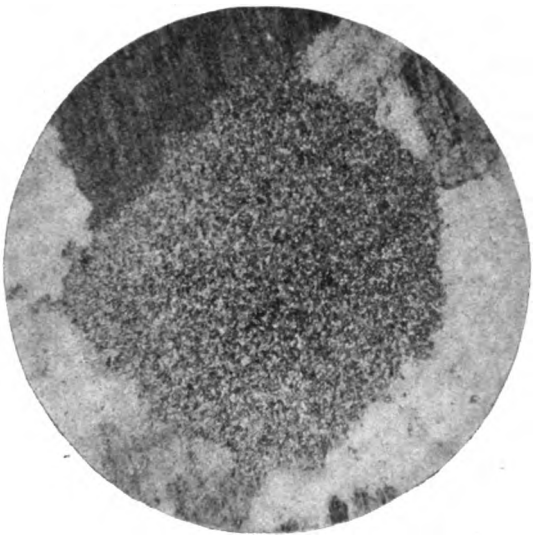

FIG. 3.—THE LARGE GRAINS FORMED AT THE GERMINATIVE TEMPERATURE IN THE PROCESS OF ABSORBING THE EQUILIBRIUM GRAINS IN THE HOTTER PORTION OF THE SAMPLE. × 18.

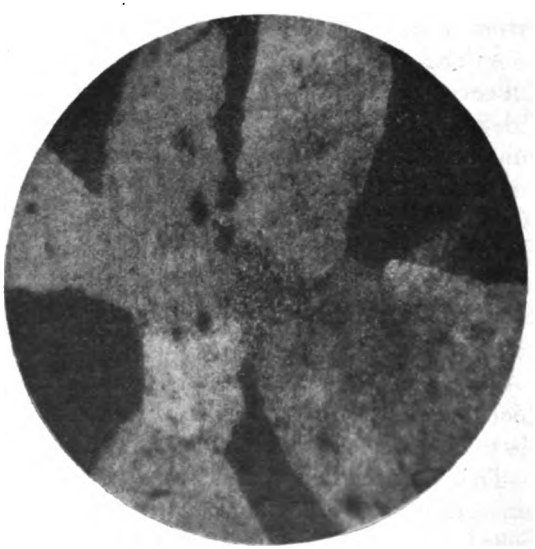

FIG. 4.—THE SAME AS IN FIG. 3 WHEN NEARLY ALL OF THE SMALL GRAINS HAVE BEEN ABSORBED. × 18.

The effect of thickness of section on grain growth is general in metals. The author has found that wires less than about 0.01 in. in diameter

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have two regions in which fast grain growth takes place. One occurs at the ordinary germinative temperature. The other occurs at most any temperature above the germinative temperature, the time varying inversely as the temperature. In the second fast growth range the large grains almost invariably first form near the surface of the sample. It is the writer's opinion that in such cases, where the ratio of the surface grains to the interior grains is large, the former, being unbalanced by having only one side in contact with adjacent grains, are readily absorbed by these interior adjacent grains. When the size of the surface grains due to this cause is enough greater than the equilibrium interior grains the latter are absorbed. The formation of large grains caused by small diameters of wires can be materially checked by the introduction of foreign bodies such as non-metallics. The practical effect of the introduction of non-metallics is to decrease the diameter of wire above which coarsening does not take place. In wires having a diameter of less than about 0.002 in. it is doubtful whether the formation of grains extending across the diameter of the wire can be prevented by any means, provided the wire is exposed for a long time at a sufficiently high temperature.

Fast Growth Phenomena in Low-Carbon Steel

What has preceded in this paper has considerable bearing on the formation of coarse grains in low-carbon steel. Referring to Prof. Sauveur's Fig. 27 and Chappell's Fig. 12,[7] there is a good reason why the coarse grains in the latter should be more columnar. In Sauveur's Fig. 27, as the large grains grow toward points of less cold deformation they encounter larger and larger grain fragments. This will be recognized as an adoption of Dr. Mathewson's ideas and nomenclature. The obstruction to grain growth offered by non-metallic globules is such that after the coalescence of the grains the globules do not change position but the grains grow around them. Assuming the same to be the case with pearlite as the obstructing medium (although the ferrite of the pearlite may be divorced, leaving only the cementite as the obstructing medium), the fragments which form in the interior of an original grain of ferrite would not have obstructing bodies at their interior boundaries. In the first stages of selective grain growth the grain fragments, being small, would be absorbed readily one at a time and when the obstructing masses of pearlite were encountered grain growth would easily take place around them. As the grain fragments increase in size their resistance to absorption becomes greater; hence, a point will be reached at which equilibrium exists. This is especially true when considering a definite time period.

On the other hand, as the large germinative grains grow at the expense

[7] C. Chappell: Recrystallization of Deformed Iron, *Journal of the Iron and Steel Institute*, vol. 89, pl. 48 (1914).

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of the small inert grains, the latter will have had more time to coalesce with one another as the time of sojourn in the germinative temperature range increases. It is well known that two adjacent grains which are not able to coalesce with each other in five minutes, might do so in five hours. Another factor which should not be lost sight of is that when a metal has been but slightly cold deformed, some of the adjacent grain fragments resulting may be in the same orientation and only be separated by a slip plane of amorphous metal. This amorphous material may be absorbed during heating by the crystalline metal, thus producing what is analogous to coalescence.

In Chappell's Fig. 12, however, the grain growth took place at a temperature well above the Ac1 point (870°C.) In very low carbon steels in which the mass of ferrite is much greater than that of the pearlite, there is considerable evidence that the pearlite does not change into the equilibrium percentage of austenite when heating above Ac1. Quenching experiments on samples which have been previously normalized, invariably show less martensite in the very low carbon steels than would be expected from the equilibrium diagram. When these same steels, according to Osmond,[8] are heated to very high temperatures—1200° to 1300° C.—larger percentages of martensite are formed. It is likely, therefore, that there is less austenite formed during the heating of these steels than would be expected from the equilibrium diagram. This is borne out by heating curves and by the occurrence of globules of free cementite. In fact, Chappell's own micrographs would lead one to this conclusion.

It is further probable that germinative grains which had reached conconsiderable size below the Ac1 point, would have greater power to divorce the ferrite from the pearlite than would smaller grains.* Consequently, while the pearlite or austenite might easily prevent the small grains from coalescing with one another, the large germinative grains might have sufficient power to absorb the inert grains. This is precisely what does happen in the case of a pure metal mixed with about 10 per cent. of its volume of a non-metallic material.

Prof. Howe's suggestion, that about 13 per cent. pearlite might easily offer sufficient mechanical obstruction to grain growth to completely defeat the selective action, is highly acceptable. Before leaving this phase of the subject, it would be interesting to consider the effect of the critical transformations in steel. If a piece of steel were heated by electric current to such a temperature that the inner portion were above the

*The writer has frequently measured the areas of pearlite in very low carbon steels, and almost invariably the percentage is less than would be expected from the combustion carbon determinations.

[8] *Trans.*, vol. 27, p. 879 (1897). A steel containing 10 per cent. carbon quenched from 1050° C. would consist of 60 per cent. free ferrite and 40 per cent. martensite.

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$A3$ point and the outer portion below the $A3$ point, a fast growth temperature condition should exist at the intersection of these two regions. Similarly, during cooling from above the $A3$ point if the inner portion were kept above the transformation point and outer portion below, a fast growth temperature condition should exist at the boundary. By the proper regulation of the heating rate or the cooling rate either large austenite grains or large non-gamma iron grains should be formed.

The existence of gases in metals cannot be neglected as a factor in the resistance to grain growth. It would seem, however, that if gas globules were responsible for the obstruction of grain growth, their presence could be determined by the aid of the microscope using high magnifications. That gases in solution or mechanically entrapped in metals may greatly affect the annealing temperature after cold deformation is shown quite conclusively by Rose[9] and Phelps.[10]

Effect of Cold Deformation on Uniformity of Orientation of Original Grains

Prof. Howe's statement that the uniformity of orientation after cold deformation is greater within the individual original grains than between adjacent grains, is no doubt generally true. It must be considered, however, that in cold-deformed metals there is sufficient difference in orientation within the original grains to be the chief cause of recrystallization on annealing. That we do not always see these differences in orientation may be due (1) to the fact that the diameter of individual grains in which we can study etching pits is usually not more than 0.0002 in. This distance is so small that variations in orientation might easily escape even a close observation. (2) To the fact that the sections (viz., longitudinal) which are usually examined in deformed metals are the most conducive to similar etching tints.

An experiment will demonstrate the points in question very clearly. Small wires 0.003 in. in diameter, were heat-treated in such a way that grains about ½ in. long were formed, each grain occupying the full section of the wire for that length. Pieces of this wire were bent sharply through an angle of about 90° in such a manner that the two ends of a single grain were not internally deformed. In other words, all of the deformation due to bending was confined to a small area near the center of an individual grain. It is obvious that the two ends of the grain in question now have decidedly different orientations. The exact angle of difference is known, namely, 90°. It is obvious that there is no sharp

[9] Thomas Kirke Rose: On the Annealing of Gold, *Journal of the Institute of Metals*, vol. 10, p. 150 (No. 2, 1913).

[10] John Phelps: The Effect of Hydrogen on the Annealing of Gold, *Journal of the Institute of Metals*, vol. 12, p. 125 (No. 2, 1914).

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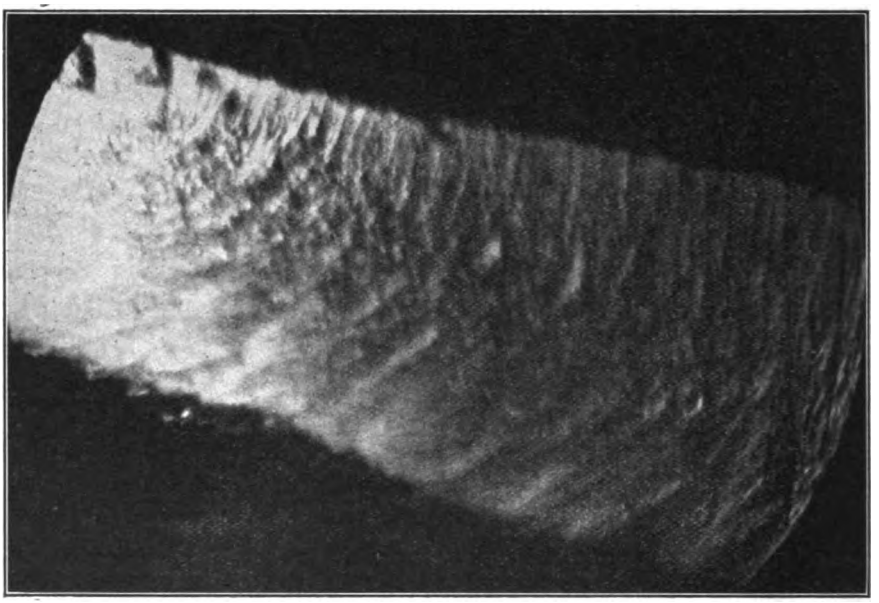
line of demarcation at the bend. The change in orientation is progressive. When a sample like the above is examined under the microscope the etching tint is usually quite uniform. However, if the same sample be subjected to a twisting operation the orientation has been changed with respect to different axes, and the etching tint does not remain uniform. Fig. 5 (660 diameters) is a photomicrograph of a sample of wire so twisted. The differences in etching tint can be readily seen.

The recrystallization properties in the bent sample are approximately the same as those in the twisted sample. As further evidence of decided change in orientation within the initial grains, Stead and Carpenter



A Single Grain, Twisted Cold. Shows Differences in Etching Tint. × 660.

demonstrate this condition very nicely by means of different etching tints within the same initial grain and by drawings (The Crystallising Properties of Electro-Deposited Iron, *Journal of the Iron and Steel Institute*, vol. 88, Figs. 6*A* and 6*B*, Plate 12. Drawings on p. 147 (1913).

No explanation of the recrystallization of cold-deformed metals on annealing has been forthcoming which did not assume a change in orientation within the initial grains due to the deformation. Rosenhain[11] seeks to explain the formation of the new grains by assuming that the amorphous material forms nuclei for the new grains. The study of saturated liquid solutions in the presence of the solid solute teaches us

[11] *Internationale Zeitschrift für Metallographie*, vol. 5, p. 65 (1913).

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that the solute in the crystalline state, and not the solute or solvent in the amorphous (dissolved) state, is the controlling factor governing saturation equilibrium. As an example, if a solution of sodium chloride in water is saturated at a certain temperature with no crystalline sodium chloride present, the dissolved salt contains a certain amount of potential energy. This potential energy is not changed in quantity by the addition of a sodium chloride crystal to the solution, but some of the dissolved salt crystallizes and the degree of concentration in the solution is lowered. The force necessary to change this degree of concentration must be looked for in the crystal of sodium chloride. In the presence of amorphous and crystalline metal, therefore, the crystalline fragments and not the amorphous layers will be the controlling factors in recrystallization.

Beilby[12] has demonstrated the above experimentally. This experiment is so basic in its general relationship to recrystallization that it should be repeated here in his own words. "Experiments have been made on the building up of an entirely amorphous mass of gold. Gold was precipitated from a solution of the chloride as a spongy brown powder. The washed and dried powder was compressed in a die after the gases it had contained had been pumped off at a temperature of about 150°. The pressure was applied by a small hydraulic press. This pressure was so great that the gold plug made a depression in the bottom plate of the die, which was of tool steel. The pressure must have been 60 to 80 tons per square inch. The gold squirted very slightly through a hole in the bottom plate, and also between the plunger and the sides of the die. The squirted gold was very hard and rather brittle, quite unlike pure gold prepared in any other way. The specific gravity of the compressed plug was 19.05, or rather lower than the specific gravity of gold. Its microstructure was granular and slightly spongy. This amorphous gold was certainly very much more rigid than even the hardest of cold-worked gold; it was almost devoid of plasticity. Another plug of compressed gold was made from the same powder, but a tiny spiral of very fine soft wire was dropped into the die before it was packed with powder. When a very moderate pressure was reached the soft gold squirted quite freely past the plunger, leaving the plug of hard gold behind.

"The first compressed plug was heated to 350° and showed no development of crystallization. In the absence of crystalline nuclei the amorphous form is stable even at this temperature. The squirted metal from the second plug which did contain nuclei crystallized freely at 350°."

Discussion of this paper on p. 589.

[12] G. T. Beilby: The Hard and Soft States of Metals, *Journal of the Institute of Metals*, vol. 6, pp. 36–37 (No. 2, 1911).

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On Grain Growth

BY HENRY M. HOWE, LL.D.,* BEDFORD HILLS, N. Y.

(New York Meeting, February, 1917)

THE brilliant and very original matter in Professor Jeffries' discussion† should rank not only as an independent paper, but as a most important one. In particular, the explanation which it gives of the remarkable Sauveur phenomenon, over which so many of us have puzzled long and hard, is so clear, complete, and cogent as to go far toward establishing Professor Jeffries' hypothesis. The very features of Professor Sauveur's photographs which fit this hypothesis so exactly are reproduced in Chappell's photographs of steel coarsened under somewhat different conditions.[1]

The ideas developed by these discoveries are so novel that convenience of language seems to call for new conventional terms for expressing them. In what follows here I propose certain terms for this end, for criticism and improvement, and I attempt to generalize from Professor Jeffries' hypotheses.

Generally speaking, a given set of grains which undergo no growth in the cold will, when the temperature is raised progressively, start to grow on reaching a certain temperature, and will continue growing at all higher temperatures.

The "germinative" temperature is that at which growth begins, quite as the conditions which cause a seed to grow are "germinative." We have then an *inert temperature range* below the germinative temperature, and a *growth range* above it, the germinative temperature being the limit between these two ranges. So, too, a germinant grain is one which is at its germinative temperature, and hence beginning to grow, while inert grains are those below their own germinative temperature, and the term "growing grains" includes those which are germinant and those which are above their germinative temperature. These last could be called "supergerminant" if the distinction should be needed.

"Germinative temperature" seems better than "critical temperature

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†Of the paper of C. H. Mathewson and Arthur Phillips, *Trans.*, vol. 54, p. 658 (1916).

[1] *Journal of the Iron and Steel Institute*, 1914, No. I, Plates 46 and 47, following p. 496.

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for grain growth" and "equiaxing temperature," first because "critical" has been so much overworked and suggests neither critically fast nor critically slow growth, and second because a germinative temperature is what the hypothesis postulates, and because the further hypothesis that this coincides with the equiaxing temperature is only an inference and for the present purpose an unnecessary one. Indeed, Professor Jeffries[2] informs me that he has found cases in which the germinative temperature is far above that of equiaxing.

When only a limited part of the metal is at its germinative temperature, this becomes a "fast-growth" temperature, because the germinant grains feed unopposed on the inert ones against which they abut.

The late results of White and Wood[3] tend to complicate our ideas, for they indicate that germinance depends not alone on temperature but also on time, so that germination may occur on a long exposure to a temperature at which none can be detected after a shorter exposure. This indeed agrees better with our general knowledge of "aging." In this view inertness is not absolute but only relative.

Grain growth, that is the absorption of certain grains by certain others, is favored, and the *germinative temperature* is consequently lowered, (*A*) by grain fineness, (*B*) by grain-size contrast, and (*C*) by prior plastic deformation.

(*A*) *Grain fineness* favors grain growth because it implies extensive grain boundary surface at which alone can absorption occur. To vary the angle slightly, a growth which doubles the size of extremely fine grains means only that each alternate very fine grain absorbs its adjoining still smaller neighbor, a much faster process than the absorption of one enormous grain by its even greater neighbor, faster whether we hold that the whole of a given grain is absorbed simultaneously, or, as seems more probable, that the swallowing is gradual, as for instance by some kind of boundary migration.

(*B*) *Grain-size contrast*, that is the difference in size between adjoining grains, favors grain growth because the absorptive and the resistant powers of a grain increase with its size, so that, when there is great grain-size contrast, growth is favored jointly by the great absorptive power of the larger grains and by the weak resistance of the smaller ones.

(*C*) *Plastic deformation* or *overstrain* favors grain growth, perhaps by leading to fineness of grain and to grain-size contrast. The fact that the germinative temperature is the lower the greater the deformation has been tallies with the former explanation, because increasing deformation would naturally lead to finer grain size. It may also increase the grain-size contrast. But the fact that grain growth will not occur at all unless

[2] Private communication, June 24, 1916.

[3] Recrystallization as a Factor in the Failure of Boiler Tubes, *Proceedings of the American Society for Testing Materials*, vol. 16, No. 2, p. 82 (1916).

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there has been plastic deformation suggests that some additional cause applies here.

(*D*) Grain growth occurs the more readily the higher the temperature, and the greater consequently the mobility.

Grain growth is opposed, and consequently the germinative temperature is raised, by:

(*E*) Obstructions, such as cementite, slag, and other sonims in iron, and thorium oxide in tungsten; and by

(*F*) Grain-size equalization, or the decrease of grain size contrast.

The rate of growth increases:

(*G*) With the difference in absorptive power between adjoining grains, whether this is due to a difference in their:

(*H*) Temperature; or in their

(*J*) Size, that is to grain-size contrast; or in their

(*K*) Prior plastic deformation. It further increases:

(*L*) With the temperature;

(*M*) With the initial grain fineness; and

(*N*) With the prior plastic deformation.

(*P*) The rate of growth decreases as the grain size increases, because this increase implies a corresponding decrease in the extent of grain boundary surface.

The very fastest growth seems, from Professor Jeffries' results, to be due to (*H*), which is thus an extremely strong influence. From the fact that low temperature, if it does not arrest growth, at least retards it very greatly, we must admit that (*L*), too, has great influence. Further light on the strength of (*J*), grain-size contrast, is needed. Whether exact identity of grain size would arrest growth completely if the temperature and the degree of plastic deformation were identical, seems hardly capable of proof, because that identity can never be attained.

The reported cases of grain-size equilibrium, in which growth is reported to cease, seem to me more easily explained as resulting from (*P*) the progressive decrease in the extent of grain boundary, and from (*J*) a progressive lessening of the grain-size contrast as the smaller grains are eliminated successively, and a progressively smaller number of larger ones remains. From this point of view the apparent arrests of grain growth and the cases of so-called "grain-size equilibrium" are to be regarded as only retardations, however marked these may be.

Grain growth may be divided into the *normal* and the *exaggerated*. In most industrial heatings there is normal grain growth, that is to say a coarsening which is leisurely unless the temperature approaches the melting point. In strong contrast to this is the exaggerated growth noted by Stead, Carpenter, Sauveur, Ruder, Chappell, and Jeffries, which may be extremely rapid and may lead to extreme coarseness.

If we leave out of consideration temperatures approaching the melting

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point, exaggerated grain growth seems to occur only at the contact of germinant and inert grains. Thus in Jeffries' experiments accelerated coarsening occurs only and always in case the mass is held under a current of such a strength as to bring only part of it to the germinative temperature, the outer and colder parts being inert because below this temperature. Here growth occurs at the germinant internal surface, the germinant grains feeding on the colder inert ones next outside them, thus increasing in size and thus creating a grain-size contrast, which according to Principle (*A*), enables them to feed on the still more inert grains still nearer the outside, and also to begin feeding on the neighboring grains nearer the axis, which, though growing because they are in the growth range of temperature, are growing more slowly because the grains on which they are feeding are themselves active, and thus offer more resistance to absorption than the inert grains against which the grains in the germinant layer abut.

Turning from this case, in which the contact of germinant and inert grains is brought about by the existence of a progressive change in temperature from shell to axis, we have the Sauveur cases in which it is due to a variation in the degree of plastic deformation which the different layers have undergone. When a piece which has been bent or has received a Brinell impression is next heated to a temperature which is at or above the germinative temperature of the most deformed layers, the germinative temperature of some one layer, or in case of a bent bar of some two layers about equidistant from the neutral axis, will equal this existing temperature. This layer and all the more deformed ones will begin growing, but the germinant layer will grow faster than the others, because it feeds on the inert because less deformed layers beside it.

A probably more accurate statement is that exaggerated growth occurs where there is a sharp increase of inertness from layer to layer. Here the grains of a certain layer are so much more active than the more inert ones in the adjoining layer against which they abut that they have a marked advantage as regards germinative power. The lead which this advantage gives them increases progressively as growth proceeds.

The experience of the wire-drawers, that though annealing often causes marked coarsening after the slight deformation of the wire-straightening process, it does not after the extreme deformation of industrial wire-drawing, conforms exactly with Professor Jeffries' hypothesis.

Here we may suppose that, after wire-drawing, the germinative temperature is so low that in the early stages of the heating up it is passed through too rapidly to permit appreciable grain growth at this relatively low temperature, whereas after the slight deformation of straightening the germinative temperature may well lie at the relatively high temperature of annealing, at which first the sojourn is long, and second the rapidity of growth is relatively great. The reason why extremely slight deforma-

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tions do not lead to coarsening on annealing is that the position of the germinative temperature which they determine is so high that it is not reached in annealing, if indeed it does not approach closely the melting point of the metal.

When a principle explains so much, the existence of certain things which it does not explain argues, not against its truth, but for the existence of additional principles not yet discovered. The two difficulties which I now raise should be taken in this sense.

My first difficulty concerns the Sauveur specimen of Professor Jeffries' Fig. 2, Professor Sauveur's Fig. 30. In this long stay at the germinative temperature of the outer parts of the coarsened bands, the grains in these bands should have fed to an important degree on the inert fine-grained core, and their resultant growth, proceeding as it should at right angles to the contact between the fast-growing band and the central core, should have developed columnar grains. Yet in fact the grains look equiaxed. On the other hand, such a columnar arrangement is prominent in the Chappell specimen just referred to.

My second difficulty is that this five-banded arrangement, with two outer bands of intermediate coarseness and a central fine one, should be the rule and not the exception, and should not have passed undetected the vigilant scrutiny of so many observers, for the coarsening is easily visible to the naked eye, and should not have had to wait till 1912 for its discovery. Thus the Sauveur coarsening is lacking in so large a proportion of cases as to suggest that it is held in check by one or more additional principles. I suggest the obstructing effect of foreign bodies as a possible explanation of one set of cases in which coarsening is lacking, a set illustrated by Professor Sauveur's finding that marked coarsening does not occur unless the carbon content is between 0.04 and 0.12, and that it occurs most readily when the carbon content is between 0.05 and 0.07 per cent.[4]

The presence of 0.12 per cent. of carbon, implying that of 13 per cent. of pearlite, might well offer enough mechanical obstruction to prevent coarsening, for each pearlite mass would act as a wholly foreign body to bar the union of the two grains between which it lies. Even as little as 0.07 per cent. of carbon, implying the presence of about 8 per cent. of pearlite, might have an important obstructive effect. On the other hand, when the carbon content is less than 0.04 or 0.05 per cent., there may well be enough iron oxide or minute gas bubbles to offer serious obstruction. Here we should remember Benedicks'[5] discovery that though the specific volume of his steels was strictly proportional to the carbon content between the limits of 0.45 and 1.20 per cent., that of his iron with 0.08 per

[4] H. M. Howe: *The Metallography of Steel and Cast Iron*, p. 363 (1916).

[5] Carl Benedicks: Recherches physiques et physico-chimiques sur l'acier au carbone, pp. 2, 30 and 35. Paris, 1904.

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cent. of carbon and 0.03 per cent. of silicon was abnormally great. It is true that this last was wrought iron, and hence that its specific volume would be made abnormally great by the presence of slag. But with only 0.03 per cent. of silicon the slag content should be extremely small, probably not more than 0.20 per cent., and hence insufficient to explain the surprisingly small density, which I refer therefore to the presence of minute gas cavities so often present when the carbon content is so small.

This brings us to a hitherto unsuspected element of fitness for man's needs which iron has, that the coarsening of its ferrite is limited to temperatures so far below the melting point as to give great viscosity, and hence to restrict the coarsening tendency. On rising past *Ac*1, the 8 per cent. of pearlite which we have just considered turns into 8 per cent. of austenite, and this increases very rapidly in quantity, till at *Ac*3 it forms the whole. As it increases in quantity it forms a more and more complete obstacle to the coarsening of the ferrite with which it is now associated, so that in fact the coarsening of ferrite, even in lower-carbon steels, is probably confined to temperatures below 800°, and thus many hundred degrees below the solidus, and hence corresponding to great viscosity.

Moreover, we may suspect that at *Ac*2 the change from alpha to beta ferrite, which Burgess and Scott[6] seem now to have established clearly, will break up any coarsening which has occurred in the alpha ferrite below this temperature. This, if true, would confine the coarsening range to below 768°.

These considerations lead us to expect to find iron less subject to coarsening than most metals.

It is true that Professor Jeffries' hypothesis explains satisfactorily and without needing this obstruction principle certain other cases of unexpected failure to coarsen, for instance that of my tapered bar of steel of about 0.01 per cent. of carbon from the American Rolling Mill Co. This, after straining with tensile stresses varying from 40,640 to 49,000 lb. per square inch, failed to coarsen noticeably in a 22-hr. exposure to 680°. Because the yield point of this steel is only about 20,000 lb. per square inch, even the least of my stresses may well have caused a degree of deformation so great that the corresponding germinative temperature was far below 680°, so that, in heating up to 680°, the germinative temperature of each of the various parts was passed through too rapidly to lead to material coarsening.

Yet this explanation hardly applies to Professor Sauveur's failure to coarsen his bent specimen in a 7-hr. exposure to 650° in case the carbon content is less than 0.04 per cent., for here there should be layers near the neutral axis in which the plastic deformation should vary progressively from nil to far above that for which the germinative temperature is 650°. Somewhere in this progressive series there should be a layer with

[6] *Comptes Rendus*, vol 163, No. 2, p. 30 (July 10, 1916).

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the deformation for which 650° is the germinative temperature, and this layer should have coarsened at the expense of its less strained neighbors nearer the neutral axis. But here the failure to coarsen may be explained by the suspected presence of gas bubbles, to which such low-carbon steel is so subject.

The important discovery that, though long heating at a low temperature might cause great coarsening, yet a quick passage up to a high temperature might prevent it, was communicated to me by Professor Jeffries on July 1, 1915, and is recorded, together with most of the information on which his present remarks are based, in a report of his which I have read, dated May 14, 1915.

Turning to Professor Jeffries' contention that uniformity of etching tint is not valid evidence of uniformity of orientation within any given grain, though he would be right if he confined himself to saying that the retention of a uniform etching tint by a single grain is not proof positive of uniformity of orientation throughout, yet when each of many grains in a single field retains a uniform etching tint which differs from that of its neighbors, the inference is irresistible that there is a far greater approach to uniformity of orientation within each grain than between adjoining grains. The fact that certain specific rotations need not alter the brightness of the reflection is beside the mark. What is important is that the great majority of random rotations will alter it.

The irregularity of the slip bands and X bands, in the absence of corresponding irregularity of etching tint, is explained more readily as representing the effort of slip to integrate into a line parallel to the major stress, by stepping back and forth from one to another of two or more sets of conjugate slip planes, than as representing irregularity of orientation. The fact that this irregularity of the slip bands, from being negligible in the first stage of deformation, increases as deformation proceeds, accords with my interpretation, for whereas increasing deformation does not cause any corresponding irregularity of etching tint, it ought to facilitate this change from set to set of planes, by increasing both the frequency and the thickness of the amorphous sheaths surrounding the residual crystalline blocklets, of which the crystallographic slip planes are the paths of that slip. For whatever tendency slip has to persist along a given plane as long as it is passing through crystalline matter is clearly lessened when that slip comes to the amorphous sheath which parts each crystalline grain-fragment from the next.[7] The regularity of the etching pits in each grain even after extreme plastic deformation and the sharp change in their direction from grain to grain are very cogent evidence of the retention of substantial uniformity of orientation during deformation.[8]

[7] Howe: *The Metallography of Steel and Cast Iron*, pp. 316, 483.

[8] *Op. cit.*, pp. 287, 485.

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Discussion

Discussion of the papers of Henry M. Howe pp. (561, 582) and Zay Jeffries, (p. 571).

W. E. Ruder, Schenectady, N. Y.—In 1913 I presented a paper before this society on Grain Growth, and at that time it seemed to me that the only explanation for grain growth was that of critical strain. I do not think I published the photographs at the time, but I made some experiments on the effect of stretching sheets of metal. This allowed for fairly accurate determinations of the amount of cold deformation. At the time I worked with silicon steel because one could see the change in grain growth with the naked eye. I stretched about 10 samples from 0.5 per cent. to 10 per cent., and, after heating to 1,050° C., I found that the maximum grain growth occurred at about 2½ per cent. cold deformation. This I designated "the critical strain" for maximum grain growth. About a year ago I talked with Prof. Jeffries, who explained to me his ideas of grain growth. It occurred to me that it would be a very easy thing to check the strain hypothesis against the strain and temperature hypothesis, if we took a certain number of these strips, strained exactly alike and heated to different temperatures; in that case the maximum grain growth ought to appear at a different percentage of deformation for each temperature. These experiments showed that there was, as Mr. Jeffries shows in his present paper, a decrease in germinative temperature with increased cold deformation. The samples were stretched from ½ to 8 per cent. and heated for 10 hr. in hydrogen. Samples were introduced into a furnace previously heated to temperature. A maximum grain growth was obtained at 5 per cent. deformation, when heated to 750° C.; at 2½ per cent. when heated to 800° C.; at 1¼ per cent., if heated at 950°C.; and at ⅝ per cent. when heated at 1,100° C. This shows very clearly that the germinative temperature decreases with the increase in cold deformation. The growth was very much more marked, however, at the high temperature, *i.e.*, the grain grew more rapidly and attained a much larger size than at lower temperature. In considering the photographs (Fig. *A*) those along the lateral edges should be ignored because they were started by the strain gradient set up by the cutting of the strips. The amount of strain so induced is of course not measurable. Another point which was brought out by Prof. Jeffries' hypothesis is shown in one of the photographs (Fig. 14) which I published at that time. This was the only sample upon which I tried the experiment of the effect of temperature gradient, and in that experiment I placed a piece of sheet steel in the furnace which was at a temperature of 1,340° C., at the hottest zone, and was graduated from there on out to room temperature at the cold end. In this case I found that I always got an enormous grain growth in the range of about 900–1,100° C., and always at the same point,

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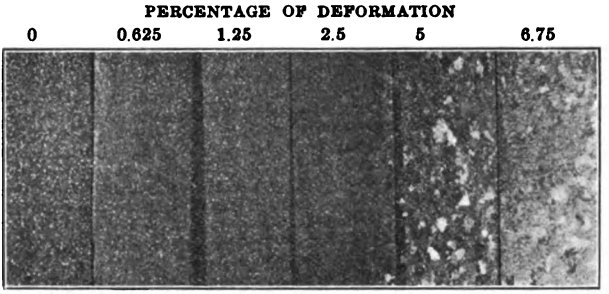
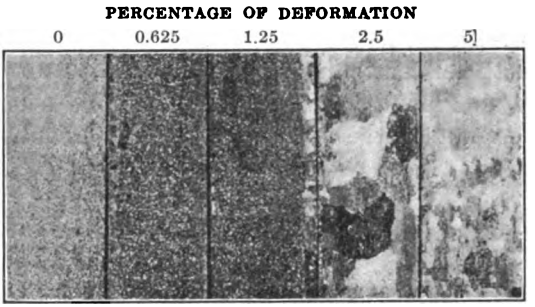
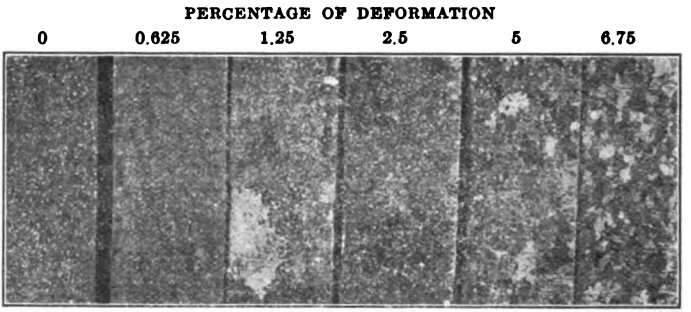


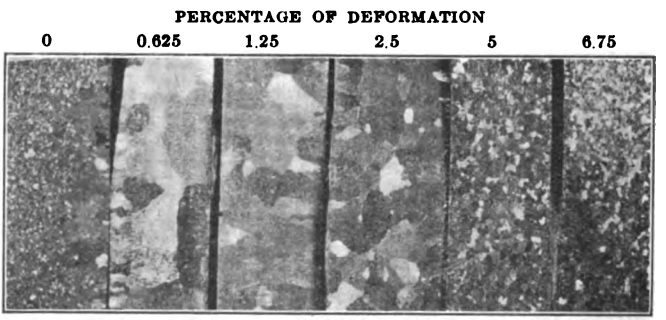
ANNEALED AT 750 °C. FOR 10 HR.



ANNEALED AT 800 °C. FOR 10 HR.



ANNEALED AT 950 °C. FOR 10 HR.



ANNEALED AT 1100 °C. FOR 10 HR.

FIG. A.

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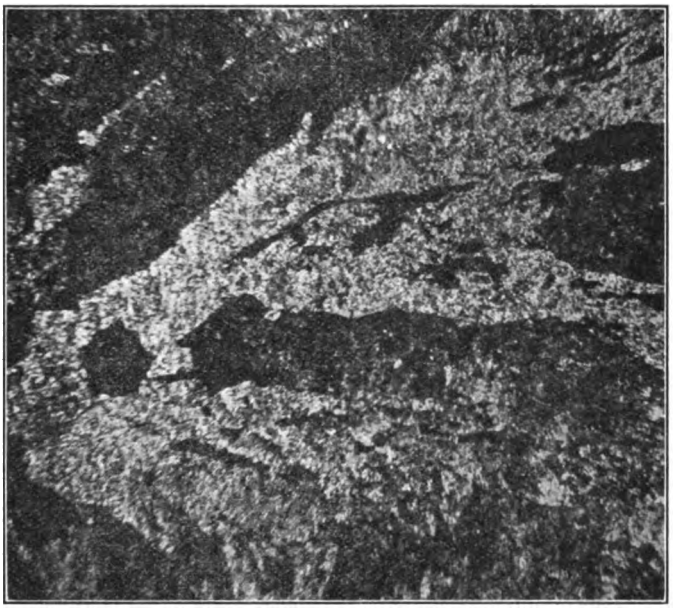

FIG. B.—PRESSED ELECTROLYTIC IRON ANNEALED AT 1000 °C. FOR 3 HR. × 50.

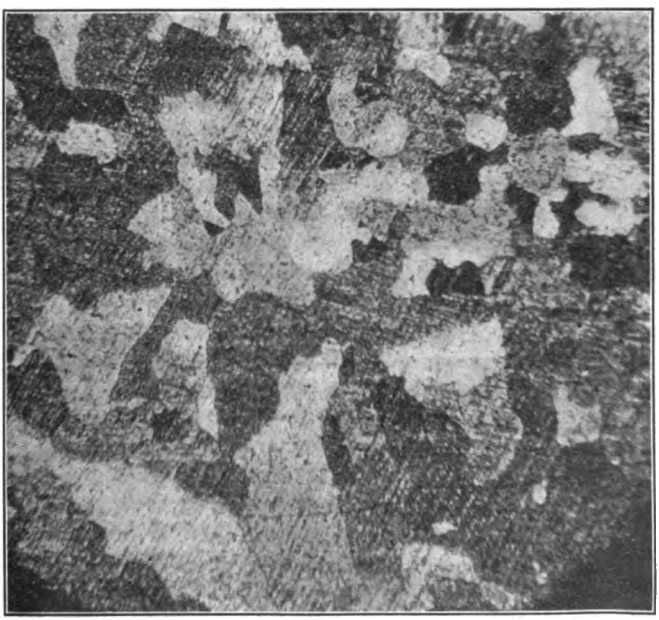

FIG. C.—SAME SAMPLE AS IN FIG. B. REHEATED TO 1250–1300° C. FOR 3 HR.

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in any number of pieces I put in. The grain growth in this case was always much greater than it was in any other heating. This, of course, falls in with Prof. Jeffries' hypothesis, or is easily explained by it, because at the point of germinative temperature, the grains had an enormous number of inactive grains to feed upon, *i.e.*, those that were below the germinative temperature; and, therefore, they always formed enormous grains. There is one case I have not been able to explain altogether and that is the formation by cold deformation of small grains inside of large grains (cf. Fig. 9, *Trans.* (1913), **47**, 569–86). I found at that time that these small grains never form except at a high temperature (1,050° C.) a temperature much greater than that at which the original strained sample began to show growth (725° C.). I am at a loss to see exactly why the large grains which completely surrounded the small ones did not re-absorb the grain fragments at this high temperature. Another case which came up recently was one of which I was reminded by Prof. Jeffries' reference to pressed metals; namely, in case of a slug of pressed pure electrolytic iron powder, I was able to get large grains in the pressed metal by heating at 1,000° (Fig. *B*), but this large grain was broken up on further heating to 1,300°—broken up into smaller, more even grains, which we might call normalized grains (Fig. *C*). I do not know why they should change at that high temperature, particularly since the first heating was above the critical range.

Frederick E. Carter, New York, N. Y.—I am interested more particularly in this question in its application to platinum. Until now very little has been done on the platinum group of metals, one reason being that it has been very difficult to get pure metals. I am fortunate enough to be able to get them pure, and have just started some work on the metallography of the subject. Some preliminary work on the subject of the platinum alloys which I did with Prof. Campbell, of Columbia University, fits in nicely with some points that have been raised here. I found that with pure platinum you readily get recrystallization at about 1,000° C. The platinum-iridium alloys, however, will not recrystallize at that temperature, and even at 1,100 and over, recrystallization only slowly takes place, the temperature depending on the percentage of iridium, *e.g.*, alloys of 25 and 30 per cent. iridium require a higher temperature for annealing than 10 or 15 per cent. iridium. I also obtained a regular series of crystal sizes, the higher iridium alloys showing much smaller grain. That is, of course, to be expected. I have been led to believe, too, that there seems to be a maximum size for these crystals. With the alloy containing 25 per cent. iridium the grain does not seem to grow even with continued heating, and the grain of the alloy with 30 per cent. iridium does not grow with continued heating to the same size as that of the 25 per cent. alloy. Just over 1,150° there is

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no change at all in these higher alloys, so it must mean that their "germinative temperature" is between 1,100° and 1,150°. Does it mean that at just over 1,150° I have reached the temperature at which the grains of the higher alloys would no longer increase in size?* Several points have come up that were of interest to me, like the gas content of the metal and the temperature gradient tending to cause an increase in the size of crystals; these of course are very important points in the use of thermocouples. A good deal of what I have heard today aroused my curiosity and gave me much information which may be applied to studies of the behavior of the alloys of the platinum group.

J. W. RICHARDS, South Bethlehem, Pa.—I think metallurgists will find some new methods of studying grain growth if they take cognizance of the advances recently made in the study of crystals by the Roentgen rays. Most of you know that the Nobel prize was presented to Prof. Bragg, of Cambridge, for investigation on the structure of crystals by the Roentgen rays, and we actually know many times more about the structure of crystals through this investigation than was ever known before; in fact, the actual molecular structure of the crystals is now known. I think the use of these methods might obviate the necessity of depending upon the uniformity of etching tint to find the orientation of the crystals. Recrystallization *evolves* energy, as Mr. Jeffries has well said, and therefore it should take place very rapidly at the germinative temperature, if the temperature is not passed through too rapidly, because it is an exothermic reaction, and therefore has the high reaction velocity characteristic of exothermic reactions.

THE CHAIRMAN (H. M. BOYLSTON, Cambridge, Mass.).—There is one little point that occurred to me in connection with Prof. Jeffries' remarks about the speed with which grain growth can take place. He says that the probable reason that, in Prof. Sauveur's bent bar, the outside layers on the convex side of the bend did not show any growth was that they had gone too rapidly through the germinative temperature. I am quite certain that his pieces were heated from the cold, in a cold furnace, so that the heating to 650° must have been comparatively slow. It seems difficult to realize that growth can take place so quickly as to cause enormous grain size in 6 sec. at the germinative temperature, while that piece

* As the result of further experiments, I desire to correct my statement that, at 1,150° C., the grain sizes of platinum and iridium alloys remain constant even with continued heating. I have since submitted them to a further prolonged annealing and have found the grains showed a small growth; the grains are, therefore, still within the germinative range.

In connection with this discussion of grain growth, it is interesting and suggestive that precious-metal thermocouples become defective due to crystallization when kept for long periods at about 1,100° C. more readily than when they are used at either higher or lower temperatures. F.E.C.

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failed to show any growth. Prof. Sauveur's bent bar must have been kept for at least 6 sec. at most of those intermediate temperatures between room temperature and 650°.

J. W. RICHARDS.—Is it not possible that the growth may have occurred during the heating of the metal and been subsequently destroyed by the higher temperature?

ZAY JEFFRIES.—No, that could hardly have taken place, because, in Prof. Sauveur's experiments, the temperature was never raised above 650° C., and the breaking up of the coarse grains due to allotropic modifications occurs only in the region near 900° C.; but the explanation for Mr. Boylston's apparent exception in this case is very easy: In the first place, this remarkable growth in the 6-sec. period which I have previously mentioned, occurred at a high germinative temperature. By special means of introducing obstructions to grain growth, we can change the germinative temperature and even raise it so that theoretically it would be above the melting point of the metal. As the temperature of the germination increases, the speed of the growth increases very rapidly. In a certain example it took an hour and a half, or thereabouts, to cause considerable coarsening in a particular kind of metal at a relatively low germinative temperature; whereas it took about 1 min. at a relatively high temperature, to form very coarse grains with the same metal. The difference between these two samples was that certain non-metallic obstructions were used in one case to raise the germinative temperature, so that these enormous speeds of coalescence of the grains took place only at a relatively high temperature. If the germinative temperature is low, as is the case in Prof. Sauveur's samples, then the rate of coalescence of the grains is slow, and that partially accounts for the absence of coarseness in the more severely deformed portions of his samples.

In my previous remarks, I should have mentioned another important factor, namely, with increasing degrees of plastic deformation, coarsening by means of furnace heating becomes more difficult. Prof. Sauveur's samples were heated in a furnace. Furnace heating practically eliminates the temperature gradient from axis to surface, of the piece of metal. As the degree of deformation increases, the strain gradient decreases; *i.e.*, the recrystallization temperatures in different portions of the same deformed sample become more nearly the same, for instance, in severely cold deformed metals, the recrystallization temperature, or germinative temperature, of the surface of the sample is the same as the germinative temperature of the axis, so that, if the temperature is uniform from surface to axis, we can get no marked grain-coarsening condition.

I can explain, I think, Mr. Ruder's inclosed grains along this same line of reasoning. He mentions that at high temperatures he usually gets small grains inclosed within large grains. The fact that a grain is

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small and abuts a large grain, does not necessarily mean that that small grain must be absorbed. The general rule is that it will be, but it may have certain characteristics which permit it to exist as an individual grain, even in contact with a larger neighbor, such as Holland abutting Germany. It may coalesce after a considerable time, but the fact that it exists as an individual small grain in the interior of a large grain is explained, in my opinion, in this manner: The very high temperatures which Mr. Ruder used would cause remarkably fast growth. I imagine that a few seconds would be sufficient, in certain of the instances which he cites, to cause a very large area of the metal with which he was working to form into one grain. Let us call this a germinant grain. Supposing that another grain had started to form as a germinant grain, not far distant from this particularly large and active grain; this second germinant grain would have reached a size which would make it somewhat resistant to coalescence with the larger grain but yet might not reach such a size that it could absorb the adjacent smaller inert grains as rapidly as could the larger, more active, germinant grain. In that event, the larger, more active, germinant grain would absorb the inert grains at a rapid rate and would advance its boundary toward the smaller germinant grain. Meeting a temporary obstruction in this smaller germinant grain, due to its rather large size, when compared with other adjacent grains, the larger germinant grain would actually grow around the smaller germinant grain and would even absorb the small inert or inactive grains surrounding the smaller germinant grain. In that manner there would be one grain entirely inclosed within another larger grain. The condition may obtain in which several small grains are inclosed within one large grain.

It seems that after a grain is entirely inclosed within a larger grain, the former becomes even more resistant to coalescence with the larger grain than when the two simply abutted. I attribute this to a balance of forces which act in all directions on the same grain at the same time and establish an approximate equilibrium; whereas, when a large grain abuts a smaller grain, there is not sufficient resistance in the smaller grain to withstand the force of the larger grain, thus producing a state of unbalance or unstable equilibrium and the only way to establish more stable equilibrium is for the smaller grain to be absorbed by the larger one.

I think that Mr. Ruder's experiment with pure iron can be explained according to the allotropic changes, and in that respect I would like to refer to some work by Stead and Carpenter,[1] on the grain size changes in electrolytic iron due to the allotropic modification which occurs at about 900° C. They found, I think, something similar to what Mr. Ruder seems to have found in this particular case. Heating to a high temperature, Mr. Ruder formed, not alpha or beta iron, but large grains of gamma

[1] *Journal of Iron and Steel Institute*, vol. 88, p. 119 (1913).

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iron; then, in cooling past 900°, these large grains of gamma iron broke up into non-gamma iron from several centers at the same time and hence caused a small grain structure of alpha iron which Mr. Ruder observed. On the other hand, when he heated to 1,000° C., he formed small gamma-iron grains, and on cooling below 900°, these small gamma-iron grains would form germinant centers of non-gamma iron and these germinant centers would absorb the small gamma grains as fast as the allotropic change took place from gamma to non-gamma iron. In my opinion, this is an ideal condition for a rather wide range of grain size with change in the thermal treatment due to the allotropic modifications and consequent change of structure due to the change from one modification to another.

Prof. Richards' remarks are very much to the point concerning Prof. Braggs' X-ray spectrometer. In my paper a year ago, I brought that out and advocated the study of these crystals and the study of orientation with the Bragg X-ray spectrometer. Unfortunately I have not personally been able to get an instrument with which to work, but I know that work is being done on that line at the present time in the United States and I think that it will yield some very excellent results.

Regarding the remarks of Mr. Carter on platinum and iridium alloys: There is a general tendency to form small grains in solid-solution alloys, such as the alloys of platinum and iridium, and the general law of the heightening of annealing temperatures or recrystallization temperatures, here applies up to a certain point at which will be found the maximum, but the grain size should usually increase above 1,100°C. with extended time at higher temperatures. It may be possible under the germinative temperature laws, to produce the largest grains at a temperature between 1,000 and 1,100. On the other hand, the purity of the substance may be such that the actual fast-growth temperature due to the germinative laws will be in the neighborhood of 1,200 or 1,300° C. or even higher than that, according to the special conditions. This matter of equilibrium grain size is not yet well understood. Prof. Howe puts the case very well when he states that it is doubtful. Some metals seem to change in grain size at a high temperature very much more rapidly than others. I have one particular series of tests in mind where I found after repeated tests practically no difference in grain size, as the result of heating for 30 sec. and for 10 hr. On the other hand, I have found other examples in which the grain size had changed enormously during the interval between 30 sec. and 10 hr. A grain may be in equilibrium when it is 10,000 or more times as long in one direction as it is in another, or it may be in equilibrium when it is exactly equiaxial. Probably the final structure with infinite time, would be toward one grain in all pieces of metal, regardless of shape or size, if the temperature were sufficiently high and the time sufficiently long. However, the rate of grain growth is so rapid at the beginning of recrystallization that in some metals an apparent equi-

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librium grain size seems to obtain after a few minutes heating or in extreme cases even a few seconds is sufficient. I call this an "apparent equilibrium grain size" not because no further grain growth will take place with extended time at the given temperature, but because such grain growth is so slow as to be negligible in the time periods ordinarily used in industrial heatings, such as annealing or heating to obtain definite structure.

JOHN A. MATHEWS, Syracuse, N. Y. (communication to the Secretary).* —I have read with much interest and profit the papers by Profs. Howe and Jeffries in regard to the phenomena of grain growth in metals which have been subjected to stress.

The authors rightly conclude that much work remains to be done, and, in connection with future work, I would offer the following suggestion as a method of approach which might yield quantitative information in relation to the effect of stress, time and temperature.

My suggestion is, that the metals to be examined be prepared in the form of torsional test-pieces, for example, in the form of 1-in. or 1¼-in. round bars, 12 to 18 in. long. These bars could then be twisted in the torsional machine, stopping the operation before fracture, and before any reduction in area had taken place at any particular point. In this way, an estimate of the stress could be had for all zones of metal from the center to the outside. The bars thus twisted could then be cut into disks, say, ¼ in. thick, and these disks could be further quartered or halved. In this way, a large amount of material that had been subjected to uniform conditions would be available for variable time and temperature experiments. The pieces could be examined, microscopically, lengthwise and transversely, and variable conditions, produced by differences of stress from the center to the outside, could then be studied, and very nearly the exact stress at each zone could be calculated.

I shall be glad to coöperate with either of the authors in performing the torsional work on any materials they care to submit.

ZAY JEFFRIES (communication to the Secretary†).—Having seen Mr. Ruder's micrographs of electrolytic iron, I am of the opinion that the tentative explanation offered verbally is correct. Mr. Ruder has informed me that both of the samples were cooled in the furnace. The structures as shown in Figs. *B* and *C* resulted from the last cooling through the temperature range near 900° C. Assuming that the rate of cooling through this range was the same in both cases, the differences in structure of the gamma iron in the two samples must have brought about, in some way, the variations in grain size and grain shape shown in Mr. Ruder's micrographs.

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* Received Mar. 6, 1917.
†Received Apr. 27, 1917.

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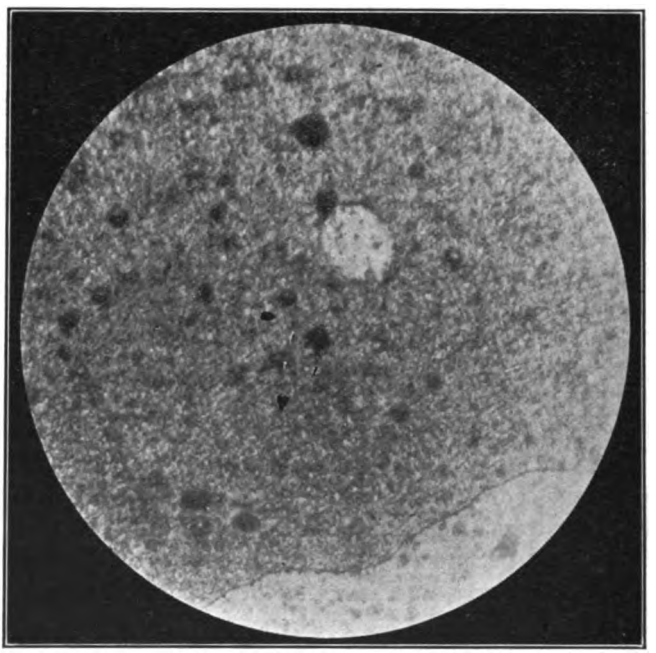
Even though there is practically no temperature gradient in muffle-furnace treatment during the "soaking" period, during both heating and cooling, there is a temperature gradient.

Fig. *B* should normally have been fine-grained prior to the last cooling, due to the comparatively low maximum temperature to which it was heated (1,000° C.). On cooling through the allotropic change point at 900°, these small gamma grains, in conjunction with the temperature gradient, should form an ideal condition for germination. Germinant grains should form near the surface of the sample (the colder portions) and the small gamma grains would be progressively absorbed as they changed into non-gamma iron.



[FIG. *D*.

The elongated grains in Fig. *B*, converging toward the axis of the piece of iron, indicate that germinant grains first formed near the surface and advanced their boundaries toward the interior of the sample.

The gamma grains in Fig. *C*, however, should have been large, prior to the last cooling, due to the high temperature treatment (1,250 to 1,300° C.). In other words, to use one of Prof. Howe's expressions, the crystalline organization should be very complete and the change from gamma to non-gamma iron on cooling would take place reluctantly. This resistance to change from gamma to non-gamma iron would probably be greater in the interior of the large grains than at their boundaries.

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Non-gamma iron centers or nuclei would probably form at a great many places, at the grain boundaries, thus masking the influence, to a large extent, of the temperature gradient. Apparently the effect of the temperature gradient has not been entirely masked in Fig. *C*, because some of the grains are elongated.

This hypothesis calls for a greater resistance to change from gamma to non-gamma iron, due to its crystalline organization, than the force tending to bring about the allotropic change, due to the interior of a grain being slightly colder than that portion of the boundary nearest the (hotter) axis of the sample. Small grains would function under these conditions as inactive grains and would be completely absorbed by germinant grains, while under the same conditions, large gamma grains would tend to partition into several non-gamma grains.

Regarding the point mentioned by Mr. Ruder of small grains isolated within large grains, I am submitting a micrograph of tungsten, showing this condition (Fig. *D*).

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Tungsten-Molybdenum Equilibrium Diagram and System of Crystallization

BY ZAY JEFFRIES,* B.S., MET. E., CLEVELAND, OHIO

(New York Meeting, February, 1917)

In this paper, it is proposed to outline a method for the determination of melting points of those metals and alloys having high fusion temperatures. The application of the method as used to determine the melting points of alloys of tungsten and molybdenum will be given. It will be seen from these results that tungsten and molybdenum form a completely

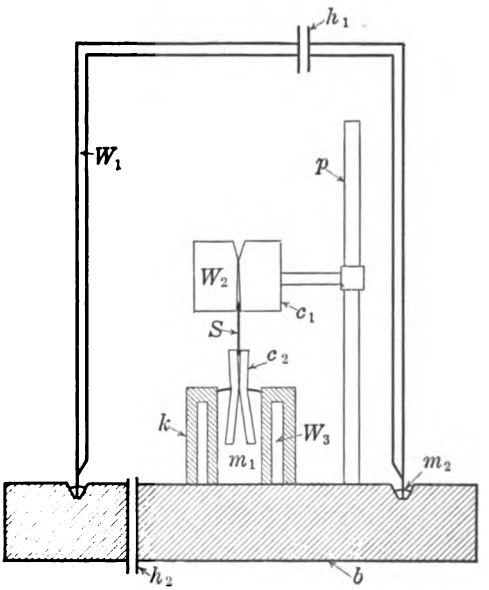

FIG. 1.—PARTIAL SECTION OF APPARATUS USED TO DETERMINE MELTING POINTS OF TUNGSTEN AND MOLYBDENUM ALLOYS.

isomorphous series. This is also verified by the accompanying photomicrographs.

Tungsten and molybdenum have been found to crystallize in the same system namely, isometric, and the crystal units are cubes. These determinations were made by examining etching pits in the pure metals.

* Case School of Applied Science.

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Description of Apparatus

Fig. 1 shows a partial section of the apparatus used for determining the melting points of the alloys. W_1 is a water-jacketed metal housing with a mercury seal $\bar{m}_2$ at the bottom. $\bar{h}_1$ is a hydrogen inlet and $\bar{h}_2$ a hydrogen outlet. $\bar{c}_1$ is the top electrode which is water-cooled, as indicated by W_3. This electrode is supported by the post $\bar{p}$, to which it is fastened by means of an adjustable sleeve. $\bar{c}_2$ is the bottom electrode which floats in a bath of mercury $\bar{m}_1$. This mercury is contained in the casting $\bar{k}$, which is water-cooled, as shown by W_3. The whole apparatus is mounted on the base $\bar{b}$ made of electrical insulating material. The electrodes are suitably connected to a current regulator, and the voltmeter and ammeter are properly inserted in the circuit.

Method of Making Determinations

Wires (S), $2\frac{3}{4}$ in. between electrode contacts and 0.030 in. in diameter, were inserted in the electrodes and hydrogen was allowed to flow through the inclosed chamber until the air was expelled. Electric current was then passed through the wire, starting with low amperage and gradually increasing until fusion occurred. The time of fusion is easily determined because the circuit is broken and the ammeter ceases to register. The readings of the voltmeter and ammeter are recorded with each increase in electric current so the fusion wattage can be accurately calculated.

Besides using samples of the same size, other conditions must be maintained constant; for example, the flow of hydrogen through the inclosed chamber, the flow of water through each of the water jackets, and the temperature of the water at the intake. The electrical resistance of the electrodes and their connections is so small that it can be neglected in the calculation for drop of potential when compared to the drop of potential between the two electrodes.

Calibration of the Apparatus using Samples of Known Melting Points

For calibrating the apparatus wires of pure platinum, pure molybdenum and pure tungsten were used. These wires were all of the same size and same length. The number of watts to fuse each is shown in Table 1.

TABLE 1

Material	Fusion Wattage
Platinum	144
Molybdenum	400
Tungsten	1,800

By considering the fusion wattage of tungsten as 100 per cent., the fusion wattage of molybdenum is 22.2 per cent. and of platinum, 8 per cent.

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The curve in Fig. 2 is obtained by plotting the following temperatures against the corresponding fusion wattage percentages:

TABLE 2

Temperature Degrees Centigrade	Per Cent. of Tungsten Fusion Wattage
20 (Room Temp.)	0.0
1,755	8.0
2,500	22.2
3,300	100.0

The temperature 3,300° C. is recommended as the most probable figure for the melting point of tungsten by Dr. Langmuir,[1] and also by the Nela Laboratories.[2]

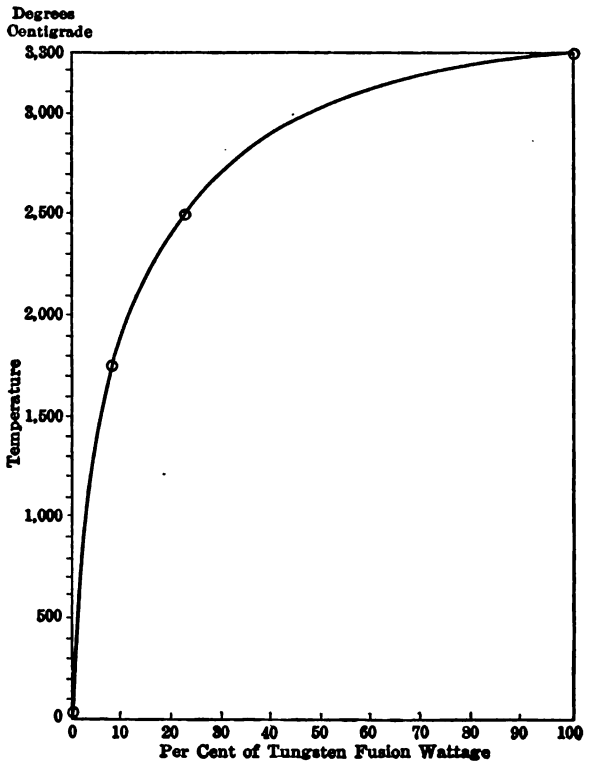

FIG. 2.—PER CENT. OF TUNGSTEN FUSION WATTAGE AT DIFFERENT TEMPERATURES.

If a sample of unknown melting point, having the same diameter and same length as the standardizing samples, be fused in the apparatus, its fusion temperature can be interpolated from its fusion wattage, from

[1] I. Langmuir: Melting Point of Tungsten, *Physical Review*, Series 2, pp. 138–157 (August, 1915).

[2] A. G. Worthing: The True Temperature Scale for Tungsten and its Emissive Powers at Incandescent Temperatures, *Journal of the Franklin Institute*, vol. 181, No. 3, pp. 417–418 (March, 1916).

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the curve shown in Fig. 2. A discussion will be found below outlining the possible sources of error in the determination of melting points by this method.

Results Obtained with Tungsten-Molybdenum Alloys

The results are shown in Table 3, and the points on the equilibrium diagram are shown in Fig. 3.

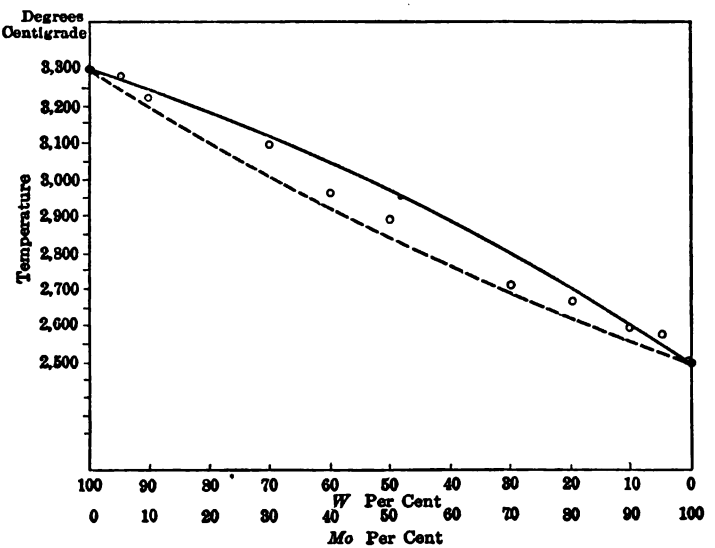

FIG. 3.—EQUILIBRIUM DIAGRAM OF TUNGSTEN AND MOLYBDENUM.

TABLE 3.—*Fusion Results Obtained with Tungsten-Molybdenum Alloys*

Molybdenum, Per Cent.	Tungsten, Per Cent.	Watts to Fuse	Per Cent. of Tungsten Fusion Wattage	Fusion Temperature, Degrees Centigrade
100	0	400	22.2	2,500
99	1	410	22.8	2,510
95	5	445	24.7	2,580
90	10	452	25.2	2,600
80	20	509	28.25	2,670
70	30	541	30.1	2,710
50	50	710	39.4	2,890
40	60	797	44.2	2,960
30	70	1,003	57.2	3,100
10	90	1,341	74.5	3,220
5	95	1,650	91.7	3,280
1	99	1,768	98.2	3,295
0	100	1,800	100.0	3,300

The solidus and liquidus curves are drawn tentatively, it being, of course, impossible to determine either of these curves by this method.

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The point of breakdown of the alloys under the action of the electric current probably lies between the solidus and the liquidus. Most of the points shown in the diagram in Fig. 3 lie between the proposed solidus and liquidus curves. The fact that two points lie outside of the area included between these two curves may be explained by saying that the accuracy of any of the determinations may be subject to an error of from 30 to 50° C.

As an example to show how the temperature is obtained from the fusion wattage, let us consider the alloy containing 50 per cent. tungsten and 50 per cent. molybdenum. The fusion wattage of this alloy was 710, and the fusion wattage of tungsten was 1,800.

$$\frac{710}{1{,}800} \times 100 = 39.4$$

This alloy fuses, therefore, at 39.4 per cent. of the fusion wattage of tungsten. Referring to Fig. 2, it will be seen that 39.4 per cent. of the fusion wattage of tungsten corresponds to a temperature of about 2,890° C.

The equilibrium diagram suggests that tungsten and molybdenum form a series of alloys which are completely soluble in each other, both in the liquid and solid states, and that the melting point of any alloy of the series will lie between the melting points of the end members. The photomicrographs confirm this indication.

Mennicke[3] observes that two compounds between tungsten and molybdenum occur, namely, W_2Mo_3 and WMo. The former would contain 79.3 per cent. tungsten and the latter 65.7 per cent. Samples closely approximating these analyses, as well as those containing more and less tungsten, have solid-solution structures. All of the evidence of the melting points indicates that tungsten and molybdenum form no intermetallic compounds.

Microscopic Examination of Tungsten and Molybdenum Alloys

Fig. 4 is a photomicrograph of nearly pure tungsten at a magnification of 320 diameters. The impurity in this sample is non-metallic. A few globules of this can be seen in the micrograph. This sample contains by analysis about 99.8 tungsten.

Figs. 5 to 7 inclusive are photomicrographs at 320 diameters of the various alloys of tungsten and molybdenum, as indicated in the accompanying descriptions. It can be readily seen that all of these alloys are solid solutions.

Judging from the micrograph (not here reproduced) alone, of an alloy of 20 per cent. tungsten and 80 molybdenum, the black areas might be considered to be due to a second phase; in fact, these regions represent

[3] H. Mennicke: *Die Metallurgie des Wolframs*, p. 262. Berlin, 1911.

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areas rich in tungsten. Under high power (1,200 diameters) the separation of the two phases could readily be detected. That they were not in

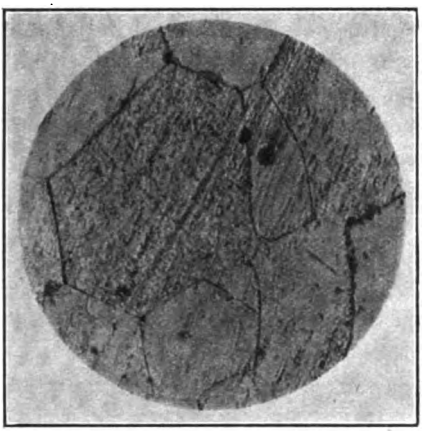

FIG. 4.—PURE TUNGSTEN. × 320.

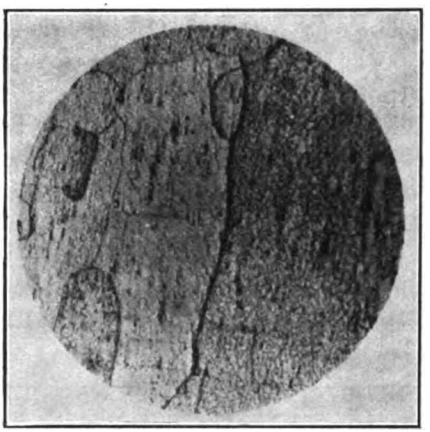

FIG. 5.—TUNGSTEN, 90 PER CENT.; MOLYBDENUM, 10 PER CENT. × 320.

equilibrium was also demonstrated by an additional heating which completely homogenized the alloy. In this connection, it would probably be advisable to mention the manner in which these alloys were made.[4]

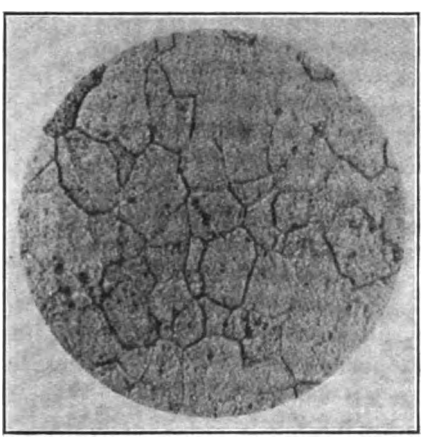

FIG. 6.—TUNGSTEN, 75 PER CENT.; MOLYBDENUM, 25 PER CENT. × 320.

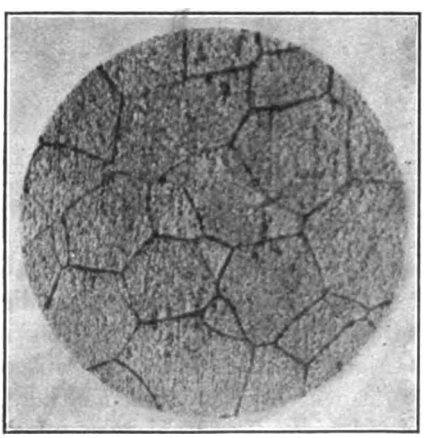

FIG. 7.—TUNGSTEN, 1 PER CENT; MOLYBDENUM, 99 PER CENT. × 3.20.

The oxides of both tungsten and molybdenum were obtained in a very pure powdered state, and reduced to powdered metal by hydrogen.

[4] The method of making and swaging these alloys was practically the same as described by R. W. Moore, *Metallurgical and Chemical Engineering*, vol. 12, p. 186 (March, 1914).

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The alloys were made up by thoroughly mixing the tungsten and molybdenum powders in the proper amounts. The mixed powders were then pressed into briquets which were sintered at about 1,300° C. in an atmosphere of hydrogen and were then heated by electric current to about 100° C. below the fusion points (which were previously determined with a separate series of briquets of these alloys) for a period of 12 min. The alloys, after receiving the above treatment, were completely crystalline. They may be ground by an emery wheel to a certain diameter, and the fusion points determined on these samples. They may also be heated and rolled or swaged to any desired diameter. The alloys used in these experiments were swaged to 0.030 in. diameter, the original cross-section of the briquets being about $\frac{1}{16}$ sq. in.

The micrographs, with the exception of Fig. 6 (which is a section of

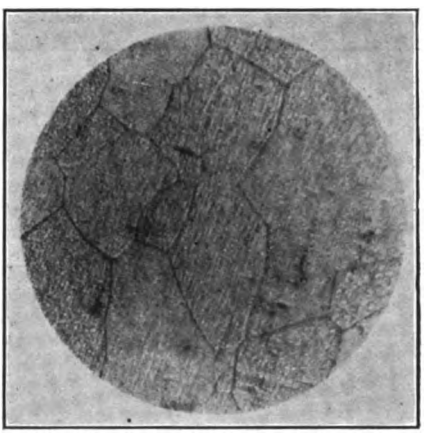

FIG. 8.—PURE MOLYBDENUM. × 320.

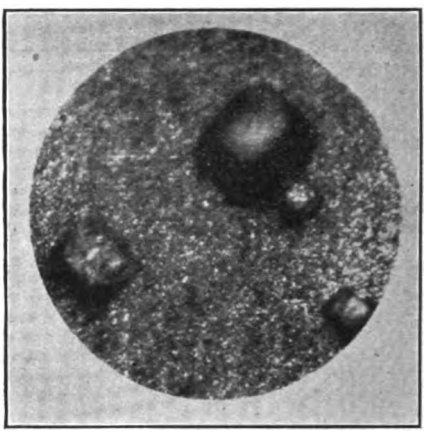

FIG. 9.—ETCHING PITS IN TUNGSTEN. × 775.

0.080 in. wire), represent longitudinal sections of these 0.030 in. diameter wires after fusion. The portions shown in these micrographs represent sections which have not been fused but which have been heated to temperatures very near their melting points. It will be evident that fusion of the wire will take place at a point approximately equidistant between the two electrodes and that adjacent portions of the wires would be heated to very near the melting point. In these sections, therefore, the solution of the tungsten and molybdenum must take place while both metals are in the solid state, or at least while the tungsten-rich portion is in the solid state. In case solution and diffusion have not been complete, two phases, one rich in tungsten, the other rich in molybdenum, will be present. In some of the briquets, the solution was not complete during the first heating, but was complete after the second heating. All evidences of

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the existence of two phases were removed when sufficient time was given the samples at temperatures near their melting points.

These samples were also examined after fusion in the parts which had been molten. There was no evidence of a second phase in any of these samples; that is, they were all true solid solutions.

Fig. 8 represents a micrograph of pure molybdenum. By analysis, it contains about 99.9 per cent. molybdenum. Its purity is also suggested by the micrograph.

These alloys are all readily etched by boiling hydrogen peroxide. Etching pits in the various alloys were produced by a comparatively long attack—say, 3 or 4 min. in boiling hydrogen peroxide. All of the evidence from the etching pits points to the conclusion that the crystal units are cubes. The alloys of tungsten and molybdenum, when made up according to the above description, seem to form etching pits very much more readily than either of the pure metals. This might be attributed to the existence of small particles of either the tungsten-rich or the molybdenum-rich portions, which, by the difference in composition, would facilitate solution at that point, thus easily forming the etching pits.

It is of interest in this connection to note that the average grain as seen in the micrographs represents about 1,000 of the original particles of the powdered metals. This free grain growth is positive evidence of free solution of the metals in each other. Had the metals remained as separate phases, each would have impeded the coalescence of the grains of the other.

Probable Accuracy of Results

The quantity of heat represented by the fusion wattage is independent of the quantity of heat actually necessary to raise a mass of metal, similar to that used in these samples, up to its melting point. It depends on the ability of the sample under investigation to dissipate heat in the apparatus. The thermal balance may be expressed by the equation:

Heat added electrically =
heat dissipated by radiation, convection and conduction.

The conditions obtaining within the apparatus are such that if the fusion of tungsten is represented by 1,800 watts, 1,750 watts could be dissipated for a long period of time without fusion of the tungsten.

The probable errors in the determinations are outlined below:

Errors due to:

1. Differences in emissivity of the various alloys under investigation.
2. Differences in their specific heats, heat capacities and heat conductivities.
3. The selective volatilization of molybdenum.

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4. Variations in the flow of water through the water jackets, of the temperature of the water at the intake, and of the flow of hydrogen through the inclosed chamber.

5. Slight variations in the diameter of the wires.

6. Possible changes of melting points due to chemical or physical combination of alloys with hydrogen.

7. The personal equation in drawing the per cent. of tungsten fusion wattage-temperature curve.

8. The readings of the electrical instruments.

The differences in emissivity of the various alloys will make but a slight difference in the fusion wattage, for the reason that only about one-twentieth of the total heat dissipated is lost by radiation, and the electrodes combined.

The errors due to No. 2 will be very slight, for the same reason.

Langmuir and Mackay[5] report that at 2,800° K., the heat loss from a tungsten filament in hydrogen at 750 mm. pressure, is 10½ times greater than that due to radiation; and at 3,500° K., the heat loss due to hydrogen is 11½ times that due to radiation. The conditions obtaining in the apparatus used by the writer were such that the losses due to the hydrogen are considerably more than is reported by these authors.

Dr. Irving Langmuir[6] suggests errors due to the selective volatilization of molybdenum.[7] The apparent high melting points of the alloys containing 5 per cent. tungsten, 95 per cent. molybdenum, and 5 per cent. molybdenum, 95 per cent. tungsten, may be partially due to this selective volatilization. The enrichment of the alloys in tungsten, due to this cause, could not have been more than 3 per cent. in any case as was indicated by the diameter measurements of the wires before and after fusion.

The errors due to No. 4 are probably greater than those due to all other variations combined. Errors due to these causes, however, are not necessary if the proper precautions are taken. These precautions would consist of supplying the water for the water jackets from a constant-level and constant-temperature tank. The same conditions should be fulfilled with the hydrogen.

To find out the approximate magnitude of these errors, samples of the same wires were fused on different days, so that the valve-settings for jacket water and hydrogen had to be re-made and re-adjusted.

[5] I. Langmuir and G. M. J. Mackay: The Dissociation of Hydrogen into Atoms, Pt. I, *Journal of the American Chemical Society*, vol. 36, pp. 1708–1722 (1914).

[6] Private communication, March 29, 1916.

[7] I. Langmuir: The Vapor Pressure of Metallic Tungsten, *Physical Review*, Series II, vol. 2, No. 5, pp. 329–342 (November, 1913).

I. Langmuir and G. M. J. Mackay: The Vapor Pressure of the Metals Platinum and Molybdenum, *Physical Review*, Series II, vol. 4, No. 4, pp. 377–386 (October, 1914).

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Several determinations showed a maximum variation in the melting point of molybdenum, as interpolated from the curve in Fig. 2, of about 40° C., and a maximum variation in the melting point of tungsten of about 10° C. The results on tungsten seem very favorable when it is considered that some methods for determining its melting point may vary 150° C.

Allowing for other errors, encountered in the determinations, it is reasonably accurate to assume that the maximum errors toward the molybdenum side of the diagram, are about 50° C., and toward the tungsten side of the diagram, 30°C. Allowing for errors of this magnitude, the points in Fig. 3 would fit nicely between the solidus and liquidus curves.

Errors due to slight variations in the diameters of the wires will be very small, owing to the fact that the ability of small wires to dissipate heat in a gas depends largely upon the existence of a relatively thick film of gas surrounding the wire. The thickness of this gas film is dependent largely upon the pressure of the gas, and not upon the diameter of the wire. The fact that these errors were slight was confirmed by an experiment, and was also predicted by Dr. Langmuir[8] from his extensive work along this line.

The suggestion was made by Dr. G. K. Burgess[9] that hydrogen might, by reason of either chemical or physical combination with the alloys, change their melting points. The writer has no knowledge concerning this. It might be well to add, however, that the handling of this type of alloys at high temperatures is done almost entirely in hydrogen.

To ascertain the approximate errors due to the drawing of the curve in Fig. 2, it was plotted independently on two sheets of paper. The maximum difference between the two curves was about 10° C. at about 60 per cent. of the tungsten fusion wattage. To facilitate the interpolation of temperatures from fusion wattages, this curve was drawn on a large piece of cross-section paper, so that the reading errors would be reduced to a minimum.

The wattage, as determined by volts and amperes, was, of course, quite accurate, so errors from this source were negligible.

Concerning the probable accuracy of results obtained by this method, Dr. Irving Langmuir writes: "I think the method you are using for estimating the temperature of filaments should give reasonably accurate results." He points out, among other things, the desirability of making chemical analyses *after* heating, to determine the actual composition of the alloy at the time of fusion. Dr. G. K. Burgess writes: "This method seems to me susceptible of very considerable accuracy."

[8] I. Langmuir: The Dissociation of Hydrogen into Atoms, Pt. II., *Journal of the American Chemical Society*, vol. 37, pp. 417–458 (March, 1915).

[9] Private communication, April 6, 1916.

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The author is convinced that the method is susceptible of greater accuracy than was obtained; that, with careful attention given to the construction and operation of the apparatus, melting points can be determined within 10° C. The method promises to offer solutions for several equilibrium diagrams of the higher melting-point metals.

The Crystal System of Tungsten and Molybdenum

Fig. 9 shows etching pits in tungsten, magnified 775 diameters. As can readily be seen from the micrograph, these etching pits represent sections of cubes cut by a plane parallel to a face.

Fig. 10 shows triangular etching pits which are formed by a plane cutting three faces of a cube.

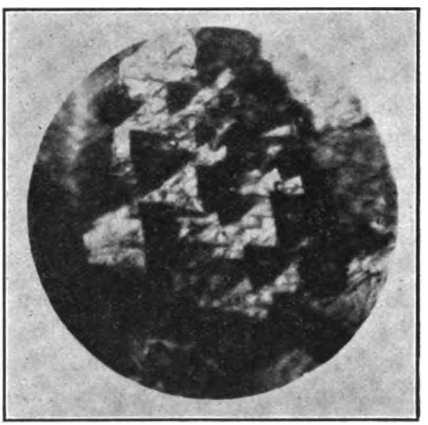

FIG. 10.—ETCHING PITS IN TUNGSTEN. × 775.

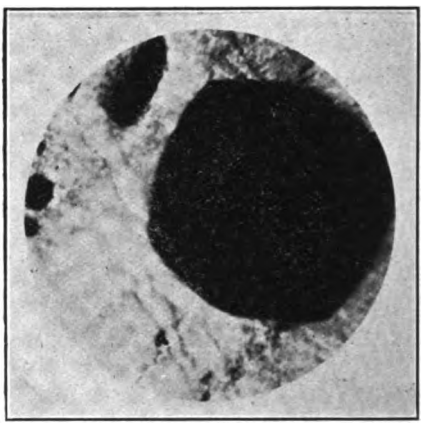

FIG. 11.—ETCHING PITS IN MOLYBDENUM. × 775.

Fig. 11, showing etching pits in molybdenum, magnified 775 diameters, represents the intersection of the six faces of a cube by a plane, By changing the direction of the illumination, the writer was able to examine the bottom of this etching pit and could easily see that it was a section of a cube.

It was noticed in some of the micrographs that the etching pits instead of having straight line sides had curved sides. Goldschmidt[10] thinks that these curved lines are due to the convection currents set up in the etching solution by the unequal rate of attack at various points. The solution will be used up most rapidly at places where the greatest amount of surface is exposed. This will cause a flow of fresh solvent by diffusion. The corners of the etching pits will offer greater resistance to the flow of convection currents than the adjacent portions, and hence will not

[10] V. Goldschmidt: From the Borderland between Crystallography and Chemistry, *Bulletin University of Wisconsin*, Science Series, vol. 3, No. 2, pp. 21–38 (March, 1908).

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receive so much fresh solution. The lines bounding the polygons are thus dissolved at unequal rates and become curved instead of straight.

A great many etching pits in all conceivable positions were examined microscopically. From these examinations, the author has very little hesitancy in saying that both tungsten and molybdenum crystallize in the isometric system and the form of their crystal units is the cube.

Discussion

A. G. Worthing, Nela Park, Cleveland, Ohio (communication to the Secretary*).—The paper by Mr. Jeffries is very interesting. He is to be commended for his pioneer work in the study of equilibrium systems at the very high temperatures which one obtains with tungsten and molybdenum.

There is one statement, however, to which I must take exception. On page 602, a statement is made which would seem to indicate that the Nela Research Laboratory joined Dr. Langmuir in the recommendation of 3,300°C. as the most probable melting point of tungsten. This is not the case. In the paper referred to by Mr. Jeffries, I have given 3,630°K. or 3,357°C. as the melting-point temperature for tungsten. Confirming observations by other workers in our laboratory justify the belief that this latter value is correct to within a small part of the discrepancy between the two values quoted.

Mr. Jeffries recognizes the possible introduction of errors in his calibration diagram in which he has plotted temperature as a function of the per cent. of tungsten fusion wattage. A different method of procedure which has merit would be the actual determining of points for the curve between the melting points of molybdenum and tungsten with the aid of a tungsten wire operated at intervening temperatures. It is perfectly possible by means of optical pyrometry, when once a temperature calibration has been established, to determine within a few degrees at what temperature such a wire is being operated. There are two or three such calibrations to select from. Of these the writer has considerable confidence in his own which is reported in abstract in the paper already referred to. Whichever one Mr. Jeffries might select, the procedure would seem to be better than that actually used. It would be interesting to note in this connection whether or not, with the use of such a calibration, the melting-point determination for molybdenum would consistently fall on the curve obtained with the tungsten wire.

In spite of our criticism and suggestion, we wish to express our high appreciation of this work.

Further discussion of this paper on p. 618.

* Received Sept. 14, 1916.

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The System Tungsten-Molybdenum

BY FRANK ALFRED FAHRENWALD, E. M., PH. D., CLEVELAND, OHIO

(New York Meeting, February, 1917)

INTRODUCTORY

A COMPLETE list of the elements as given for 1915 includes 18 that melt above 1,700° C. There does not exist one complete thermal equilibrium diagram for any pair of these 18 elements. Several of these elements have been combined, in relatively small proportions, with metals of lower melting point: *e.g.*, tungsten, molybdenum, and vanadium, in steels, or with nickel and cobalt; but of this refractory group, platinum is the only one that has been extensively studied in this connection.

When one considers that, with few exceptions, the industries have made use of alloys to the exclusion of pure metals, it is reasonable to expect that alloys of this refractory group will invade fields in which the pure metals have found little application. The past neglect of this class of metals has, no doubt, been partly due to the rarity of some of them, for even though alloys of very valuable properties should be developed, their cost, unless in exceptional applications, would be prohibitive.

There is also a well-founded reluctance to undertake research involving very high temperatures which, in the case of most of these elements, must be accompanied by an inert atmosphere as well. This combination of high temperatures* and control of atmosphere within the heating chamber of any furnace of present design is a difficult problem.

It is the purpose of this paper to give results,[1] of an investigation of one binary series of this group, in which methods were employed that avoided the usual difficulties accompanying high-temperature alloy investigation.

Historical.—The alloys of tungsten and molybdenum have received very little consideration during the past. A review of available literature reveals very little regarding this system, and that which has been recorded seems to be at fault. Mennicke[2] writes, "Die bekannteste Verbindung

* The classification of temperatures into "high" and "low" is only relative, but ranges above the maximum of the gas furnace are usually considered "high."

[1] A preliminary account of this series appears in *Trans.*, vol. 54, p. 541 (1916).

[2] Hans Mennicke: *Die Metallurgie des Wolframs*, p. 255.

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entspricht in ihrer Zusammensetzung der Formel W_2Mo_3. Es sind dies Kristalle von debeutender Härte und Festigkeit mit grossen Flächen. Die Farbe ist silberweissglänzend, die Dichte 14.8" * * * "(*a*) W_2Mo_3: Wird durch Reduction des Molybdänbioxides mit Wolframsilizid in Gegenwart von Kalks dargestellt nach

$$W_2Si_3 + 3MoO_2 + 3CaO = W_2Mo_3 + 3CaSiO_3$$

(*b*) WMo: Dies Komposition ist weniger bekannt. Sie ist von Stavenhagen und Schuchard[3] durch aluminothermische Reduction einer Mischung von Molybdän- und Wolframsäure nach

$$WO_3 + MoO_3 + 2Al_2 = WMo + 2Al_2O_3$$

bei Weissglut im Schamottetiegel dargestellt worden."

These two references are practically the sum of recorded experiments on tungsten-molybdenum alloys, as revealed by available literature. Referring to the original work of Stavenhagen and Schuchard, there seems to be small ground for the assumption of a definite compound as expressed by Mennicke. My own studies on this series have not revealed a compound of any formula.

Experimental.—A preliminary account of this series has already appeared,[4] but since that time more detailed work has been done in order to definitely locate the melting-point curve, and also to verify the equilibrium constituents for various percentage ranges. The materials, methods, and apparatus used in these experiments are those which have been described[5] in my earlier paper. The fusing-point curve shown in Fig. 1 was located by means of an optical pyrometer.[6] This was roughly checked by measuring the electric current necessary to fuse specimens of standard cross-section and of uniform length. The test specimens were prepared by compressing the mixed powders (reduced from the mixed and ground chemically pure oxides) into briquets, under standard pressure, which were then heated to near the melting point and afterward ground on an emery wheel to standard dimensions. These test-pieces were then mounted between tungsten electrodes in a chamber filled with hydrogen. The amperage and voltage in each case was accurately recorded, and the amperage was advanced in increasingly small steps until the circuit was broken by fusion of the specimen. From these figures the resistance of the specimen at the melting temperature was calculated.

The heat generated in a closed circuit by an electrical current is independent of the resistor and is directly proportional to the square of the current and the first power of the resistance; while the heat lost varies

[3] *Berichte Deutsche Chemische Gesellschaft*, vol. 35, p. 909 (1902).

[4, 5] *Trans*, vol. 54.

[6] The method of employing this means of temperature measurement is described in detail in the above reference, pp. 570 to 573.

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according to a higher power of the temperature. The method of determining temperatures from wattage-temperature curves is common. It is often employed in temperature control of resistance furnaces; it is used to determine temperatures of incandescent lamps; and forms the principle upon which certain optical pyrometers are constructed. It is also used in the preparation of certain high melting-point industrial alloys. As used in the present work, a standard wattage-temperature curve was first located by fusing specimens of platinum, iridium, molybdenum, and tungsten, respectively, under conditions identical with those to be employed for the unknowns. The heat losses by conduction to the electrodes and by convection with the reducing gas, are assumed to have been equal for standards and unknowns, throughout similar temperature ranges. Energy losses due to radiation are dependent upon the emissivity of the hot body, which is different for each substance, varying also with

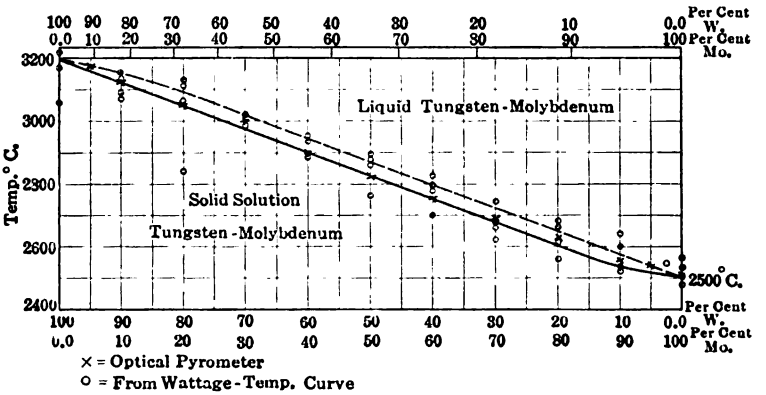

FIG. 1.—THERMAL EQUILIBRIUM DIAGRAM.

the physical nature of the radiating surface. It is thought, however, that the range between 2,500° C. and 3,200° C. is very closely located for the varying percentage alloys when each limit has been fixed by the corresponding end-member of the series.

Fig. 2 shows a curve drawn through points representing the melting temperatures of the standards as ordinates, and the current necessary to fuse each, as abscissæ. The dimensions of test specimens, the flow of inert gas, and other conditions, which might cause fluctuations, were kept as nearly constant as possible. The rather erratic results obtained by measuring the current required to fuse the alloys are due, not to a basic fault in the method, but to the improper conditions under which it was employed. In a proper application of this method of alloy investigation, the specimen should be of such dimensions as to furnish considerable resistance to the heating current and to have comparatively small carrying capacity, so as to simplify the accurate reading of volts and amperes.

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In the experiments here described briquets were used of 20.7 sq. mm. cross-section and with a uniform length of 2 cm. between electrode faces. The tungsten electrodes were, in turn, imbedded in heavy copper bars, which gave ideal conditions for large conductivity losses. Under these conditions a current of 525 amp. and of only 8 volts was required to fuse specimens of pure tungsten, thus bringing both of these factors into the range of difficult reading with the wide-range instruments available.

Table 1 gives average wattage data for a typical series of these alloys.

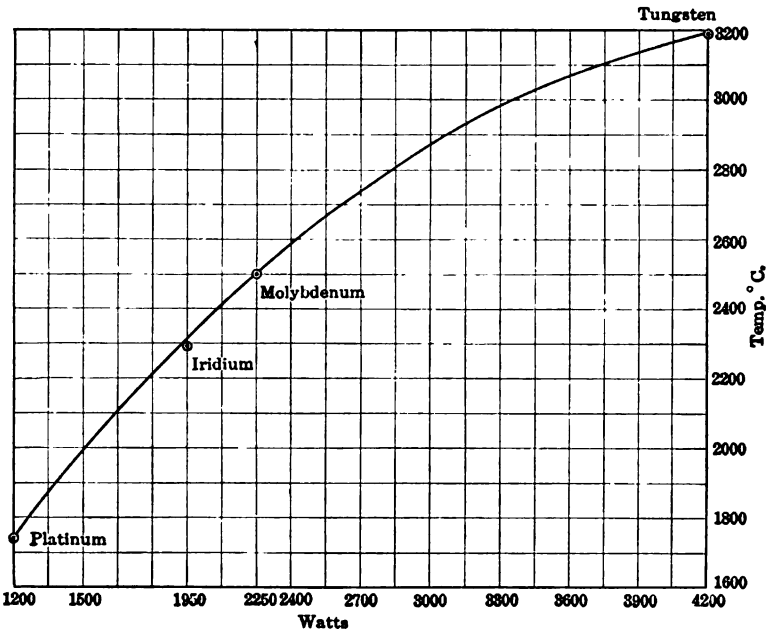

FIG. 2.—WATTAGE-TEMPERATURE CURVE DRAWN THROUGH STANDARD POINTS.

TABLE 1

W, Per Cent.	Mo, Per Cent.	Wattage: Average of Four Readings
100.0	0.0	4,200.0
90.0	10.0	3,840.0
80.0	20.0	3,510.0
70.0	30.0	3,330.0
60.0	40.0	3,150.0
50.0	50.0	3,030.0
40.0	60.0	2,790.0
30.0	70.0	2,550.0
20.0	80.0	2,430.0
10.0	90.0	2,400.0
0.0	100.0	2,250.0

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A brief description of this "electrical" method of investigating alloys is included with this report on the tungsten-molybdenum system despite the rather erratic results given by it (which are due to improper working conditions), because the entire high-temperature alloy field is thus brought within the range of comparatively easy investigation. The writer is at present employing it in the investigation of several alloy series of the refractory metals, and under such conditions that it reveals temperature fluctuations of a more delicate nature than will the optical pyrometer. The electrical resistance readings of any specimen furnish the points of a curve on which any sudden change due to the separation of a new constituent may be detected.

In the tungsten-molybdenum series no attempt was made to define the

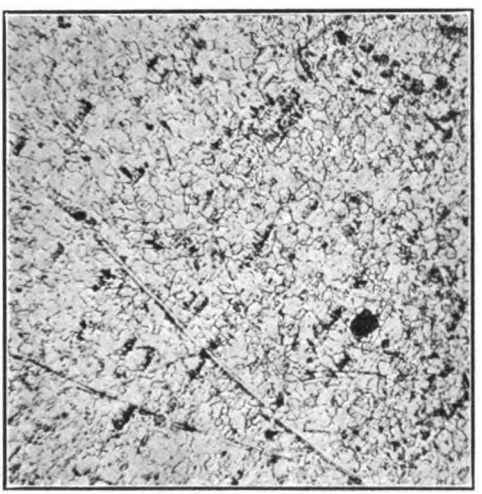

FIG. 3.—ALLOY 50 PER CENT. MO, 50 PER CENT. W, × 250. (Black spots are due to residual porosity of compressed briquet.)

liquidus curve; for as yet no method has been devised for this purpose. The rather narrow solid-liquid range indicated on the equilibrium diagram, Fig. 1, has been tentatively constructed from observation of the suddenness with which fusion took place, and of the lack of definable segregation, or "coring," within the crystal grains. It is quite probable that the fusion-point curve corresponds to the liquidus for a short distance near the tungsten end and to the solidus throughout the remainder of the curve; for near the upper end the amount of liquid (molybdenum) first formed may be slight, while near the lower limit the amount of liquid is large. However, this condition could exist only momentarily, if at all; for with even rapid heating equilibrium takes place very quickly. There might be sufficient lag, however, to cause the circuit to break at the solidus near the molybdenum end and at the liquidus near the tungsten end.

No critical point of any kind was observed within the series, which is

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evidence in favor of the complete isomorphism of tungsten and molybdenum. Breaks of a delicate nature could not have been detected under

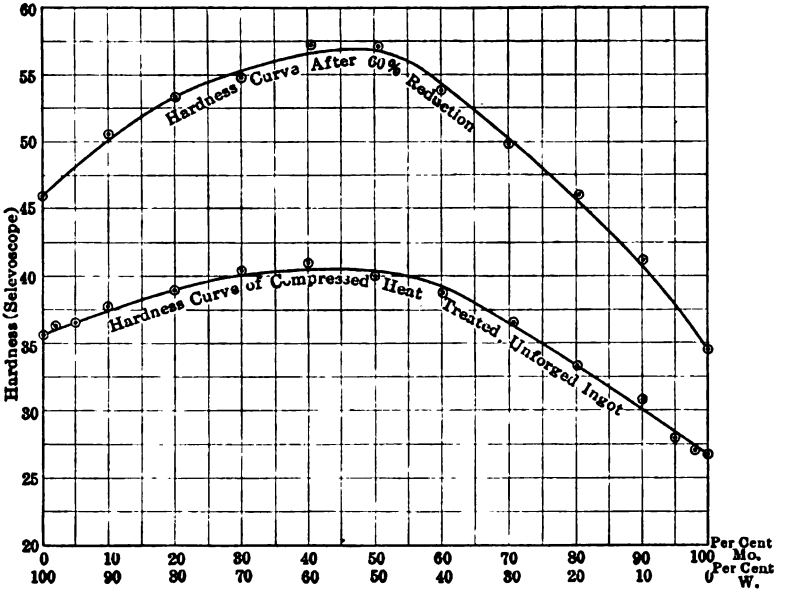

FIG. 4.—HARDNESS CURVES.

the described conditions, by noting any change in resistance during heating, but those caused by the formation of a separate constituent are not likely to have been overlooked. Microscopical examination of the alloys

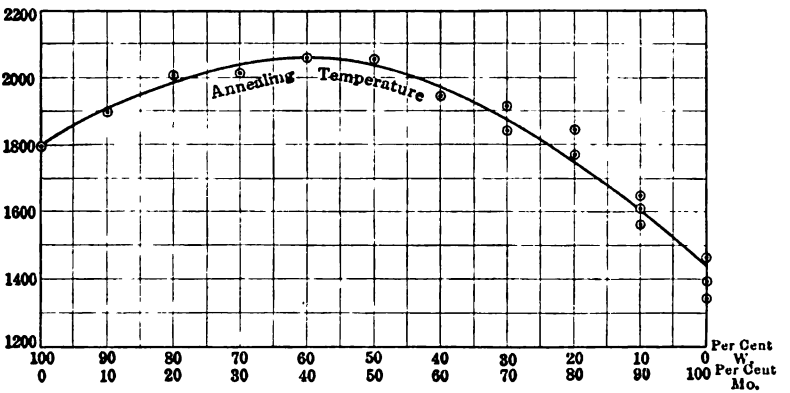

FIG. 5.—EQUIAXING TEMPERATURE CURVE.

gave final evidence of the complete isomorphism of these metals, for no new constituent was observed at any percentage composition. Fig. 3 shows a typical solid-solution structure and is representative of the entire series.

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Fig. 4 shows the hardness (Scleroscope) curves of this series. This cannot be taken as giving exact figures for either ingot or wrought tungsten, for in making these ingots a certain amount of porosity is unavoidable and is eliminated only after considerable working. The upper curve, however, should closely check values for the dense-wrought alloys.

Fig. 5 is a curve giving the temperatures at which specimens that had undergone about 60 per cent. reduction, would equiaxe in 10 sec. The position of this curve will vary, of course, with the different degrees of cold working, but it indicates an advantage of higher annealing temperature for the alloys over the pure components.

Summary

1. By compressing the mixed reduced powders of tungsten and molybdenum into briquets and then heating with an electric current in an atmosphere of hydrogen, alloys of this series were prepared varying in composition from 100 per cent. tungsten to 100 per cent. molybdenum.

2. The solidus curve for the series was located by means of optical pyrometer temperature measurements and checked by comparing the fusing current with a standardized wattage-temperature curve.

3. The equilibrium diagram for this series shows no critical points, appearing as resistance fluctuations, corresponding to a separation of a new phase. Its construction has been based upon this fact and upon results of microscopical analysis.

4. Curves for hardness, and for equiaxing temperatures, are smoothly convex, being typical of an uninterrupted series of solid solutions (mixed crystals).

5. As a result of thermal and microscopical analysis, the metals tungsten and molybdenum are reported to be completely isomorphous.

6. All alloys of this series are malleable and ductile under proper conditions.

Discussion

Discussion of the papers of Zay Jeffries (p. 600) and F. A. Fahrenwald (p. 612).

J. W. Richards, South Bethlehem, Pa.—I think that the diagrammatic results can be much more clearly set forth if they are plotted as follows: The present diagram, shown on page 602 of Mr. Jeffries' paper, plots the variation of wattage with temperature in a very inconvenient and uncertain way. If, however, you plot as abscissas the *logarithms* of the wattage, you get very nearly a straight line, so that you can interpolate the values with much greater accuracy than you can by using simply the wattage. In the accompanying diagram the abscissas run from 0 to 2, representing the logarithms of 1 to 100 per cent. of the wattage employed. The ordinates are the melting points. Line I represents Jeffries' results, which extrapolated from 2,500° C. seem to point to 3,430° C. as the melting point of tungsten. Line II gives Fahrenwald's results, which register perfectly for the melting points of

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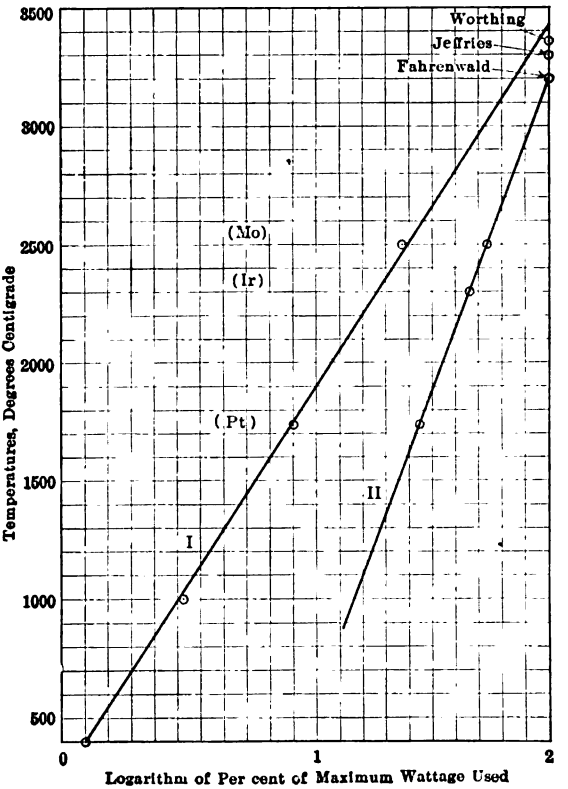
platinum, iridium and molybdenum, and also with Fahrenwald's assumed melting point of tungsten, 3,200° C. Worthing gives the latter temperature as 3,360° C.

ZAY JEFFRIES.—I would like to say that that is a very helpful suggestion.

THE CHAIRMAN (H. M. BOYLSTON, Cambridge, Mass.).—I notice that Prof. Jeffries draws a full line for the liquidus in the equilibrium diagram and dots the solidus, and that Mr. Fahrenwald does just the reverse. I wonder if there is any explanation of that.



ZAY JEFFRIES.—The points as actually observed are indicated by little circles, and I noticed, by microscopic examination, a little coring. By coring is meant the separation of the tungsten-rich material from the molybdenum-rich material. Coring indicates that there is a difference in temperature between the solidus and the liquidus; what that difference is, I have no means of knowing, so I suppose that I really should not have put in the indicated difference in temperature between the liquidus and solidus. If I am not mistaken, however, I mentioned the fact in the paper that the liquidus and solidus curves were only tentative.

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A Chemical Explanation of the Effect of Oxygen in Strengthening Cast Iron

Discussion of the paper of W. McA. Johnson, *Trans.* (1916), **53**, 451.

Henry M. Howe, Bedford Hills, N. Y. (communication to the Secretary.)*—Mr. Johnson's explanation, that the rounding of the graphite masses in oxygen-bearing cast iron is due to their being in part re-precipitated after re-solution in forming carbonic oxide temporarily with that oxygen, is certainly most attractive and suggestive. It remains to be shown whether this rounding occurs also when the solidification is too rapid to permit this process to go on. J. E. Johnson might easily test this by casting a single ladleful of his oxygenated iron in masses of varying size. If this present hypothesis is true, then the rounding ought to increase progressively with the size of the casting, and with the distance from the outside, that is to say with the time available for re-solution and re-precipitation.

I question whether a like benefit is to be expected even on this hypothesis, from the oxygenation of steel. The spheroidizing of graphite does good because graphite itself is so weak. But the constituent which can be spheroidized in steel is its cementite, which is a source of strength.

Lamellar pearlite, in which this cementite in effect forms long dendrites, is far stronger than divorced pearlite, in which these dendrites have been broken up into spheroids. Even the surfaces of contact between ferrite and cementite are probably sources of strength because of the amorphous iron which probably fills them.

* Received Nov. 15, 1916.

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The Function of Alumina in Slags

BY CARL HENRICH, LINCOLNTON, GA.

(New York Meeting, February, 1917)

I HAVE read with particular interest that portion of the discussion by Anton Eilers referring to the high-lime (and also high-alumina) slags made by August Raht in 1881, while smelting the Horn Silver ores at Franklin, Utah. The two analyses of such slags, furnished by Mr. Raht to Mr. Eilers are:

Horn Silver Slags

	Slag I	Oxygen(*b*) percentage	Slag II	Oxygen(*b*) percentage	Average of I and II	
						Oxygen
SiO_2	33.9	18.1	35.0	18.7	34.50	18.40
FeO	26.8	6.0	25.4	5.7	26.10	5.80
CaO	26.1	7.5	24.9	7.1	25.50	7.30
BaO	3.2(*a*)	0.3	3.2	0.3	3.20	0.30
Al_2O_3	10.8	5.1	9.3	4.4	10.05	4.75
	100.8	37.0	97.8	36.2		

(*a*) Not determined in this analysis.
(*b*) Not given in Mr. Eilers' paper.

Oxygen ratio, Al as base.

I. O in acid: O in base::18.1:18.9::0.96:1
II. O in acid: O in base::18.7:17.5::1.06:1
Average: 18.4:18.15 = 1.01:1

Oxygen ratio, Al as acid.

I. O in acid: O in base::23.2:13.8::1.68:1
II. O in acid: O in base::23.1:13.5::1.76:1
Average: 23.15:13.4 = 1.73:1

By assigning to alumina the rôle of a base, Mr. Eilers deduces the formula $3FeO.2SiO_2 + 4CaO.2SiO_2$.

The nearest approach to a simple formula expressing the average composition of these slags, which I can figure, is

$$9RO.Al_2O_3.6SiO_2.$$

Allowing a ratio of 4FeO : 5CaO, we have for this formula;

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SiO_2 = 35.0 per cent.; Al_2O_3 = 10.0 per cent.; CaO = 27.0 per cent.; FeO = 28.0 per cent.; in round numbers. This would be a singulo-silicate of CaO and FeO with about 34 per cent. or one-third of the RO bases replaced by Al_2O_3.

However, it has always appeared to me to be an absurdity to force a sesquioxide to replace FeO or CaO or any other RO, simply to make the analysis of some slag or mineral fit into some orthodox formula. That Al_2O_3 can be substituted for Fe_2O_3 or Cr_2O_3, just as MgO, CaO, BaO, FeO, and MnO can be substituted for each other in various chemical combinations, is well established. But I do not know of any natural silicate in which it has been shown conclusively that Al_2O_3 would take the place of RO, and I do not believe that alumina will do it in any artificially produced slag silicate.

If in the above slags we consider alumina as an acid, *i.e.*, the slags as mixtures of silicates and aluminates, we find that they satisfy, or closely approach the formula:

$$(9RO.5Al_2O_3) + 6(6RO.5SiO_2).$$

In short, assuming alumina as an acid, $2Al_2O_3$ would be the equivalent of $3SiO_2$, or 102.8 mass-units of alumina would be the equivalent of 90 mass-units of silica, and the oxygen ratio of the correct slag of the above type would be 5 : 3. The slag would be essentially a lime-iron 5 : 3 "silic-aluminate" (slightly acid "sesqui-silicaluminate").

Slag No. I corresponds very nearly to this degree with an oxygen ratio of 23.2 : 13.8. Slag No. II does not correspond so well, and it is also metallurgically slightly inferior to No. I as shown by the lead and silver contents. On the supposition of Al as basic, the increase in the silica of this slag should make it better metallurgically than No. I, which, however, is evidently not the case.

I am aware that many metallurgists are unwilling to assign to alumina the rôle of an acid or to consider it as a substitute for silica in the calculation of their slags. Except in iron blast-furnace practice, it is of no great importance whether we consider it as a base or an acid or do not consider it at all. As the insoluble residue of ores or limestone is frequently calculated as silica, it is automatically assigned to the rôle of an acid. But practical metallurgists have, as a rule, avoided the issue by following Kerl's advice and have kept the alumina of their slags as low as possible, so low, in fact, that usually it has been immaterial which way the alumina has been regarded. The metallurgist in a modern centrally situated blast-furnace plant can do this, but occasionally the alumina problem becomes a serious one to the metallurgist at an isolated plant, smelting, perhaps, the ore from only one mine, so that the question has its interest from a practical as well as from a scientific side.

In any slag the chief points of interest are:

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The formation and melting points, since these determine coke consumption and furnace temperature; its viscosity, which must be small, both to allow it to flow readily from the furnace and to permit a quick mechanical separation of it from the matte, speiss or metal; the chemical composition, since it must not dissolve large quantities of the valuable constituents of the ore, and must (as in iron smelting) occasionally remove detrimental ones.

In general, an increase of alumina or silica will raise the formation temperature of a slag, and ordinarily it will be made thereby more viscid at or near its formation temperature; but these qualities are likewise affected by the various bases entering the slag and their relative proportions.

As practical experience has ordinarily shown that scaffolds over the tuyères, and hard slag and metal taps resulted from high alumina, among the bases in unisilicate and sesquisilicate slags, alumina has acquired a bad reputation among lead and copper smelters.

I might suggest at this point that it is a well-known fact that suddenly cooled slags ("chilled") are more quickly attacked by hydrochloric acid than if slowly cooled. This suggests that the crystallized and crystalline minerals occurring in slowly cooled slags are not contained as such in the molten magma, but are only formed from the original magma during the cooling process.

Among the cases in which high alumina in the smelting charge became a practical problem were the Horn Silver ores smelted by Mr. Raht in 1881, already mentioned, the oxidized ores of the early days of the Copper Queen at Bisbee and the Detroit Copper Mining Co. at Morenci and my own experiences in smelting some ores of the Champion and United Copper Mines near Nelson, N. Z.

In those times, I suppose, we all considered alumina as a base when we considered it at all. I know that I did when confronted with an unusual amount of kaolinized porphyry in some of the Detroit Copper Mining Co.'s ores. I know, too, that the behavior of the slags calculated in this way was often a sore puzzle to me. For instance, I give below two slags made at Morenci about 1884. No. I was a good slag, keeping the furnace open and running freely, the other was a bad slag, inclined to form noses and scaffolds above the tuyères, thick, sticky and sure to bring the smelting campaign to an abrupt end if not promptly changed.

The oxygen ratio, considering alumina as a base, does not throw any light on the radically different nature and behavior of these slags. The only thing which at that time seemed feasible to me, was to follow Kerl's rules as far as possible, and keep the alumina low.

But when I went from Morenci to Nelson, N. Z., and there faced, unexpectedly, the problem of smelting the copper ores of one mine, with no available flux but magnesian limestone, I found myself "up against

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Detroit Copper Mining Co.'s Slags

	Slag I	Oxygen percentage	Slag II	Oxygen percentage
SiO_2	34.3	18.3	38.1	20.3
Al_2O_3	11.8	5.5	16.9	7.9
FeO	38.5	8.6	33.1	7.4
CaO	10.1	2.9	8.0	2.3
MgO	2.3	0.9	2.2	0.9
	97.0		98.3	

Oxygen ratio,

Al_2O_3 base:	18.3 : 17.9	20.3 : 18.5
Al_2O_3 acid:	23.8 : 12.4	28.2 : 10.6

it." The ore was mainly chalcopyrite, with some pyrite, in a gangue locally known as serpentine. The average composition of the country serpentine on both sides of the lode serpentine appeared to have the approximate formula $3MgO.Al_2O_3.3SiO_2$ or about 30 per cent. MgO, 25 per cent. Al_2O_3 and 45 per cent. SiO_2.

The lode serpentine, forming the gangue of the ore, approached more nearly the composition of $4MgO + 4Al_2O_3 + 5SiO_2$, or about 17.5 per cent. MgO, 43.5 per cent. Al_2O_3 and 39 per cent. SiO_2. One actual analysis of the gangue of roasted ore figures 36 per cent. SiO_2, 46 per cent. Al_2O_3 and 18 per cent. MgO_2. That silica-alumina ratio in the available ore of the smelting charge was a decided and puzzling novelty. Kerl's advice was evidently not applicable in this case. As I published, in 1886 or 1887, in the *Engineering and Mining Journal*, an account of my experiences in smelting that ore, it would be superfluous to repeat that account here. The following slag analyses and appended remarks will suffice:

	Slag 1	Oxygen percentage	2	O	3	O	4	O	5	O
SiO_2	36.4	19.4	34.6	18.4	39.1	21.3	21.8	11.6	18.4	9.8
Al_2O_3	27.8	13.1	26.4	12.4	22.9	10.8	31.8	14.7	20.8	9.8
FeO	14.2	3.1	32.2	7.2	28.6	6.4	30.6	6.8	35.9	7.9
CaO	1.5	0.4	....	....	....	....	....	.....	10.0	2.9
MgO	18.4	7.4	6.8	2.7	8.4	3.3	15.5	6.2	14.3	5.7

Oxygen ratio,

Al basic:	19.4:20.4;	18.4:22.3;	21.3:20.5;	11.6:27.7;	9.8:26.3;
	0.95	0.83	1.05	0.42	0.37
Al acid:	32.5:10.9;	30.6:9.9;	92.1:9.7;	26.8:13.0;	19.6:16.5;
	2.98	3.09	3.32	2.02	1.19

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	6	O	7	O	8	O	9	O
SiO_2	19.7	10.5	22.6	12.0	24.2	12.9	23.9	12.7
Al_2O_3	18.5	8.7	22.5	10.6	26.2	12.3	26.8	12.6
FeO	28.5	6.3	32.7	7.3	32.1	7.1	33.5	7.4
CaO	15.0	4.3	5.1	1.4	8.0	2.3	6.4	1.8
MgO	18.3	7.3	18.4	7.4	9.2	3.7	9.4	3.8

Oxygen ratio,

Al basic: 10.5:26.6; 12.0:27.8; 12.9:25.4; 12.7:26.6;
0.40 0.43 0.51 0.50

Al acid: 19.2:17.9; 22.6:15.2; 25.2:13.1; 25.3:13.0;
1.07 1.49 1.92 1.95

1. Slag of first trial run—very short run. Scaffolded and froze up. Furnace: 36-in. round water-jacketed (1884) Arizona copper furnace.

2. Slag from same run, when furnace began to scaffold.

3. Same run. From chilled scaffold above tuyères.

4. From second equally short run. Somewhat better roasted ore, more coke and somewhat higher blast pressure. Thick, gluey slag from slag-tap. Scaffolding.

5. From third, successful run, this time considering alumina as an acid, and counting 35 weight-units of alumina the equivalent of 30 units of silica. Blast pressure: 9 to 10 oz. First slag fall, changed to:

6. By addition of more limestone flux to the charge. This is a very free-running light slag, separating easily and clean from the 45 per cent. copper-matte. Thirty-six-inch furnace running at the rate of 45 tons smelting charge in 24 hr.

7. Slag from last 12 hr. of run, finishing roasted ore on hand, and with shortage of limestone flux, which could not be replenished in time. Furnace not running as free as on No. 6 slag, only at the rate of 27 tons in 24 hr. As available limestone flux diminished, Slag Nos. 8 and 9 ensued, which allowed to finish the campaign with increased fuel and

	Slag	Oxygen percentage
SiO_2	26.6	14.2
Al_2O_3	15.4	7.2
FeO	42.6	9.5
CaO	9.5	2.7
MgO	0.2	0.1

Al_2O_3 as a base; Oxygen ratio: 14.2:19.5.
0.73

Al_2O_3 as an acid; Oxygen ratio: 21.4:12.3.
1.74

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blast, using up all roasted ore on hand. If continued on this slag, however, the furnace would surely have scaffolded and frozen up.

An instructive slag, high in alumina, is the preceding typical Copper Queen slag of about 1884.

Considering the composition and behavior of all the slags given above it seems to me evident:

1. In slags containing larger quantities of alumina, the alumina should be considered as an acid, replacing silica, and not as a base.

2. The higher the percentage of alumina, the nearer the slag should approach a "singulo-silicaluminate," *i.e.*, the nearer the oxygen ratio of the bases should come to the combined oxygen of the silica and alumina.

3. An increase in magnesia calls for a higher percentage of bases, while absences of magnesia, and a pure limestone as a flux, will permit an approach to a "bi-silicaluminate" slag.

4. The safe way will be to start with a "sesqui-silicaluminate" as the type of slag to be produced:

$$x(4RO.3SiO_2) + y(2RO.Al_2O_3),$$

which is approximately the slag actually, successfully and involuntarily, I suppose, made in 1881 by August Raht, while smelting very aluminous and very limey ores from the Horn Silver Mine.

There has always been a bias among metallurgists in favor of assigning to alumina the rôle of base in the composition of the slags. This has been natural, as the textbooks on mineralogy assign to alumina in the composition of the silicate minerals the rôle of a base, with very few exceptions. Hence, not questioning the correctness of this authoritatively assigned basic function of alumina, expressed as such in all the orthodox formulas of these minerals, it seemed only proper and correct to consider Al_2O_3 as a base in the artificial silicates, the slags. When this view played the mischief with the running of the furnace, this was ascribed to the innate depravity of alumina. You could support a little of it, but you must not allow too much of it, or it would get the better of you. That was about the practical conception in lead and copper smelting, using low furnaces, cold blast, low pressure, and under the necessity of keeping the fuel cost down.

At least, this accurately describes my own view of the case in 1884, when confronted with kaolinized porphyry of the Detroit Copper Mining Co. But having had forced upon me the conviction of the acid character of Al_2O_3 in the artificial slag silicates, or at least in some of them, it has engendered some doubt whether alumina always has that function of a base in the natural silicate minerals assigned to it in the orthodox formula of silicates, a question with which I purpose dealing at some future time.

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Discussion

A. S. Dwight, New York, N. Y.—Mr. Henrich's paper is rather inconclusive as to the role that alumina really plays in slags. He gives a number of interesting instances of high alumina, with conclusions that he draws from the analyses, in the light of what he knows of the accompanying conditions, but the information given is hardly sufficient to permit intelligent criticism of these conclusions, for it is well known that the slag composition is not the only factor that can produce some of the furnace results favorable or disastrous, as the case may be, which he records. As a matter of fact, most metallurgists who have wrestled with this problem in connection with the smelting of lead and copper ores, usually arrived at some sort of a working hypothesis which met the local conditions fairly well, and then, being busy men, were inclined to leave the theoretical question very much as Mr. Henrich leaves it when he says: "A question with which I purpose dealing at some future time."

It is unfortunate that in the Golden Age of the smelting industry in the West, when we were all dealing with large tonnages of complex ores, often containing large percentages of alumina in combination with other refractory elements, the smelting companies were not generally equipped for research as they are today, and much experience that might have been most instructive, and perhaps have led to a solution of the problem, was lost to us.

A problem of this kind must be attacked from the empirical side, and on a full working scale. That is to say, we must find a slag which works well in the furnace, flows hot and clear, melts rapidly, reduces well, and keeps the furnace open and free. Having once produced the slag that fulfills these ideal conditions, we should try and find out why it is good, and in this we can gain much help from laboratory appliances and theoretical reasoning, determining the analysis, specific gravity, melting point, specific heat, viscosity, oxygen ratios, etc. Through a knowledge of these factors we may learn how to reach the same result another time. But I maintain that if we start our reasoning with oxygen ratios, formation or melting temperatures, or even slag analyses disassociated from experience, we can never reach safe conclusions, to be applied to the solution of the practical problem.

Most metallurgists have, as Mr. Henrich says, tried figuring alumina on both the acid and basic side of the equation, and that has also been my own experience. While always decidedly doubtful of the possibility of aluminates being formed at the temperatures prevailing in the lead-smelting furnace, I nevertheless obtained better results with my slags when I figured Al_2O_3 as an acid, *i.e.*, replacing SiO_2, thus corroborating to some extent Mr. Henrich's experience, than when I treated it as a base; but I finally adopted with most satisfactory results the theory

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proposed by the late T. S. Austin, then Superintendent of the El Paso Smelting Works, and with whom I enjoyed a close business association and professional acquaintance. Austin's theory was that in lead slags Al_2O_3 played a neutral role and might be considered as remaining dissolved as Al_2O_3 uncombined in the slag. I am familiar with the experiences which led him to adopt this working theory, which were briefly as follows: The El Paso plant was called on to treat large tonnages of ore from the Parral district in Mexico, containing considerable percentages of fluorspar (calcium fluoride). The slags were figured as usual, assuming that the lime of the fluorspar went into the slag as CaO, but the slag always came down off type, and showing all the physical and metallurgical characteristics of a slag deficient in lime, though by analysis it was apparently correct. As previously stated, the actual behavior of the slag itself is the only true guide to the experienced metallurgist, and in this case the addition of lime to the charge would invariably bring it around to its proper characteristics, and behavior. The amount of correction needed was equivalent to the lime present as fluorspar. Finally, by neglecting the lime present as fluorspar, Austin got his correct slag on first calculation, so he reasoned that fluorspar was not decomposed in the smelting but simply dissolved in the mass of slag as such. Some time after that he happened to encounter an ore high in Al_2O_3, and obtained excellent results by applying the same line of reasoning, *i.e.*, considering it as an inert diluent. Subsequently ZnO was found to behave very much the same way, at least satisfactory results were obtained with very zinky ores when this working theory was followed. My own experience, extending now over a good many years, has corroborated the practical value of Austin's theory as applied to these three elements. But like Mr. Henrich, "I propose to deal with this at some future time," unless some one is more forehanded and gives us the true answer to the riddle.

E. P. MATHEWSON, Toronto, Ont.—I want to add a few words to what my friend, Mr. Dwight, has said, and to confirm his findings. It is some time since I had to do with very heavy alumina in slags, but when I did have to do with it, I had a great deal of trouble at the start, due to the fact that we were dyed-in-the-wool on certain types of slags and had to follow those types. A certain idea struck us some years ago in Colorado when alumina was forced on us from Cripple Creek, that we would not figure alumina as a base, would not figure alumina as an acid, and see what happened. The effect was magical. The slags, calculated as if no alumina existed in them, ran freely, and the furnaces went faster than they ever did on the old charges when alumina was not present in any appreciable quantity. We followed this up when we were dismantling furnaces and had enormous quantities of brick bats to dispose of. We got

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rid of these without any trouble and had the fastest running charges imaginable by disregarding the alumina entirely and calculating the charges as if no alumina existed. The same thing happened in copper smelting later, in Montana, although we had not quite so high alumina; by disregarding the alumina and treating it as an inert body, we had magnificent results.

WM. B. BOGGS, Douglaston, N. Y.—I notice in all these slags that the sum of the lime and magnesia runs up anywhere from 2 to 15 or 18 per cent. I have been forced to handle a lot of basic and siliceous ores from South America and Canada, carrying large amounts of aluminum. We make a very peculiar slag in our furnaces; that is, it is not normal, it runs from 32 to 34 per cent. silica, no lime at all, 8 to 10 per cent. alumina; and the balance is iron oxide, a very small amount of zinc oxide and magnesia oxide. We received a shipment of ore recently that ran 16 to 20 per cent. alumina, 8 to 10 per cent. silica, 4 to 5 per cent. copper and was high in iron and sulphur. Because of the shortage of sulphur we were forced to use a large amount of this ore, even to keep our grade of matte down to 55 per cent., and we found that it was all right to neglect the calculation of alumina in the slag composition, as long as the alumina was below 10 per cent. When the alumina in the slag went over 10 per cent., the furnace slowed down and dropped to about 2 tons per square foot of hearth area. By constant analysis we found that our silica had become 26 per cent. in the slag. When we substituted for this sulphide, a clean sulphide that did not contain alumina, the furnaces picked up, that is, we could not force the furnace to run with a slag over 10 per cent. alumina. If we were running lime, it would act differently; if we substituted lime for some of the basic ores high in alumina the furnace made a slag higher in silica and this diluted the slag so that the alumina came down; with our blast furnace making a matte of about 55 per cent. copper, we found that we could not get a tonnage of over 3½ to 4 tons per square foot of hearth with a slag containing more than 10 per cent. alumina.

JOSEPH W. RICHARDS, So. Bethlehem, Pa.—The title of this paper is too broad; it should be "The Function of Alumina in Lime-Iron Slags." The remarks of the author do not apply to the alumina in many other slags in which it is a large constituent. In iron blast-furnace slags, we have to take alumina into account; it is too large a constituent to neglect. In the siliceous slags of charcoal iron furnaces, the alumina undoubtedly acts like a base; but in the ordinary basic slag of coke iron furnaces, the alumina is best taken care of and understood if it is considered as an acid. This is the position of alumina in iron blast-furnace slag, and the qualifications which the author makes, and his remarks concerning alumina, do not apply to alumina in this important class of slags.

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A. S. DWIGHT.—Following Prof. Richards' remarks, I will add to what I have already said that it is my belief, based on long observation, but without special investigation by pyrometric tests or otherwise, that at the maximum temperatures produced in the blast furnace smelting lead ores, neither aluminates nor silicate of zinc are formed. On the other hand, the temperatures in the iron blast furnace are much higher, and we must not fail to recognize the probability that entirely different combinations of the elements may take place. The lead and copper slags must be studied for themselves.

W. C. SMITH, Chrome, N. J.—Some few years ago I had charge of a small lead refinery, treating refinery byproducts. The principal material we had to get rid of was a mixture of magnesite brick, clay brick and silica brick not properly bedded. The furnace had been running along all right with about 10 per cent. of alumina in the slag. The night man called me up about 3 o'clock one morning, and said that the furnace was slagging slowly, and that the air did not go through properly, slag cold. By morning the furnace had picked up and was getting through more tonnage than we had gotten through in a number of weeks. We thought the troubles were over. The regular morning slag was sent to the laboratory, when the laboratory reported 26 per cent. of alumina Thinking the chemist had made a mistake, I sent it to him again, and the analysis came back with 27 per cent. alumina. The furnace continued to make this slag for 3 days, we had evidently cleaned up the material high in Al_2O_3, and in going back to our old slag the furnace froze. I never knew why the immediate range from the high-alumina slag going down to a normal of about 10 per cent. would freeze the furnace. The SiO_2, FeO (CaO and MgO) ratio was about the same in our slags with 10 per cent. Al_2O_3 and with 26 per cent. Al_2O_3. If Al_2O_3 is inert and simply dissolved in the slags, why did the slags carrying between 10 and 26 per cent. Al_2O_3 give trouble?

J. W. RICHARDS.—It may not be generally known that some iron blast furnaces have been run regularly for a considerable time with 30 to 35 per cent. alumina in the slag, properly running slag which also excluded sulphur quite satisfactorily from the pig iron.

CARL HENRICH (communication to the Secretary*).—I have read with much interest the discussions of my paper by Messrs. A. S. Dwight, E. P. Mathewson, Wm. B. Boggs, Jos. W. Richards, and W. C. Smith.

Mr. Dwight's acknowledgment that he always—in lead smelting—obtained better results by figuring Al_2O_3 as an acid in his slags, I may take as a confirmation of my views, notwithstanding his later conversion by the late T. S. Austin to the latter's empirical view and way of neglect-

* Received Apr. 20, 1917.

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ing the consideration of Al_2O_3 altogether in the composition of slags—letting the slags "dissolve" in Al_2O_3. I have a strong suspicion that Mr. Austin, in a large custom smelting plant at El Paso, has been able to keep the alumina percentage of his slags sufficiently low, not to give him much trouble, although I have also a suspicion that his furnace foreman may have had a somewhat differing opinion of the practical working of the "dissolving" theory, whenever its applicability was tested to its limits. That Austin should have conceived the notion that the Ca of CaF_2 would enter into a slag as CaO, seems hardly in accord with the recognized ability of that late metallurgist. It is a well-recognized fact that even small amounts of CaF_2 will reduce the viscosity of a slag; but I did not think anyone ever had an idea that CaF_2, in some mysterious way, parted with its F and coupled itself with O to satisfy a craving for CaO on the part of SiO_2—least of all in a lead furnace. The "dissolving" theory for CaF_2 was doubtless appropriate, and an improvement on figuring the Ca of CaF_2 as CaO to satisfy the demand of SiO_2 for RO bases. However, I cannot understand the rationality of extending this "dissolving" theory to Al_2O_3 and to ZnO.

It would be interesting to have more specific data from Mr. Mathewson in regard to the actual composition—analyses—of the slags made, when he had these unusually high alumina slags in Colorado. We "old" smelters all know that within certain limits, usually found out by rather tough experience, we could safely "disregard" Al_2O_3. In fact, I believe, we never really analyzed for alumina, except in slag analyses, when we got it as an incidental to the Fe determination. Specific analyses, furnished by Mr. Mathewson, of successful highly aluminous slags, and also of such which were not so successful, might throw more light on the question of the safe limit to which the "dissolving" theory for Al_2O_3 in slags may be pushed—and also on what the constitution of such a slag would be, with or without considering Al_2O_3 as an essential constituent of its chemical composition.

Mr. Boggs appears to have found that safe limit of the "dissolution" of Al_2O_3 to be near 10 per cent. alumina. I might suggest to Mr. Boggs, that the "not normal" slag made by him (about 33 per cent. SiO_2; 9 per cent. Al_2O_3; 58 per cent. FeO) ceases to be abnormal if you consider it as $(Fe, Mg, Zn)O.Al_2O_3 + 2(4RO.3SiO_2)$ — 32.5 per cent. SiO_2; 9.2 per cent. Al_2O_3; 58.3 per cent. FeO; *i.e.*, a normal sesquisilicate slag dissolving—or combining with according to my opinion—about 15 to 16 per cent. of a normal magnesia-zinc-iron spinel. Of course, the presence of magnesia and zinc would further predispose the spinel formation, but is not essential thereto.

I agree with Mr. Richards that my paper would better have been entitled: "The Function of Alumina in Iron-Lime Slags." I may plead in extenuation, that the title was not of my selection; also that the paper

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was originally written merely as a discussion of Mr. Anton Eilers' discussion of another paper on high-lime slags; and that on invitation of some officers of the Institute I prepared a more extended paper on the function of alumina in slags, for which, however, also on request, I substituted my original discussion of Mr. Anton Eilers' discussion. In that unpublished paper I had dealt also with the function of alumina in slags of iron furnaces. I had come to the conclusion, that while Al_2O_3 in these slags, as a rule, plays the role of an acid, a scarcity of RO bases (CaO mostly) and a preponderance of Al_2O_3 may force Al_2O_3 into the role of a base, in part.

Both Mr. Richards' and Mr. Smith's experiences with high alumina slags in iron furnaces are, of course, incompatible with assigning to Al_2O_3 in a slag an "inert" character, *i.e.*, with the "dissolution" theory of Messrs. Dwight and Mathewson. It would be interesting, to me at least, to get the analyses of these high alumina slags, both the 10 per cent. and 26 per cent. alumina slags mentioned by Mr. Smith, and the regular 30 to 35 per cent. alumina slag spoken of by Mr. Richards. I believe they would confirm my views on the function of alumina in slags.

Additional discussion on p. 941.

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The Viscosity of Blast-Furnace Slag*

BY ALEXANDER L. FEILD,† A.B., M.S., PITTSBURGH, PA.

(New York Meeting, February, 1917)

INTRODUCTION

THE Bureau of Mines is investigating the problem of slag viscosity, its variation with the temperature and with the composition of the slag, and its effect upon the distribution of the sulphur between molten iron and slag, as introductory to a series of contributions to the theory of the metallurgical processes. The method of measurement, viscosity apparatus, furnace and other accessories mentioned in this paper have been described fully in a Bureau of Mines report.[1]

THE LITERATURE ON SLAG VISCOSITY

The viscosity of blast-furnace slag had not been measured previous to the investigation described in this report. A number of investigators have determined the "fusibility," or softening temperature, of slags by means of the cone method or other deformation method, but these methods supply only limited information regarding the temperature-viscosity relations of the slag in question. Any deformation method that may be employed indicates only the temperature at which the slag attains a more or less definite viscosity, the magnitude of the value obtained depending on the method used. For details of the various methods used by different investigators, see the Bureau of Mines paper already referred to. An isolated measurement by Doelter on a single mixture at 1,300° C. represents the highest temperature reached in the various viscosity investigations previous to those described in this report, in which results have been referred to absolute units. Arbitrary deformation tests have of course been made at much higher temperatures.

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†Assistant Metallurgist, U. S. Bureau of Mines.

[1] A. L. Feild: A Method for Measuring the Viscosity of Blast-Furnace Slag at High Temperatures, *Bureau of Mines Technical Paper* 157, 1916, 29 pp.

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Theory of the Method Employed

In these investigations the author has used a modification of the method originated by Margules[2] in 1881. In this method the liquid is confined between two concentric cylinders. The outer cylinder is rotated at a constant speed and the torque exerted upon the inner cylinder is measured. The method is applicable to liquids of a wide range of viscosity, and has been used, with modifications, by Gurney[3] in the accurate measurement of the viscosity of water. The principle of the method is used in the Stormer and Clark viscosimeters, and in those of Carmichael and Grosvenor, and of Kottmann.

The principle of the method employed by the author is therefore not a new one. The torsion method, however, had not hitherto been employed at all for measurements of viscosity at temperatures higher than are generally used in testing oils, etc., *i.e.*, about 250° C. Quite paradoxically it appears to be the only method of measurement which is possible experimentally over a large range of temperature and of viscosity, and for this reason may be called a quite universal method. In applying the method to temperatures as high as 1,600° C., which is about 400° C. higher than the property of viscosity had been hitherto measured by means of any method, certain modifications in the construction of the apparatus were necessary and which alone made possible the attainment of a relatively great accuracy under the rather severe experimental conditions. These modifications consisted in the use of Acheson graphite in the construction of all parts of the apparatus subjected to high temperatures, and in the use of a system of damping the suspended system so as to give to it the stability and aperiodicity characteristic of the familiar damped D'Arsonval galvanometer.

The present modification of the method of Margules has been applied without difficulty to measurements on slags over a range of viscosity from 200 to 3,000 (H_2O at 20° C. = 1). It is certain, however, that the method is applicable over a much wider range of viscosity than this.[4]

For a full discussion of the theory of the method the reader is referred to *Bureau of Mines Technical Paper* 157, pp. 7–10. It is sufficient here to say that the viscosity of the substance under examination is proportional

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[2] Max Margules: Über die Bestimmung des Reibungs—und Gleitungscoefficienten aus ebenen Benegungun einer Flussigkeit, *Sitzsungsberichte, Kaiserliche Akademie Wissenschaften (Vienna)*, vol. 83, pt. 2, p. 588 (1881).

[3] L. E. Gurney: The Viscosity of Water at Very Low Rates of Shear, *Physical Review*, vol. 26, pp. 98–120 (1908).

[4] See *Metallurgical and Chemical Engineering*, vol. 15, p. 541 (Nov. 1, 1916), where the application of the method to very viscous substances is discussed, with experimental data on a sample of asphalt over a range of viscosity from 80 to 4,000.

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to the torsion couple exerted on the inner concentric cylinder and inversely proportional to the speed of rotation in revolutions per second; while, conversely, the torsion couple is proportional to the viscosity and to the speed of rotation. With cylinders of fixed dimensions, therefore, and with the inner cylinder suspended by means of a calibrated steel ribbon, the viscosity of the slag melt is directly proportional to the product of the time of rotation of the outer cylinder and the deflection produced by means of the confined slag melt upon the inner cylinder.

The Application of Viscosity Data to Metallurgical Operations

Before taking up the description of the viscosity apparatus, furnace, and accessories, and tabulation of the experimental results obtained, it is advisable to consider briefly the application of accurate viscosity determinations on metallurgical slags to manufacturing processes, and also to related research in this field. Particular attention must be given in the present paper to a consideration of the principles of the metallurgy of iron, although in many other metallurgical operations, such as the smelting of copper for instance, a knowledge of the temperature-viscosity relations of different types of slag is of great importance.

Apart from the question of mining cost and freightage, the value of an iron ore sufficiently rich in iron to be considered marketable is largely dependent upon whether it can be made to yield economically a slag of desirable viscosity and desulphurizing power. A casual glance at the slag analyses in Table 1 shows at once the comparatively wide range of slag composition that has been found practicable by different manufacturers. In each case the particular slag composition was undoubtedly determined in a large measure by the composition of the ore mixture and fuel which it was deemed expedient to use, and also by the grade of iron produced. However, it is entirely probable that in certain cases the slag composition was not the optimum one from the standpoint of economy and excellence of product. It is one of the purposes of these investigations to determine what are the optimum conditions.

In spite of the lack of scientific research on the physical and chemical properties of slags at high temperatures, it is quite well understood what functions the slag must perform in the blast furnace. In the first place, it must be sufficiently fluid to flow from the cinder notch at the temperature which exists in the hearth. In the case of charcoal practice where desulphurization is a minor item, the viscosity of the slag at flush is a primary consideration. If, for instance, it is found that a siliceous charcoal slag possesses a viscosity of 800 at the temperature of the hearth, the question arises whether a limey slag would perform its functions properly if it possessed an equal viscosity. The limey slag may be

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prevented from performing these functions for one or both of the following reasons. In the first place, the limey slag with a viscosity of 800 might have an extremely high rate of change of viscosity with temperature, *i.e.*, this particular point on the temperature-viscosity curve might occur at a temperature where the slag underwent rapid softening or hardening with small changes of temperature. In the second place, it might not be at a sufficiently high temperature properly to desulphurize the pig iron.

The Desulphurization Process and its Relation to Slag Viscosity

The greater portion of the sulphur enters the furnace in the coke, in which it is present as ferrous sulphide to the extent of from 0.5 to 1.4 per cent. sulphur. The total quantity of sulphur is for practical purposes entirely distributed between the molten iron and slag. It may be assumed quite safely that the sulphur which is dissolved by the pig iron exists as ferrous sulphide, FeS. On the other hand, it is necessary to assume in the case of the sulphur dissolved in the slag that it exists for the most part in normal slags as calcium sulphide, CaS. In high-manganese slags, however, it is probable that a portion of the sulphur exists as manganese sulphide.

According to the well-known distribution law of Nernst,[5] when a substance is distributed between two immiscible solvents, such as molten iron and slag, the ratio of the concentrations of the given substance in the two solvents, when the condition of equilibrium is reached, is constant for any given temperature, *provided* the dissolved substance has the same molecular weight in the two solvents. Moreover, in the case of several dissolved substances each substance distributes itself as though the others were not present.

Let us consider an ideal case where manganese is entirely absent from the slag and iron. Since calcium sulphide is insoluble in molten iron, the distribution effect must be that of ferrous sulphide between molten iron and slag. The ferrous sulphide present in the slag undergoes the following reaction:

$$FeS + CaO \lesseqgtr CaS + FeO \qquad (1)$$

Since the concentration of lime, CaO, is very large in comparison with the concentrations of the other reacting substances, it may be considered to be a constant; wherefore, it would follow, from the general law in chemical equilibrium,[6] that the concentration of the ferrous sulphide of the slag is proportional to the product of the concentrations of the calcium sulphide, CaS, and the ferrous oxide, FeO.

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[5] Walter Nernst: *Theoretical Chemistry*, pp. 485–487. New York, MacMillan & Co., 1904.

[6] Nernst: *op. cit.*, pp. 434–435.

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Assuming ferrous sulphide to be distributed according to Nernst's distribution law, the concentration of the ferrous sulphide, and, therefore, of the sulphur, in the pig iron would be proportional to the product of the calcium sulphide and ferrous oxide concentrations in the slag layer, provided no other side reactions occurred.

However, there is a side reaction which must inevitably occur since ferrous oxide is not stable at high temperatures in the presence of carbon. This reaction is as follows:

$$FeO + C \lesseqgtr CO + Fe \qquad (2)$$

On combining equations (1) and (2), the net effect is seen to be:

$$FeS + CaO + C \lesseqgtr CO + Fe + CaS \qquad (3)$$

Equation (3) is the usual one given by most authorities to illustrate the course of the desulphurization process.

In the course of the chemical reaction represented by equation (3) it is necessary that FeS diffuse from the iron into the slag and also that the CaS, which is a product of the reaction, diffuse with sufficient rapidity to prevent the reversible reaction from coming to a standstill prematurely. For this reason the desulphurization reaction is conditioned largely so far as its speed is concerned by the viscosity of the slag, and resolves itself into a reaction confined to a great extent to the boundary plane between molten iron and slag. This boundary plane exists around the molten iron globules previous to their fall to the bottom of the hearth and between the contiguous layers of iron and slag in the hearth. To what extent desulphurization is completed previous to and during the fall of the iron globules through the slag layer is not known.

If it is true that during the smelting process equilibrium is reached under normal conditions in the case of the partition of sulphur between iron and slag, then the desulphurizing power of any given slag is independent of the viscosity of the slag, as such. In such a case the distribution of the sulphur would be a function only of the composition of the iron and slag and of the temperature of the hearth. However, if equilibrium is not reached under normal conditions, there cannot be in the strict sense of the word a distribution of sulphur, since the laws of distribution refer only to equilibrium conditions. In this case, the amount of sulphur absorbed by the slag depends on the speed of the reaction of desulphurization, which, as has been noted above, is conditioned by the viscosity of the slag, as well as by the temperature and composition of molten iron and slag.

With the help of the temperature-viscosity data of a number of different slags, obtained by means of the method which is described in this report, the Bureau of Mines is undertaking research investigations to determine the following facts: (1) By determining the speed of sulphide

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absorption by different slags at such temperatures as to possess identical viscosities, to deduce the effect due simply to differences of temperature and composition; (2) by determining the speed of sulphide absorption by the same slag at different temperatures, to deduce the effect due to changes in viscosity and temperature; (3) and by determining the speed of sulphide absorption by different slags at the same temperature, to deduce the effect due to changes in slag composition and in viscosity. Also, in each case, the final conditions of equilibrium are to be determined.

This work when completed should give a clear insight into the mechanism of the desulphurization process, and should be a test of the validity of the theory of Turner[7] and of Schafer,[8] that the silicate of the slag acts as a comparatively inert solvent for the spinels (aluminates) which alone are capable of reacting with the sulphide sulphur in the pig iron. According to this theory a slag low in alumina should possess a relatively weak desulphurizing power; and there should exist a lower limit of alumina content below which a slag could not be made to act satisfactorily in the blast furnace.

The Relation between the Composition of Blast-Furnace Slags and their Softening Temperatures Determined by the Cone Test

Formerly in the absence of a method of viscosity measurement at the extreme temperatures of the blast-furnace hearth, it was customary for investigators to obtain an idea of the softening temperature or deformation temperature of slags by means of the familiar cone tests, in which test pieces similar to Seger cones are made from the slag and the temperature noted at which these test pieces bend over, melt to a ball, or otherwise show signs of incipient fusion.

In the ceramic industries and also in determinations of the fusibility of coal ash, cone tests furnish valuable information, since in these cases the important point in view is to determine the minimum temperature at which deformation or softening occurs. If, for instance, a firebrick softens appreciably at 1,600° C., it is not a matter of technical interest to know how this brick would behave at 1,800° C., nor would a cone test give any information in regard to this question.

Mellor[9] has defined the softening temperature concisely, as follows: "The softening temperature measures the temperature at which the inward surface pressure of the substance becomes greater than those intra-

[7] *Journal of the Society of Chemical Industry*, 1905, p. 1142; *Stahl und Eisen*, vol. 26, No. 1, p. 172 (1906); *Metallurgie*, p. 164 (1906).

[8] *Ferrum*, vol. 5, p. 129 (Feb. 8, 1913).

[9] J. W. Mellor, A. Latimer, and A. D. Holdcroft: The Softening Temperatures of Lead-Silica Glasses, *Transactions of the English Ceramic Society*, vol. 9, pp. 126–149.

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molecular forces which hinder the molecules taking up a position of stable equilibrium-minimum surface area."

It is undoubtedly true that silicate mixtures at temperatures at or near their softening point possess a surface tension of considerable magnitude, as may be shown by the rounding off at the end of a glass rod when held in the flame of a Bunsen burner. What is observed actually is the resultant effect of decrease of viscosity and action of surface forces upon the softened body.

There is given in Table 1 the softening temperatures and analyses of 18 commercial slags. These determinations were made in a platinum-wire resistance furnace in an atmosphere of air, temperatures being measured by a platinum-rhodium thermocouple. The sulphur content of the slag is accordingly entirely converted to the sulphate by the oxygen of the atmosphere, and possibly a portion was volatilized. The measurements cannot, therefore, be considered as representing entirely the conditions within the blast furnace with its atmosphere of carbon monoxide and nitrogen. The slags are arranged in the table in the order of increasing refractoriness.

TABLE 1.—*Softening Temperatures of Slags by the Cone Test*

Slag Lab. No.	SiO_2	Al_2O_3	Percentage of		CaS	MnO	Softening Temperature, °C.
			CaO	MgO			
22,967	48	8	32	5	2.0	0.1	1,244–1,254
22,965	38	10	40	4	3.1	1.2	1,262–1,264
22,964	38	9	43	2	2.4	0.2	1,263–1,266
22,968	44	9	40	2	2.7	0.2	1,279–1,279
22,960	37	11	25	20	3.5	2.2	1,297–1,300
22,958	34	27	27	6	4.9	0.3	1,342–1,342
22,953	36	12	41	6	3.1	0.7	1,331–1,346
22,956	35	11	42	7	3.6	0.5	1,352–1,357
22,963	34	14	41	6	3.4	0.6	1,343–1,360
22,969	34	12	43	6	3.2	0.5	1,358–1,364
22,961	34	15	38	10	2.9	0.3	1,365–1,368
22,955	32	16	44	1	4.4	0.1	1,356–1,390
22,952	32	12	45	6	3.4	0.5	1,383–1,391
22,957	31	15	36	10	5.5	0.2	1,388–1,398
22,954	18	35	31	10	4.1	0.3	1,410–1,410
22,966	33	11	44	4	5.9	0.5	1,425–1,441
22,962	32	15	48	2	3.5	0.2	1,403–1,443

While it is impossible to draw any definite conclusions as to the variation of the softening temperature with the composition from these measurements, it is at once evident that, in so far as the effect of silica is concerned, low softening temperatures are in general associated with high

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silica content in the slag. In regard to the effect of lime or of alumina the data in the table yield nothing conclusive.

As is well known, the softening of a silicate mixture such as blast-furnace slag depends upon the amount of eutectic which is formed during incipient fusion and upon the viscosity of this eutectic. It is obvious that, given a sufficient amount of this eutectic to overcome the rigidity imparted to the test piece by the unfused portion, deformation will occur even though the eutectic possesses an extremely high viscosity. This softening may require time on account of the high viscosity of the eutectic mixture, but in cone tests sufficient time is usually furnished for this slow deformation.

Reference to the work of Rankin and Wright[10] at the Geophysical Laboratory on the system lime-alumina-silica, shows that the minimum ternary eutectic of this system is composed of CaO, 23.25 per cent., Al_2O_3, 14.75 per cent., and SiO_2, 62 per cent., which melts at a temperature of 1,170° C. Further examination shows that in all cases low-melting binary or ternary eutectics in the system lime-alumina-silica correspond to a higher silica content than is usually found in blast-furnace slags. Therefore, in all blast-furnace slags which possess more than the usual amount of silica there is formed on incipient fusion a large amount of a low-melting eutectic, which when it attains a sufficiently low viscosity, causes the test piece to soften and deform visibly.

It follows that high-silica slags, which are known to be more viscous at furnace temperatures than more basic slags, possess quite paradoxically a very low softening temperature. One might attempt to explain the high viscosity of siliceous slags in practice by claiming that such slags produce a low hearth temperature, and that the high viscosity is due to the low temperature of the slag. Whether this be true or not, the measurements which have been made in these investigations show conclusively that at all practicable hearth temperatures a siliceous slag is much more viscous than a normal slag at the same temperature.

If, therefore, one should assume that the slags given in Table 1 required the same amount of superheating above the softening temperatures there given, 200° C., for instance, no importance could be attached to subsequent deductions, because actual measurements of viscosity and temperature show that during a range of 200° of superheating certain slags attain a fluidity three times as great as others.

Emphasis should be placed upon the distinction between the physical melting point and the softening temperature of silicate mixtures, which latter is often loosely referred to as the "melting point." The former refers to a temperature which is perfectly definite in the case of the majority of silicate mixtures, viz., the temperature at which the *last*

[10] G. A. Rankin and C. E. Wright: Ternary System: $CaO - Al_2O_3 - SiO_2$, *American Journal of Science*, ser. 4, vol. 39, pp. 1–79 (1915).

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crystalline phase disappears upon slowly raising the temperature of the melt. Only in the case of silica[11] itself and certain alkaline feldspars[12] does the physical melting point appear to be a rather uncertain point. The softening temperature may differ enormously from the physical melting temperature, nor can deductions be made simply on the basis of melting-point determinations as to the relative positions of the softening and melting points, with respect to each other. For complete information in regard to the melting points and stability relations of the silicates the reader is referred to the monumental work[13] of Day, Shepherd, Rankin, Wright, Merwin, and Sosman at the Geophysical Laboratory.

Slag Temperatures Measured at the Blast Furnace

In the course of the investigations described in the present paper a number of attempts were made by the author, with the coöperation of Dr. J. K. Clement, physicist of the Bureau of Mines, to determine slag temperatures by means of a Holborn-Kurlbaum optical pyrometer. The conclusion reached in the course of these observations was that, due to the impossibility of obtaining a clean surface on the outflowing slag and of obtaining a slag which was entirely free from fumes, such measurements, at the best, were liable to a very large error. Accordingly a special type of platinum platinum-rhodium thermocouple was constructed, the inner mechanism of the couple being protected by a sheath of Acheson graphite where subjected to the action of the molten slag.

For the sake of comparison, a number of simultaneous measurements of temperature by means of the optical pyrometer and thermocouple were made at furnaces Nos. 2 and 3 of a large Pittsburgh steel company. For purposes of illustration a few of these results are given in Table 2.

[11] C. N. Fenner: The Stability Relations of the Silica Minerals, *American Journal of Science*, ser. 4, vol. 36, pp. 331–384 (1912).

[12] A. L. Day and E. T. Allen: The Isomorphism and Thermal Properties of the Feldspars, with optical study by J. P. Iddings, *Publication* No. 31, *Carnegie Institute of Washington*, 1905.

[13] A. L. Day and E. S. Shepherd: The Lime-silica Series of Minerals, with optical study by F. E. Wright, *American Journal of Science*, ser. 4, vol. 22, pp. 265–302 (1906).

E. S. Shepherd and G. A. Rankin: The Binary Systems of Alumina with Silica, Lime, and Magnesia, with optical study by F. E. Wright, *American Journal of Science*, ser. 4, vol. 28, pp. 293–333 (1909). Preliminary Report on the Ternary System $CaO - Al_2O_3 - SiO_2$; a Study of the Constitution of Portland Cement Clinker, with optical study by F. E. Wright, *Journal of Industrial and Engineering Chemistry*, vol. 3, pp. 221–227 (1911).

G. A. Rankin and F. E. Wright: Ternary System $CaO - Al_2O_3 - SiO_2$, *American Journal of Science*, ser. 4, vol. 39, pp. 1–79 (1915).

G. A. Rankin and H. E. Merwin: The Ternary System $CaO - Al_2O_3 - MgO$. *Journal of the American Chemical Society*, vol. 38, pp. 568–588 (1916).

R. B. Sosman: The Common Refractory Oxides, *Journal of Industrial and Engineering Chemistry*, vol. 8, pp. 985–990 (1916.)

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TABLE 2.—*Temperatures of Slag at Flush by Means of Holborn-Kurlbaum Optical Pyrometer and Platinum Platinum-Rhodium Thermocouple*

Date	Flush	Temperature	
		H. K. Pyrometer	Thermocouple
Furnace No. 2.		°C	°C
Jan. 24, 1916.....	2d, a.m.	1,487	1,470
.....	3d, a.m.	1,487	1,572
Furnace No. 3.			
Jan. 24, 1916.....	1st, a.m.	1,465	1,525

Further measurements were made with the thermocouple alone. Some of these measurements are given in Table 3.

TABLE 3.—*Temperatures of Slag by Means of a Platinum Platinum-Rhodium Thermocouple*

Date	Furnace No.	Temperature, °C.	Date	Furnace No.	Temperature, °C.
Jan. 25, 1916.....	2	1,515	Jan. 25, '16	2	1,415
.....	3	1,485	.....	2	1,490
.....	3	1,502	.....	2	1,538
.....	3	1,440	.....	3	1,475

The average temperature at flush by means of the thermocouple was, on the basis of the data given in Tables 2 and 3, equal to 1,500° C. for furnace No. 2, and 1,485° C. for furnace No. 3, with a variation in temperature at flush during the day of 108° C. (the 1,415° reading is omitted since its correctness is questionable) in the former case, and of 85° C. in the second case. Both furnaces appeared to be working perfectly normally, so far as could be judged from the behavior of the slag at flush.

Table 4 gives the analysis of the slag from both furnaces on the day upon which the temperature readings were taken.

TABLE 4

	Furnace No. 2, Per Cent.	Furnace No. 3, Per Cent.
SiO_2.....	35.18	37.60
Al_2O_3.....	12.08	12.46
CaO.....	43.73	40.80
MgO.....	4.23	3.66
CaS.....	4.21	3.65
MnO.....	0.39	0.55
	99.82	98.72

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An examination of what Johnson calls the curve of free-running temperature[14] shows a decided minimum at a silica content of approximately 44 per cent., this minimum free-running temperature corresponding to 2,450° F. or 1,339° C.

The results of the present investigations demonstrate conclusively that, for alumina content approximating 12 per cent., the slags of minimum free-running temperature correspond with a certain degree of approximation to a silica content of 36 per cent., rather than 44 per cent.

It is of interest to note that such a silica content (36 per cent.) corresponds to average blast-furnace practice using coke as fuel.

Of course, in charcoal practice using high-silica slags, it is possible to work with a more viscous slag than is the case in coke practice. However, in constructing such a curve as given by Johnson, the term "free-running temperature" necessarily refers to a temperature corresponding to a definite and constant viscosity.

The Temperature-viscosity Relations of Graded Series of Synthetic Slags

The only satisfactory method of studying the temperature-viscosity relations of blast-furnace slags and the effect of the different constituents on the viscosity is the determination of such relations for a graded series of synthetic slags in which one constituent is gradually replaced by another. Such investigations are now in progress in these laboratories.

To illustrate the experimental results which are being obtained, there is shown in Fig. 1 the temperature-viscosity relations of a synthetic slag containing 48 per cent. lime (curve A) and of a synthetic slag of the same composition except that 20 per cent. of the lime has been replaced by an equal percentage by weight of magnesia.

Description of Furnace and Accessories

The electric furnace used in the experiments of the author is of the granular carbon, resistance type, and is similar as regards the arrangement of resistor and supporting refractories to the one described by Jeffries.[15] The arrangement of the furnace and accessories are shown in Plate 1. This is also described fully in *Technical Paper* No. 157 of the Bureau of Mines.

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[14] J. E. Johnson, Jr.: On the Operation of the Blast Furnace, *Metallurgical and Chemical Engineering*, vol. 14, pp. 363–372 (Apr. 1, 1916).

[15] Zay Jeffries: Notes on the Gran-Annular Electrical Furnace, *Metallurgical and Chemical Engineering*, vol. 12, pp. 154–157 (March, 1914).

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Rotating Device

A ¾-in. steel shaft, by means of which the cylindrical crucible containing the slag is rotated, projects up into the furnace through a stuffing box placed centrally in the base of the sheet-iron shell. The lower end of the shaft is supported on a bearing fixed to the tripod. The steel shaft

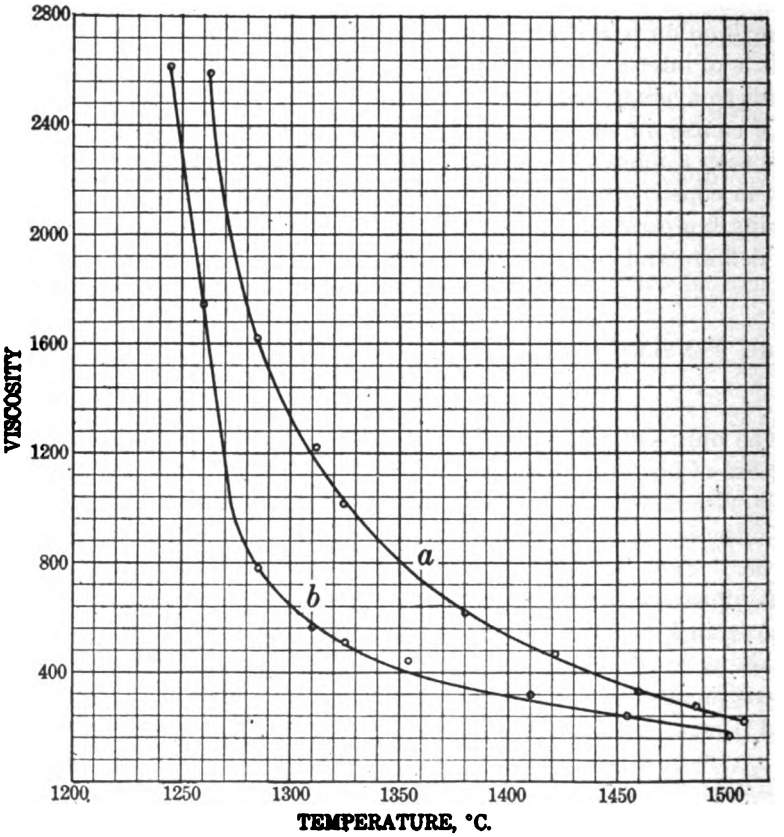

FIG. 1.—(*a*) TEMPERATURE VISCOSITY CURVE OF SYNTHETIC SLAG CONTAINING 48 PER CENT. CaO. (*b*) SAME SLAG IN WHICH 20 PER CENT. CaO HAS BEEN REPLACED BY 20 PER CENT. MgO.

extends only a short distance beyond the bottom of the furnace shell, and is bored for the reception of a slightly smaller supporting rod of Acheson graphite. This supporting rod is threaded at its upper end for the reception of the graphite slag crucible (see Plate 2), the graphite rod being tapered at its upper end to a diameter of ⅜-in. in order to eliminate, as far as possible, conduction of heat from the crucible. The crucible possesses an inside diameter of ¾ in. and an inside depth of 3 in., with a thickness of wall of ¼ in. The bottom of the crucible is ½ in. thick, so

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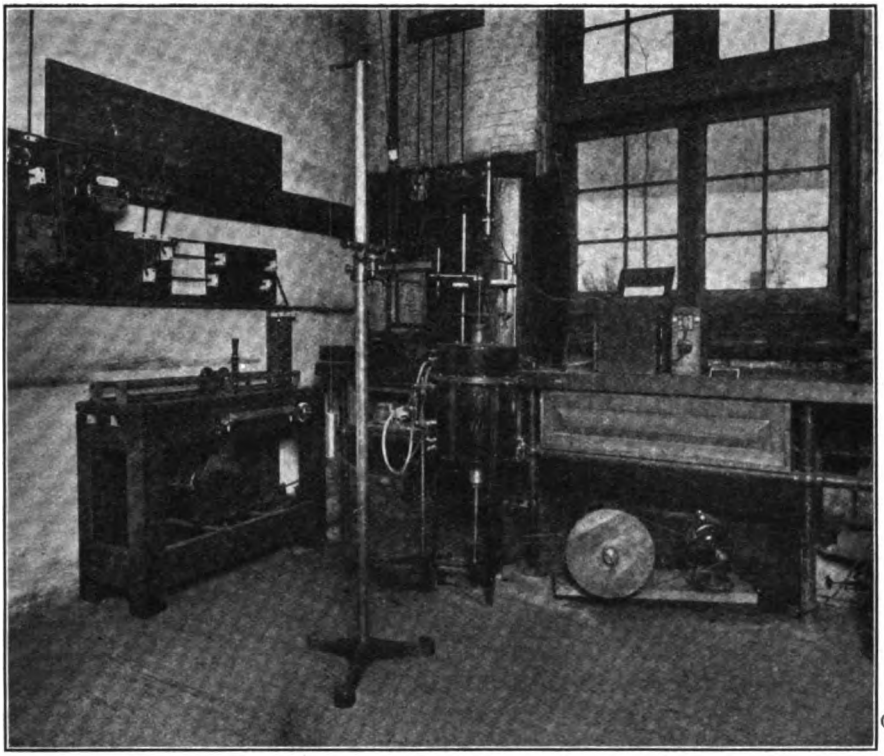

PLATE 1.—APPARATUS FOR MEASURING VISCOSITY OF BLAST-FURNACE SLAGS.

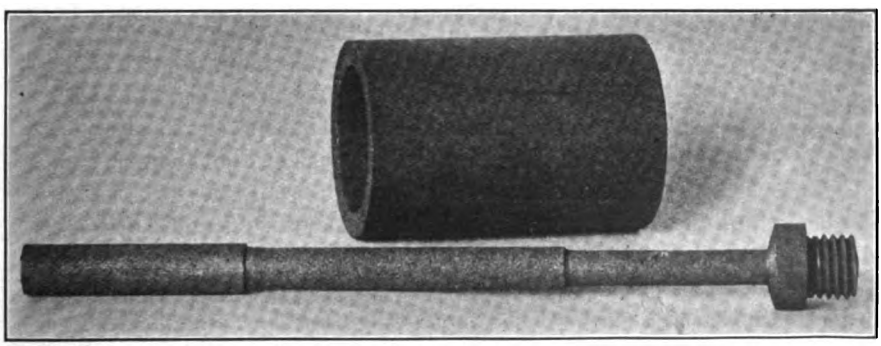

PLATE 2.—GRAPHITE CRUCIBLE AND SUPPORTING ROD.

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that the rod may be firmly threaded into it. The supporting rod fits snugly into the hole in the axis of the steel shaft to a depth of 2½ in., and its length between the top of the steel shaft and the bottom of the crucible is about 6 in. In the earlier experiments with the method it was thought necessary to introduce a Marquardt porcelain rod between the crucible and the steel shaft to prevent excessive heat conduction. By using a graphite rod tapered down to a diameter of only ⅜ in., no excessive heat conduction has been noted; and this also eliminates the undesirable experimental features of using a brittle porcelain part in the supporting system.

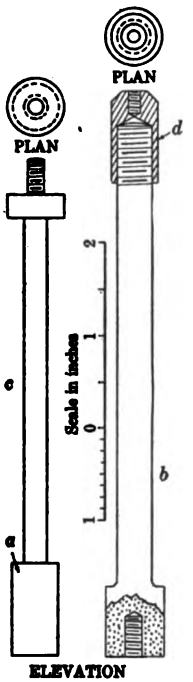

FIG. 2.—VISCOSITY SPINDLE AND CONNECTING ROD. LEFT VIEW, VISCOCITY SPINDLE—*a*, GRAPHITE SPINDLE; *c*, SPINDLE SHAFT. RIGHT VIEW, CONNECTING ROD—*b*, GRAPHITE ROD; *d*, STEEL CONNECTING COLLAR.

Source of Heat

The heating current is supplied by a transformer equipped with an adjustable panel which gives voltages ranging from 40 to 100 volts in steps of 2 volts. The maximum power input amounts to slightly over 5 kva. and the maximum current to 110 amp.

Furnace Atmosphere

The normal atmosphere of the furnace is a mixture of carbon monoxide and nitrogen, corresponding to that in the hearth of the blast furnace.

Viscosity Apparatus

Acheson graphite seems to be the only refractory available for such use as that of high-temperature viscosity measurements on blast-furnace slag. This possesses the advantages of having a very low coefficient of expansion, a relatively great mechanical strength at high temperatures, desirable working properties, and shows inappreciable alteration on being kept for long periods at high temperatures. The action of the molten slag on the viscosity spindle immersed in it is so small that the radius of the spindle decreases only approximately 0.005 cm. during a viscosity determination. The action of the slag is perfectly uniform over the surface of the spindle, so that the same spindle may be used repeatedly, due allowance being made for the slight change in its radius.

The construction of the viscosity spindle and connecting rod is shown in Fig. 2. The large cylindrical part, *a*, at the base of the spindle (on the left of the figure) dips into the molten slag, while the upper part is threaded

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into the base of the connecting rod (shown in the right of the figure). This connecting rod is also made of graphite, except for the steel cap at its upper end, *d*, by means of which the connecting rod is in turn attached to the upper part of the suspended system shown in Fig. 3.

The distance from the surface of the slag to the bottom of the steel suspension (*r*, Fig. 3) is approximately 21 in. Such a system would not in itself be at all stable under the conditions of measurement. It was therefore necessary to place a weight of about 200 grams between the sus-

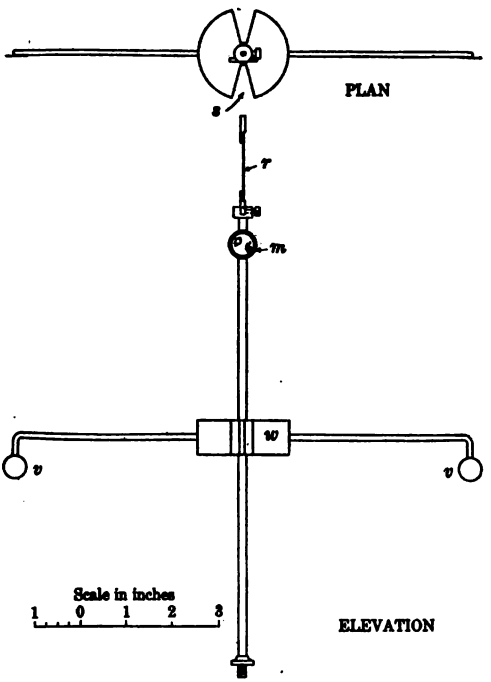

FIG. 3.—UPPER PART OF SUSPENSION SYSTEM. *m*, MIRROR; *r*, STEEL SUSPENSION RIBBON; *s*, OPEN SECTOR, *w*, WEIGHT; *vv*, VANES.

pension ribbon and the spindle at a point just outside the furnace shell and about 4½ in. below the suspension. This weight gives stability to the system, decreases the effect of vibrations upon the mirror, and holds the spindle in its position in the slag mass. Moreover, in order to further increase the inertia of the system about its longitudinal axis and to render the system aperiodic and practically non-susceptible to disturbing influences of short duration, the 200-gram weight (see *w*, Fig. 3) is furnished with two diametrically opposite vanes (*v* and *v'*) which dip into cups filled with a viscous fluid, such as castor oil, at a distance of about 4 in. from the axis of the system.

The mirror deflection is measured in the usual way by means of a telescope and scale.

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The entire suspension system is fixed to a rod that projects horizontally from an apparatus support, firmly fixed to a heavy table and provided with a millimeter scale for precise vertical adjustment of the apparatus. The furnace itself is first adjusted so that the steel shaft is in a vertical position, which insures the alignment of the crucible also. The point to which the suspension system is fixed is then aligned by means of a plumb bob with the axis of the shaft and crucible. In this manner the viscosity spindle is suspended centrally within the crucible.

The fluctuation of the scale reading rarely amounts to more than 1 per cent. of the scale reading. It is probable that the mirror reading which is a measure of the viscosity of the slag is more precise than the reading of temperature in certain cases, although not so accurate.

The Measurement of Temperature

The temperature of the slag was measured by means of an optical pyrometer of the Holborn-Kurlbaum type. The pyrometer is sighted directly upon the surface of the rotating slag through an open sector in the 200-gram weight (see Fig. 3).

As a check on the black-body conditions of the furnace used in the viscosity measurements, a sample of artificial diopside, $CaO.MgO.2SiO_2$, was prepared, and its temperature-viscosity relations determined in the same manner as adopted in the case of slags. This compound has a physical melting point at 1,392° C. and is converted, coincidently with its melting, from a crystalline substance to a fairly fluid liquid.

The results of these measurements are given in Fig. 4. The viscosity curve bends quite sharply upward in the neighborhood of the theoretical melting point, the observed difference of about 20° C. being probably due to the fact that the artificial diopside did not have exactly the theoretically correct composition. Analysis of the artificial diopside gave the following composition: CaO, 25.98 per cent., MgO, 19.48 per cent.; SiO_2, 55.08 per cent. The correct composition is: CaO, 25.84 per cent.; MgO, 18.58 per cent., SiO_2, 55.58 per cent.

Operation and Manipulation of Apparatus

The method of using the apparatus is as follows: 175 grams of slag is placed in the graphite crucible. This quantity of slag, when ground to 100 mesh, approximately fills the crucible. Although the height of the crucible internally is 3 in. the slag after having been melted and cooled forms a cylinder only about 1⅝ in. in length. The crucible, containing the slag, is properly placed, and the upper removable part of the furnace is fixed in position. The crucible is then rotated and the furnace is heated at such a rate that at the end of about 1½ hr. the slag reaches a temperature of 1,200° C. The rate of heating is then decreased in order that the large mass of slag may acquire the temperature of the heating

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tube and diminish the lag effect. When the slag has become sufficiently fluid, as determined by several tentative lowerings of the viscosity spindle, the suspension system is carefully lowered until the viscosity spindle rests on the bottom of the crucible. During this operation the rotation of the crucible is stopped in order to avoid the possibility of breaking the graphite spindle while it is being introduced into the viscous slag. The spindle is then raised exactly 0.5 cm. above the bottom of the crucible, the adjustment being made by means of the scale in the apparatus support.

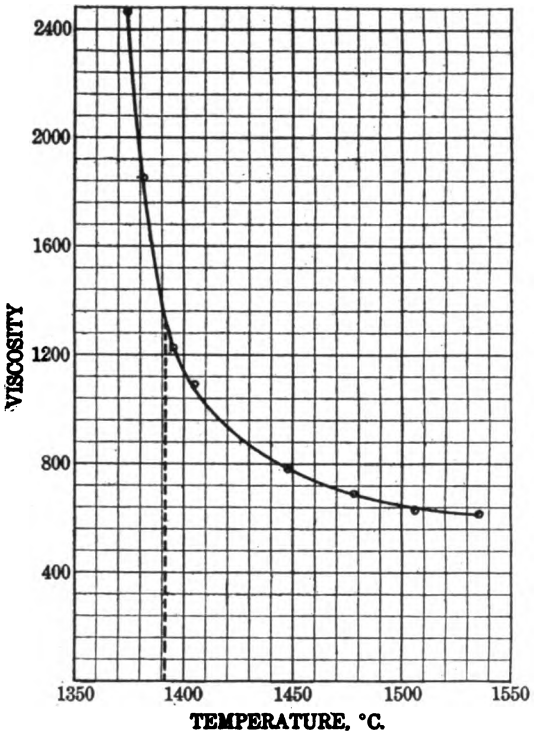

FIG. 4.—TEMPERATURE-VISCOSITY CURVE OF AN ARTIFICIAL DIOPSIDE.

After the suspension system has been lowered and the depth of the viscosity spindle adjusted, the temperature of the slag is slowly raised at a rate of 2° to 5° C. per minute, the temperature and scale deflection being noted at approximately regular intervals. When the highest temperature desired, usually 1,500° to 1,600° C., is reached, the heating current is so adjusted that the furnace cools at approximately the same rate that it was heated. Readings of deflection and temperature are taken, until the maximum deflection (corresponding to a viscosity of 2,000 to 3,000) is reached. The spindle is then raised out of the slag before the latter becomes hard enough to prevent withdrawal.

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Calibration of the Viscosity Apparatus

For the method of calibration used, the reader is referred to *Technical Paper* 157, p. 18. Measurements are referred to the viscosity of water at 20° C. as unity. In order to convert to c.g.s. units, these values should be divided by 100.

Results of Viscosity Measurements

The results of viscosity measurements on eight different commercial slag samples (Lab. Nos. 22,954, 22,962, 22,958, 22,963, 24,016, 23,663, 22,960, and 22,968) are given in Table 5 for the temperature range 1,275° to 1,575° C. The composition of the slags is shown by the results of chemical analyses given in Table 6.

TABLE 5.—*The Viscosity of Typical Blast-furnace Slags at Various Temperatures*

[H_2O at 20° C. = 1]

Temperature, °C.	Viscosity of Slag, Lab. No.							
	22,954	22,962	22,958	22,963	24,016	23,663	22,960	22,968
1,275	.....	.....	.....	1,400	2,600			
1,300	.....	.....	1,700	1,000	1,500	.....	1,900	
1,325	.....	.....	1,060	820	1,080	2,000	1,100	3,000
1,350	2,600	.....	800	680	800	740	840	1,800
1,375	1,150	.....	640	540	620	560	640	1,220
1,400	750	3,000	510	460	500	480	510	1,000
1,425	540	580	420	400	420	410	440	820
1,450	410	480	350	350	350	360	380	680
1,475	320	415	300	310	290	300	320	580
1,500	230	380	260	280	250	250	280	480
1,525	160	340	225	250	230	200	240	400
1,550	.....	290	.....	220	205	.....	.....	340
1,575	.....	.....	.....	.....	.....	.....	.....	310

TABLE 6.—*Results of Analyses of the Eight Slags*

Lab. No.	Composition of Slag											Total
	SiO_2	Al_2O_3	Fe_2O_3	FeO	Fe	TiO_2	CaO	MgO	MnO	CaS	Alkalies	Moisture	
	Per Cent.	Per Cent.	Per Cent.	Per Cent.	Per Cent.	Per Cent.	Per Cent.	Per Cent.	Per Cent.	Per Cent.	Per Cent.	Per Cent.	Per Cent.
22,954	18.30	35.30	0.00	0.47	0.05	0.58	31.24	9.69	0.35	4.07	0.50	0.02	100.57
22,962	31.54	14.79	0 00	0.07	0.22	0.29	47.65	1.80	0.21	3.56	....	0.10	100.23
22,958	33.67	26.62	0.00	0.28	0.51	0.28	26.67	6.43	0.33	4.86	....	0.12	99.77
22,963	34.27	13.78	0.00	0.07	0.28	0.56	41.30	6.39	0.55	3.35	....	0.04	100.59
24,016	35.76	[a]13.36	....	....	....	....	42.11	3.94	0.49	3.70	....	....	99.36
23,663	36.04	[a]12.10	....	....	....	....	42.04	4.03	0.35	3.92	....	....	98.48
22,960	37.18	11.46	0.00	0.31	0.11	0.52	25.33	19.58	2.21	3.51	....	0.04	100.25
22,968	43.56	9.48	0.00	0.21	0.38	0.19	40.18	2.08	0.21	2.75	0.50	0.05	99.59

[a] Includes Fe and FeO as Fe_2O_3.

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Temperature-viscosity curves were plotted, as shown in Figs. 5 to 8 for slags Nos. 22,954, 22,958, 23,663, and 22,968. The curves closely approximate in form a rectangular hyperbola. These figures contain all the experimentally determined points obtained in the heating and cooling

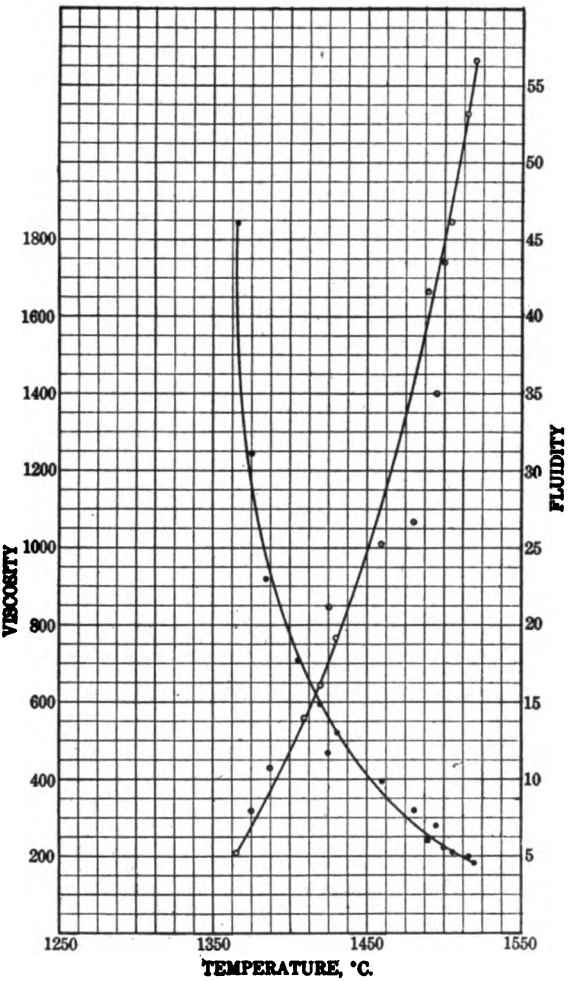

FIG 5.—RELATIONS OF TEMPERATURE TO FLUIDITY AND VISCOSITY SHOWN IN EXPERIMENTS WITH SLAG NO. 22,954.

of the sample, so that a smooth curve drawn through all the points must represent very closely the ideal conditions desired, the deviations tending to neutralize one another.

The temperature-fluidity curves are also given in Figs. 5 to 8 for the four slags mentioned above. The values are calculated by means of the

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familiar relation, Fluidity = 1/Viscosity. As was to have been expected from the general shape of the viscosity curves, the curves representing fluidity at various temperatures approximate in form a straight line.

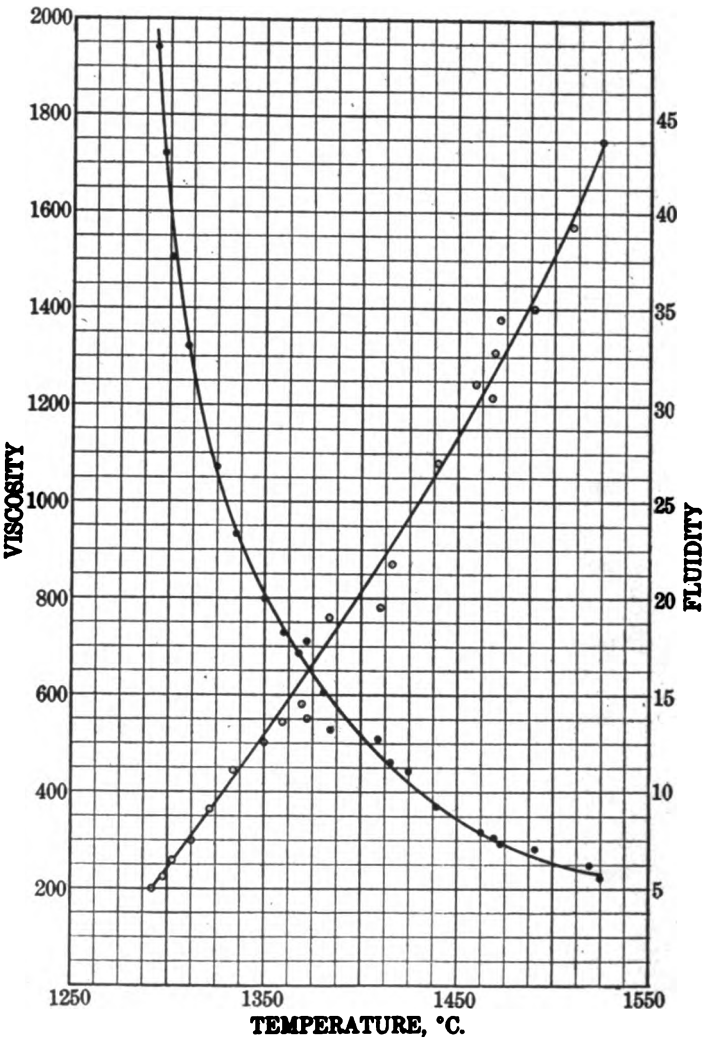

FIG. 6.—RELATIONS OF TEMPERATURE TO FLUIDITY AND VISCOSITY SHOWN IN EXPERIMENTS WITH SLAG NO. 22,958.

The actual experimental data obtained in the case of slag No. 22,958 are given in Table 7, which contains results obtained with rising and falling temperatures.

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TABLE 7.—*Comparison of Viscosity Values Obtained with Rising and Falling Temperatures*

[Results obtained in experiments with slag designated Lab. No. 22,958]

Rising Temperature		Falling Temperature *a*			
Temperature, °C.	Viscosity	Temperature, °C.	Viscosity	Temperature, °C.	Viscosity
1,382	605	1,292	1,940	1,382	611
1,410	513	1,297	1,707	1,387	524
1,415	460	1,302	1,505	1,425	446
1,425	442	1,312	1,319	1,440	375
		1,322	1,074	1,466	323
1,463	320	1,337	922	1,470	304
		1,350	805	1,488	285
1,473	291	1,359	728	1,525	228
1,510	255	1,368	689		

a Results are given in reverse order, for easy comparison with results under "Rising Temperature."

Duplicate determinations on separate samples of the same slag gave results which agreed well within the known errors of measurement. In Table 8 are given the results of duplicate determinations on slag No. 23,663. The deflections observed with falling temperature are given for both slags, these deflections being proportional to the viscosity of the melt.

TABLE 8.—*Duplicate Measurements on Slag No.* 23,663

Original		Duplicate	
Temperature, °C.	Deflection	Temperature, °C.	Deflection
1,510	26		
1,487	31	1,487	32
1,477	34	1,472	34
1,460	38		
1,450	39	1,450	40
1,435	40	1,440	40
1,425	42	1,430	43
1,410	44	1,410	45
1,387	48	1,387	48
1,337	90	1,347	82

GENERAL APPEARANCE OF SLAG MELTS

When cold the slag was easily removed from the graphite crucible by pressing on the bottom of the melt through the hole into which the graphite crucible support is threaded. The slag on cooling shrinks away

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from the graphite crucible wall sufficiently to prevent objectionable binding and sticking. The melt as taken from the crucible is cylindrical with an upper surface which is almost flat, as the viscosity at the moment when the spindle was withdrawn was not great enough to prevent the slag from flowing into the space previously occupied by the graphite spindle.

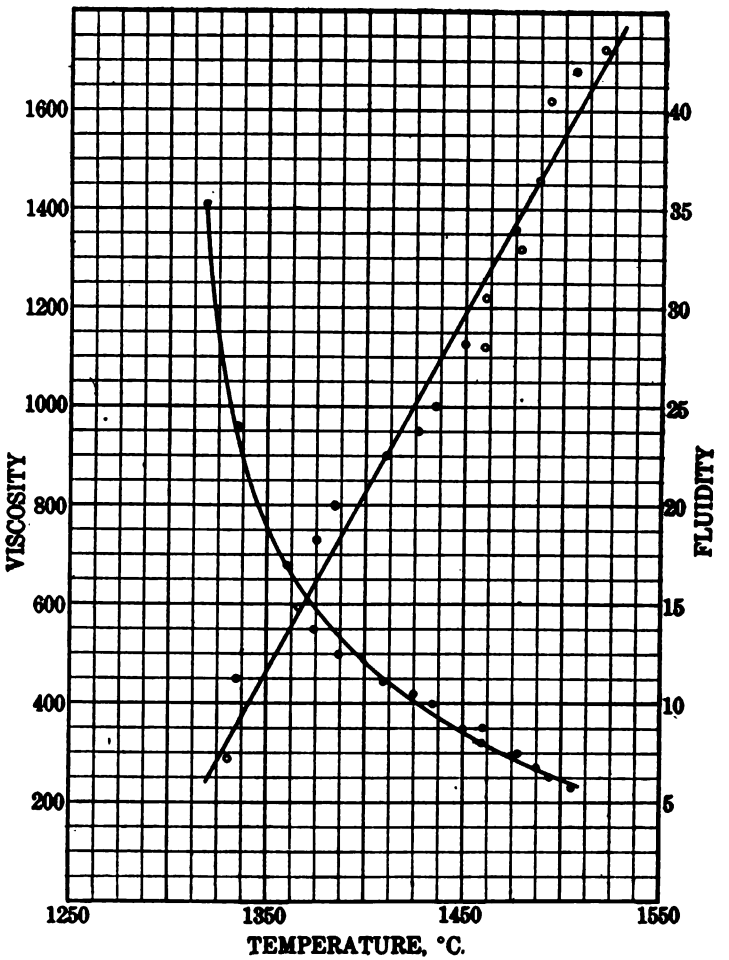

FIG. 7.—RELATIONS OF TEMPERATURE TO FLUIDITY AND VISCOSITY SHOWN IN EXPERIMENTS WITH SLAG NO. 23,663.

Plate 3 shows photographs of six fragments of slag melts after removal from the crucible. Samples Nos. 1 and 3 (reading from left to right) are the upper and lower portions of the same melt (slag No. 22,968). Sample No. 2 is a fragment of melt from slag No. 22,954, while sample No. 4, containing the lower end of a viscosity spindle embedded in it,

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is a fragment from a duplicate determination on the same slag. In this case the spindle was not removed soon enough, and in attempting to do so the shaft was broken off at the surface of the melt. Sample No. 5 is a fragment of the artificial diopside melt used in calibrating the black-body conditions of the furnace, as described hitherto. The crystalline struc-

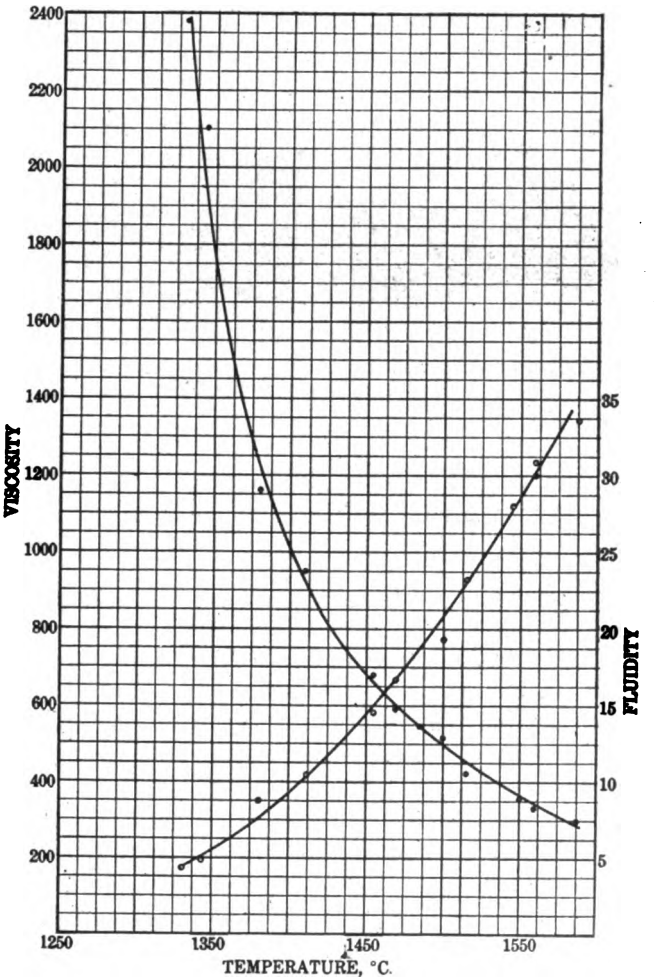

FIG. 8.—RELATIONS OF TEMPERATURE TO FLUIDITY AND VISCOSITY SHOWN IN EXPERIMENTS WITH SLAG No. 22,968.

ture of this melt is easily seen from the photograph. Sample No. 6 (on extreme right of photograph) is a fragment of a synthetic slag melt which does not differ greatly in composition from that of gehlenite, $2CaO.Al_2O_3.SiO_2$. The network of large crystals seen in the photograph of this melt are presumably crystals of gehlenite.

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Samples 1 to 4 also show the presence of numerous smaller crystals easily visible with the naked eye.

However, the crystalline structure of the cooled slag was not studied systematically in connection with the experiments reported.

The separation of metallic iron, owing to the strongly reducing atmosphere of the furnace and the duration and intimacy of contact between molten slag and the furnace atmosphere, was frequently observed. These metallic globules were found mostly on or near the surface of the

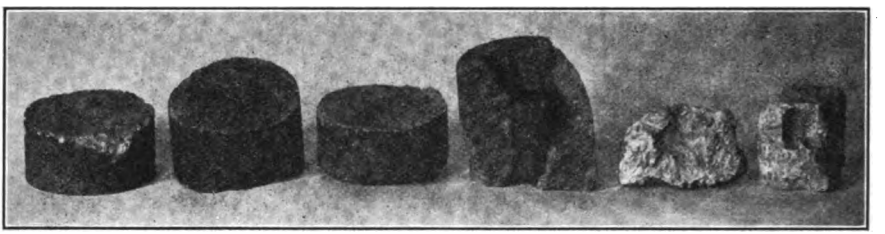

PLATE 3.—FRAGMENTS OF SLAG MELTS AFTER COOLING, SHOWING GENERAL SHAPE AND STRUCTURE.

cooled melt; some were 2 to 3 mm. in diameter. Their formation was only noted in the case of slags abnormally high in iron.

DISCUSSION OF EXPERIMENTAL MEASUREMENTS

The measurements described above give for the first time an accurate idea of the actual magnitude of the viscosity of blast-furnace slags in units which are capable of a physical interpretation, as well as of the rate of change of viscosity of different slags with temperature over a wide range of temperature and viscosity.

If an average slag temperature of 1,500° C. is assumed, an examination of the data given in Table 5 discloses the fact that the viscosity of the normal blast-furnace slag as it flows from the cinder notch approximates 250(H_2O at 20° C. = 1). This represents a viscosity less than that of castor oil at room temperature but greater than that of olive oil.

The viscosity of the eight slags, considered in Table 5, at a temperature of 1,500° C. varied from 230 to 480. A synthetic slag upon which measurements have been recently made showed a viscosity of 770 at 1,500° C., this slag containing 48 per cent. silica; while reference to Fig. 4 shows that at this temperature the artificial diopside containing 55 per cent. silica had a viscosity of 640. The effect of silica in such amounts upon the viscosity of silicates at temperatures above their melting points is thus clearly demonstrated, the increase in viscosity over that of normal

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slags being marked even in the case of diopside which contains no alumina and a high percentage of magnesia.

It would seem that even such high percentages of alumina as are present in the slag designated Lab. No. 22,954, although causing a high initial softening temperature, do not have a very noticeable effect in increasing the viscosity of the melt at high temperatures. This peculiar property of alumina, which may be a quite general characteristic, is probably associated in some manner with the uncertainty of its action within the furnace.

Comparison of Viscosity Measurements with Cone Tests

It was thought a matter of interest to compare the results of viscosity measurements with cone tests made with corresponding slags. The cone tests were made, as has been previously described, under conditions which converted the sulphide content of the slag to the state of the sulphate. For this reason the comparison is not a strict one. In Table 9 are given in relative order the softening temperatures of six slags and their viscosities at 1,400°, 1,450°, and 1,500° C.

Table 9

Slag Lab. No.	Softening Temperature, °C.	Viscosity at		
		1,400°	1,450°	1,500°
22,968	1,279–1,279	1,000	680	480
22,960	1,297–1,300	510	380	280
22,958	1,342–1,342	510	350	260
22,963	1,343–1,360	460	350	280
22,954	1,410–1,410	750	410	230
22,962	1,403–1,443	3,000	480	380

Acknowledgments

In concluding this account of the investigations carried out by the author, acknowledgment is made of the active interest and help of F. H. Willcox, metallurgical engineer, at whose suggestion and under whose general supervision the work was undertaken. F. G. Cottrell, chief metallurgist, and D. A. Lyon, metallurgist, have given helpful coöperation. The slag analyses were made by F. D. Osgood, junior chemist.

Summary

1. There has been described a modification of the torsion method of Margules which is applicable to the measurement of the viscosity of

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blast-furnace slag at high temperatures—the upper limit of temperature of 1,600° C. being imposed by the furnace refractories and not by inherent limitations in the viscosity apparatus.

2. Viscosity values are given for eight typical commercial slags, two synthetic slags, and an artificial diopside.

3. The temperature-viscosity curve generally approximates in form that of the rectangular hyperbola, whereas the temperature-fluidity curve approaches a straight line.

4. The average viscosity at 1,500° C. of eight commercial slags was found to be 301(H_2O at 20° C. = 1).

5. A refractory slag—that is, a slag possessing a high initial softening temperature—is not necessarily more viscous at high temperatures than a more fusible slag.

6. The theory of the method has been discussed, and the literature on slag viscosity and high temperature viscosity measurements has been reviewed.

7. The application of viscosity data to metallurgical operations has been briefly discussed, as well as the blast-furnace desulphurization process and its relation to slag viscosity.

8. The differences between softening temperature determined by a deformation test, the physical melting point of slags, and the viscosity of slags have been emphasized.

9. The nature of the free-running temperature of slags and the measurement of slag temperature at the furnace have been discussed.

Discussion

A. W. Fahrenwald, Socorro, N. M. (communication to the Secretary*).—When asked to discuss Mr. Feild's paper, I felt most highly complimented to have the privilege of commenting on such a worthy and needed piece of apparatus.

In consideration of the importance of the viscosity of blast-furnace slags and the relation existing between the melting point of a slag and its viscosity, the introduction of this apparatus should be a welcome means or assistant in the study of the physical properties of a slag. Up to the present time, it is a fact that this side of the practical working of the blast furnace has been sadly neglected by those investigating slags. It is easily seen that the fluidity of a slag is of as great importance as its melting point. It is not necessarily true that a slag having a low melting point will have a correspondingly low viscosity or be highly fluid. For example, a slag having a melting point of 1,100° C. may have to be superheated to, say, 1,300° C., in order to have as high a fluidity as another slag whose melting point is, say, 1,250° C. The latter in this case would be

* Received Jan. 30, 1917.

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the more economical. This point has been brought out very nicely by Mr. Feild.

Before discussing this paper further, I think it will be well to outline some of the points, a discussion of which may be in order:

1. Effect of variation in rate of speed of rotating crucible.
2. Effect of eccentricity of crucibles.
3. The depth of slag and relative position of surface must be the same in every case.
4. Effect of slag and high temperature upon crucible and spindle.
5. The effect of slip.
6. The volume of slag that can be maintained at uniform temperature is relatively small.
7. Irregularities in viscosity curve may or may not be due to mechanical or observational errors.
8. Reduction of iron by graphite crucibles which is greatly assisted by rotation of crucible.
9. Furnace difficulties.
10. Practicability of the apparatus.

Some of the points outlined above have been carefully considered in Mr. Feild's paper.

The effect due to variation in speed of the rotating crucible would be very small, owing to the slow rate of rotation of the rotating crucible as compared with the speed of an electric motor, especially if it be of the constant speed type and be supplied with uniform current.

Considerable error would undoubtedly result in case the relative position of surface of slag was not the same in all tests, owing to the difference in specific gravity of slags, depending upon the constituents. The same quantity by weight would not give equal volume, and hence would cause a noticeable variation in viscosity in the case of a series where one substance is replacing another. Would the error thus caused be sufficient to be taken into consideration?

All slags do not have a similar effect upon Acheson graphite. In my work, along the same line, I have noted particularly that highly basic slags have a greater tendency to adhere to the graphite on cooling and in some cases they are removed from the crucible with difficulty. Acid slags do not show this tendency. This characteristic would probably have a direct bearing upon "slip," therefore it would be expected that the effect or error due to slip would be more noticeable in determining the viscosity of highly acid slags than in basic ones, on account of its lesser "wetting" ability. It is probable that if any slip is present it could be entirely eliminated if the viscosity spindle and crucible were finely grooved, longitudinally, and especially if the crucible wall was farther removed, say at least 1 in. all around.

I cannot feel thoroughly convinced that the irregularities in the vis-

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cosity curve are due entirely to mechanical and observational errors, as Mr. Feild states, but think that possibly they are due to an actual critical point of some kind. So little is known regarding equilibrium between mineral constituents at high temperatures that I think Mr. Feild is hardly justified in ascribing fluctuations in his curves to error and not to some shifting of equilibrium.

For example, if a refractory mineral compound is present, it is known that the slag will consist of eutectics, or solid solutions, surrounding these crystals of compounds. On heating, the viscosity would perhaps not change much until the compound was all dissolved, when there would be a sharp brake in the curve. On cooling, when the temperature was reached where this compound commenced to separate out, there would begin a new rate of viscosity increase. There are many possibilities for different viewpoints, and in the absence of experimental facts these fluctuations in the viscosity curve cannot be satisfactorily accounted for. It is too bad that Mr. Feild did not take a few cooling curves on some of the samples described, along with the temperature-fluidity curves, as this would have thrown much light on the problem.

The intimacy of contact between slag and graphite parts, also reducing atmosphere in the furnace, causes much iron to be reduced, which, in the case of highly ferruginous slags, I find is considerable. It is also greater when a slag in mechanical mixture is heated and formed in the crucible than it is for formed slags, as from a blast furnace. The error caused by this separating out of metallic iron is so great that I have found it necessary to first form my slags (those having melting point below 1,375° C.) in a nickel or nichrome crucible, then subjecting them to the viscosimeter. While Acheson graphite is an admirable material, it has this disadvantage, it causes boiling in the crucible due to the liberation of oxygen, an objection that cannot be overcome until a more suitable material presents itself.

I notice that Mr. Feild has experienced the same difficulty with the furnace, in connection with the viscosity apparatus, that I have, and that is the cracking of the inner and outer tubes, between which is the resistor (carbon, crystol, etc.). Of course the cracking of the outer tube is not so serious, but the inner must not be deformed very much because local heating will result. F. A. Fahrenwald,[1] Cleveland, Ohio, tells me that the life of the tube can be increased 1,000 per cent. if plastered with alundum cement, and that when they crack he cements them up with the same.

This cracking is undoubtedly due to the unevenness with which the carbon resistor heats up, as alundum tubes do not crack if wound with platinum wire for a platinum wire resistance furnace. For my work on viscosity, in connection with the apparatus, the furnace is not the gran-

[1] Personal communication.

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annular type but one (a description to appear later) which is more rapid and in which any temperature can be attained in less than 1 hr. This furnace gives greater heat area with less bulk to the heating unit, no inner tube is required, hence the rotating crucible could be large. This heating unit will last several times longer than the gran-annular resistor.

Mr. Feild's viscosimeter certainly gives complete data on the viscosity of a given slag, but I question the practicability of the apparatus. Could this apparatus be used right at the blast furnace to work up a very fluid slag, and would the complexity of the apparatus and the skill required to operate it prevent its practical application?

I am using in my laboratory an improved form of the apparatus described by W. Mc. A. Johnson.[2] He uses a gas furnace which is a rather awkward arrangement for heating slag and measuring the temperature at which it drips through an elongated orifice of given diameter. I use an Acheson graphite crucible with an elongated orifice of known diameter and length, in an especially designed electric furnace in which temperature as high as 2,000° C. can be readily obtained. I superheat to say, 1,600° C., remove a graphite plug and allow slag to drip, for about ½ min., then insert plug again, allow slag to cool through 50° C. and take another drip sample for ½ min., and so on until the slag refuses to drip. From the weight of these samples I produce a viscosity curve similar to Mr. Feild's. The details and conditions for this method I hope to describe later.

In concluding, I would say that the apparatus described by Mr. Feild is a clever device and will no doubt find an open and hospitable field.

ROBERT B. SOSMAN, Washington, D. C. (communication to the Secretary*).—The author has made an excellent beginning on the difficult problem of the fluidity of molten silicates. From the standpoint of research on the pure silicates, it is to be regretted that he did not make some attempt to correlate the fluidity with the chemical composition, at least to the extent of recalculating the analyses to a molecular basis so that we can see about what is the relation among the important oxides in each of the slags examined. The next step, now that a satisfactory method and apparatus have been developed, would be to study a series of slags made up of pure oxides, preferably beginning with mixtures of only two or three oxides, so that a strict correlation of composition and fluidity, as well as of temperature and fluidity, can be made. The practical man is usually eager to have the properties of his own complicated mixtures determined at once, but unfortunately is not always able to use the results when obtained, for the simple reason that the variables are too numerous; they must be evaluated *one at a time*.

[2] A New Apparatus to Determine the Melting Points of Slags, *Electrochemical and Metallurgical Industry*, vol. 4, p. 262 (1906).

* Received Nov. 30, 1916.

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The author has overlooked in his review one important contribution of the character indicated. It is a paper by E. Greiner, on the dependence of the viscosity in silicate melts upon their chemical composition.[1] Greiner's method is not, I believe, as good as that used by Mr. Feild, but the results are of some comparative value. More measurements on the same plan would yield results of considerably more value, in the long run, than several times as many measurements on complex slags.

G. A. RANKIN, Creighton, Pa. (communication to the Secretary*).—In the introduction to this valuable paper, Mr. Feild has said, "The relation of high temperature to the viscosity of blast-furnace slags, aside from being of considerable theoretical interest and value, has an important bearing on the metallurgy of iron." This is true, but it would appear that one is justified in making much broader assertions than are contained in this statement; namely, that this investigation has an important bearing on the metallurgy of iron largely for the reason that it is of theoretical interest and value. In other words, for the reason that it is based on sound theory.

In so many of these so-called practical investigations, not only slag investigations but all investigations involving high temperatures, data are obtained by "hit-and-miss" methods of experiment with but little regard for many of the factors concerned. For that reason it is a real joy to read of Mr. Feild's work on the viscosity of slags, since in this study he has not only devised a very ingenious and practical piece of apparatus for viscosity measurements at high temperatures but has also shown due regard for all theoretical factors involved.

In regard to the viscosity apparatus as a whole, it would appear to one who has had no experience with apparatus of this nature that there is little room left for improvement. But if one considers simply the electric furnace employed, a platinum-wire furnace would undoubtedly be found more satisfactory than the granular carbon furnace. The advantages of the platinum over the granular carbon furnace are: (1) It is possible to obtain more uniform temperatures; (2) the furnace atmosphere can be more readily controlled. The one big drawback to a platinum furnace is, of course, the expense involved.

Concerning the preliminary viscosity measurements which Mr. Feild has made, it may be said that they are excellent as far as they go. It cannot be said, however, that sufficient data have been obtained to make possible any broad general statements showing just what effect each chemical component of the slag will have on the viscosity values at various temperatures. To obtain such data it would appear desirable

[1] *Inaug. Diss.*, Jena, 1907. Abstracted in *Chemical Abstracts*, vol. 2, No. 10, p. 1381 (May 20, 1908); vol. 4, No. 8, p. 1007 (April 20, 1910).

* Received Dec. 17, 1916.

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to make viscosity measurements of all possible mixtures of two or more of the principal chemical slag components. By proceeding in this way to the systematic determination of viscosity curves for all systems involved, it should be possible ultimately to state definitely the extent to which each component affects the viscosity of a slag. This would, of course, require the preparation of a large number of special slags and the accumulation of a large amount of data. It would not appear, however, that it will be a matter of any considerable difficulty to obtain these data, now that a satisfactory method has been developed for viscosity measurements at high temperature.

The data which it is thus possible to obtain with this method should be of great value to the iron and steel industry. Besides the determination of the viscosity of slags, this method is equally applicable to the study of the viscosity of glasses, a matter of extreme importance to glass manufacturers.

J. E. JOHNSON, JR., New York, N. Y. (communication to the Secretary*).—I have read with much interest Mr. Feild's paper describing his method of testing the viscosity of slags. This is a subject which is intimately associated with the economical and successful operation of blast furnaces, and while it has received attention in the past, very few of the investigations made have been of any value whatever because they determined softening points instead of free running temperatures, and as I have more than once pointed out, the interval between these two may vary from 50° or less to 200° or more, and as a variation of 200° in the free running temperature means the difference between economical and wasteful furnace operation, it is obvious that softening points are of little practical value.

Mr. Feild has apparently realized this and he has developed a method for determining the viscosity at any desired temperature, which examination indicates to be entirely practicable. The concordant results obtained confirm this conclusion.

It is not a simple matter to develop an apparatus which is reasonably free from experimental errors and which can be operated at temperatures approaching 3,000° F. for considerable periods of time, and I think that Mr. Feild is entitled to congratulation on the success of his efforts.

When it comes to the practical results obtained, however, I am afraid that the practical man will not obtain the benefit he hopes from such tests until they have covered a much wider range, and particularly until they are referred to an accepted standard.

For instance, if we assume that in Lake ore practice a normal slag is silica 33½ per cent., alumina 13½ per cent. and lime and magnesia 50 per cent., we shall not miss average conditions by any great amount.

* Received Dec. 5, 1916.

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Furnace men know how a slag like this will act, what coke consumption it requires with a given percentage of ore, etc. The real use of viscosity determinations is to establish the physical nature of such a slag as this under definite conditions and use it as a datum from which to measure the viscosity of other slags.

For this purpose, I would suggest that slag temperatures be determined at a furnace running on a typical slag of this general order. My personal preference as to the method of taking it would be to use an optical pyrometer of approved type on the slag as it runs over the slag dam. A few days spent in visiting normal working furnaces and making such temperature determinations would serve to establish fairly definite limits within which the temperatures of the slag vary, for they *do* vary within the length of a single cast. Now if one of these actual slags were subjected to test by Mr. Feild's process, within this temperature range, we should know the viscosity under what might be called a standard temperature condition. What we then need to know is *not* the change in viscosity at the *same* temperature which is brought about by a variation in the slag composition, but the variation in *temperature* to obtain the same viscosity. This temperature is what I have called for a good many years the critical temperature of the blast furnace. Upon it the whole thermal operation of the furnace depends.

With variations in the viscosity, on the other hand, we are not much concerned because we do not care to have the cinder thinner than a certain standard for a given purpose, while if it is thicker or stiffer than is required to meet the physical condition of flowing out through the cinder notch, the furnace will not operate and we are not interested in measuring this unusual viscosity but in avoiding it. For the latter purpose the only means available is to increase the temperature, which means increasing the fuel consumption in much greater degree.

There is only a small range of viscosity for each slag through which temperatures need be determined. This will reduce the number of determinations required and facilitate the obtaining of information which would be of great practical value to furnace men. Collateral determinations of the effect of calcium sulphide up to, say, 3 per cent., should be made for a few slags of different composition to see whether this component has any effect on free running temperature.

To boil the whole matter down, therefore, if viscosity determinations are to be useful to the furnace men, they should be:

First, based on the physical characteristics of a definite slag.

Second, plotted with composition and viscosity as the independent variables, and temperature as the dependent one.

Woolsey McA. Johnson, Hartford, Conn. (written discussion).—When we regard the number of British thermal units running into the billions that must be applied to metallurgical slags in the United States,

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we cannot but conclude that the physical chemistry of slags is a very important subject when measured in dollars and cents. A further contemplation of the subject shows us that it has been hitherto treated in too purely a practical manner. It is needless to comment at length on the cost of these heat units and on the cost of fluxes to produce these metallurgical slags, for it amounts to tens of millions of dollars annually. Accordingly, this research is of value to the country.

But we must all balance our results against our efforts. Therefore, while the speaker is unwilling to criticise unduly this meritorious research, he does criticise it in the interest of the profession for the reason that it seems to him too highly theoretical. The article is much too scientific to produce results at a minimum cost.

Let us explain our meaning. It has been said that there are always two sides to a question. It may be further said that there are always three sides to a question, the two extremes and the middle ground. In any industrial research there are the highly theoretical side, the purely practical side, and the theoretico-practical side. To study a slag and be practical in the studying, there is something needed that represents in the laboratory (or in microcosm) what actually happens macrocosmically in the blast furnace. There is such a thing as the art of metallurgical research occupying the middle ground between theory and practice.

The only direct connection to the operations of slag in the blast furnace in Mr. Feild's research that the speaker can see, after a diligent study, is that to the old practice of pulling out slag in strings with a hook.

Scientifically, the method employed is open to the criticism that the coefficient of friction between graphite and slag is a variant. Furthermore, the curves are not drawn with due regard to the theory of probabilities. It might be suggested that the influence of fluorspar on the fusibility of slags would be a fertile field for investigation.

The speaker has made many determinations of a similar nature, and along lines that he hopes were "theoretico-practical," in the period 1904–1914. He originated for this purpose two forms of apparatus. In one, the temperature at which a slag runs through a standard graphite aperture was measured (this temperature being called "the running-point" of the slag); in the other, an apparatus of Acheson graphite resembling an hour-glass was used. In this, the time taken in running a given amount of any slag at a given temperature could be measured. For a detailed account of these, the members are referred to *Electrochemical Industry* (July, 1906), **4**, 262, and to *Trans.*, (1913), **47**, 220. Prof. Guess has used a similar apparatus, in measuring the running temperature of mattes.[1]

By these methods, 20 determinations can be made at possibly one-third the expense and without any skill as compared with Mr. Feild's

[1] *Trans.*, vol. 55, p. 775 (1916).

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method. What is equally important, the results could be translated into practical language at once and one determination gives the practical temperature needed.

GEORGE K. BURGESS, Washington, D. C. (written discussion).—Mr. Feild is to be congratulated for his successful application of physical measurements to the important metallurgical problem of the viscosity of blast-furnace slags over considerable ranges of temperature and his paper is an excellent illustration of what may be accomplished when the methods of physics are applied to metallurgy.

Among the questions that may be raised is that of the constancy or permanence at a given temperature of the viscosity value for what appears to be a definite slag as shown by analysis. In other words, does a slag of a given chemical analysis possess the same physical properties if held for a considerable time at a temperature at which it is fluid? Mr. Feild gives some figures, but does not appear to dwell on this point.

It would be interesting also to make determinations of density of the fluid slag, for which Mr. Feild's apparatus may be readily adapted.

Although it is a minor matter, I should like to see a little more definite check as to the correctness of the temperature determinations, for his arrangement of apparatus, by the optical pyrometer sighting upon the slag surface. The known freezing point of a definite chemical substance such as diopside or synthetic anorthite, or better perhaps of a slag itself, could have been observed, or a direct comparison made with the indications of a thermocouple immersed in the slag.

There is one item in this paper, however, to which I would take serious exception; that refers to the interpolated observations with Dr. Clement on the temperatures of slag at the blast furnace. In Table 2 there is here shown an apparent and considerable discrepancy between the values as given by the optical and thermo-electric pyrometers, and it is clearly to be inferred that the authors do not place reliance on their optical determinations. If, however, as explained in my paper on Temperature Measurements in Bessemer and Open-Hearth Practice, they had made the necessary corrections (which they do not say was done) to the readings of the optical pyrometer, the discrepancy noted would be practically eliminated. To the optically determined temperatures of Table 2 should be added some 60° C. for the emissivity of the slag (the most probable value of this emissivity is 0.65). The average temperatures are then: optical = 1,540° C., thermo-electric = 1,522° C. One would expect the latter to be somewhat low.

Furthermore, since it was found possible to immerse in the slag stream a closed-end graphite tube, it would have been easy to obtain true temperatures optically by sighting the pyrometer into this tube; incidentally this observation combined with the surface observation would have given

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the emissivity of the slag, a characteristic the limits of the numerical range of which it is highly desirable to determine for as many slags as possible.

R. H. RICHARDS, Boston, Mass.—One point occurs to me in regard to the details; I am not able to judge whether there is anything in it or not. Years ago I was asked by Gen. Paine to try different surfaces for the yacht Volunteer, when the international race was to take place. He wanted to get the one that gave the least friction. We used this very same apparatus to put our various substances upon to test the question of friction, and of all the substances we tried, graphite gave the least friction, it was the most perfect lubricant. Now the question I would ask is, whether graphite, which, in the cold, gave the most perfect lubricant, is really the best substance to use in viscosity tests? Do you want a substance that gives the greatest friction, in order to get this viscosity measure? The second question is, did the graphite stay as graphite when the test was being made?

J. W. RICHARDS, So. Bethlehem, Pa.—I would rather have seen the title of this paper "The Fluidity of Slags;" that is the thing the blast-furnace man is usually interested in most. I wish to call particular attention to the fact that, on page 650, the slag number 22,954, the first one in the list, which is the most viscous at the low temperature and the most fluid at the high temperature, is the highest in alumina of the whole set. It contains 35 per cent. of alumina, and yet at 1,500° it is the most fluid of all the light slags tested. This is quite in line with the fact that if you reduce the silica so as to make room for the alumina, you can get high alumina slags of quite satisfactory fluidity for iron blast-furnace purposes. For low-melting, iron-lime slags, in lead or copper smelting, alumina is of course objectionable above a small amount, but this limitation does not apply to iron blast-furnace slags, which are essentially lime silico-aluminates. I wish to make the following general remarks concerning slags: In general, slags are not of definite chemical composition, it is a mistake to assign them definite formulas. A slag with three constituents, for instance, is a sort of triangular compound, and its properties cannot be inferred from the properties of any two of its constituents, taken two by two. It is a true ternary compound. In fact, if you do get a slag which corresponds to a definite chemical formula, it is usually the slag that you ought to get away from, since it is generally the least fusible of all the compositions that you can get of those ingredients; it occupies a maximum on the melting-point diagram, so that the effort of the blast-furnace manager is to get away from such a maximum which has a definite chemical composition, toward the eutectics which we all know do not correspond to definite chemical formulas. Let us further avoid considering that slags are practically chemical compounds

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of the ingredients, two by two; they are a combination of all the ingredients present, three, four, or five, whichever it may be, and they cannot be separated in the melted state into their constituents. When they have solidified, you may pick out the various constituents, two by two or three by three, but when in the melted state, the slag is homogeneous and does not contain any distinct constituent.

G. A. GUESS, Toronto, Ont.—The paper deals only with iron blast-furnace slags, and is not of so much interest to copper and lead men. I remember that old paper of Dr. Hofman's, giving the formation temperature of various silicates. Some of our students attempted, a couple of years ago, to measure the fluidity of slag along those lines, to see how the fluidity curve would agree with the formation temperature curves as given by Dr. Hofman. We used a little graphite cup, with a hole in the bottom, similar to the cups that were used for the melting temperatures of matte as described in the paper read by Mr. Lathe and myself a year or so ago, and we checked that also by getting typical slags, such as siliceous slags from Granby, from Anaconda and from the Tennesee Copper Co. The hole in the cup was sufficiently small so that, when the slag started to run, it did not run stringy, but would run in such a condition that I concluded that if it was hot enough to run out of that small hole, it was hot enough to run from the furnace. Although we did not get very far, we made a few synthetic slags, and work will be carried further still. It was interesting to compare what we would call the flowing temperature of a Granby slag with the flowing temperature of an Anaconda or Tennesee Copper Co. slag. I considered that those things were interesting and might be useful, but we did not get very much out of it.

A. L. FEILD.—In regard to the communication from Dr. G. K. Burgess on the standardization of furnace conditions, I conclude that he has seen only a copy of *Bureau of Mines Technical Paper* 157 which described the present method. In the paper now under discussion, however, a measurement is given on diopside which shows that the temperature conditions in the furnace check with the theoretical within 20° C. This accuracy was considered sufficiently close for practical purposes, although it may be possible ultimately to secure greater accuracy. As to the measurement of slag temperature at the blast furnace, this problem was not entered into very extensively. The measurements were made simply to obtain some idea of what results are obtained by the use of an optical pyrometer without emissivity corrections, because in the literature there are a very large number of measurements made by means of optical pyrometers without corrections being applied, or at least references are made to such measurements. The correction requires a knowledge of the coefficient of emissivity. We were not equipped to obtain this value.

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The suggestion of Dr. Burgess, however, can be put into practice very easily with the aid of the emissivity measurement on blast-furnace slag which he has given in his discussion. Our thermocouple measurements on the other hand, were sufficiently accurate to determine what slag temperature prevailed at flush.

In regard to Prof. R. H. Richards' remarks, I would emphasize the fact that this idea of measuring viscosity by means of the torque on an inner concentric cylinder is one of the oldest ideas on the subject. It dates back about 40 years; it was emphasized in the paper that the idea was an old one. In regard to the use of Acheson graphite—while it is well known that graphite has a very small friction coefficient when a solid body is rubbed against it, the present method uses only a liquid in contact with graphite. There is no experimental evidence to show that there is a "slip." Measurements made on mercury in contact with glass with the highest precision at the University of Chicago have shown that there is no slip between the outer layer of mercury and the glass. As to the suggestion to make the surface of the outer cylinder rough, I may make the following statement: by using concentric cylinders which do not have any irregularities on their surfaces, there is obtained what is practically demanded—and by that I mean what is mechanically demanded—in order to make this measurement what it is supposed to be, *i.e.*, a measurement of viscosity. The measurement is made without any turbulent flow in the liquid and without the production of eddy currents or impact. It is simply a question of shearing the liquid uniformly; and if a paddle is used or if the sides of the crucible are corrugated, the torque exerted on the inner cylinder is due partly to the viscosity of the substance, but quite largely to the kinetic energy and impact of the liquid, this impact being exactly the same as is obtained in ordinary hydraulic problems. It represents energy and can cause a deflection of the inner cylinder.

In reply to the remarks of Mr. Guess, I may say that in a later publication[1] of our results, the work done by him and Mr. Lathe was referred to. At the time the present article was written it had not come to my attention. Mr. Guess' method of using an orifice is undoubtedly at present the only method that can be used around a blast furnace to get any idea of the "softening temperature," apart from cone tests. While it may have certain advantages over a cone test, both of these tests are deformation tests and have the element of time entering into them. If we take a crucible with a hole in the bottom, and put in the crucible some pitch or asphalt—hard enough to cause a hammer to rebound from it—the pitch or asphalt will finally all flow out of the orifice, showing that a great

[1] A. L. Feild: The Viscosity of Blast-Furnace Slag and its Relation to Iron Metallurgy, Including a Description of a New Method of Measuring Slag Viscosity at High Temperatures, *Transactions of The Faraday Society*, 1917 (communicated by Sir Robert Hadfield, F. R. S.).

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many bodies supposed to be solid at ordinary temperatures have a measurable viscosity; that is, the viscosity is not infinite. To a great extent, therefore, in the case of all deformation tests, the results secured will be determined by the length of the experiment.

In reply to Mr. Fahrenwald's inquiry as to the error introduced by the variation of the density of the slag, I would refer him to page 17, *Bureau of Mines Technical Paper* 157. Mr. Fahrenwald says that according to his experience highly basic slags have a greater tendency to adhere to Acheson graphite on cooling than acid slags. Laying aside for the moment the fact that "slip" has not been conclusively demonstrated to exist between a solid and a liquid for any single case, it is evident that the difficulty of removing a *cooled* slag from the graphite crucible is determined by the coefficient of expansion of the particular slag and can have no relation whatsoever with the *liquid* slag in its behavior toward the walls of the crucible.

While it is possible that further research may prove the existence of points of inflection in the first derivative of the temperature—viscosity curve which can be given a definite physical significance, the experimental data given in the report which is the subject of the present discussion are not sufficient to warrant a conclusion of such a hypothetical nature.

The amount of iron oxide present in blast-furnace slag lies usually between 0.05 and 0.50 per cent., so that the separation of metallic iron caused by the carbon monoxide atmosphere of the furnace is of no serious consequence. In the case of copper or lead blast-furnace slags, which contain considerable iron, the separation of metallic iron in undesirable quantities would doubtless be noted. This absence of easily reducible oxides in the iron blast-furnace slag precludes the existence of the undesirable "boiling" mentioned by Mr. Fahrenwald.

The practicability of the viscosity work which is the subject of the present paper is not confined to the possible operation of the furnace and apparatus at the blast-furnace by an operator of average skill and ability. The measurements recently made in this laboratory and other measurements now in progress will furnish the blast-furnace man with an accurate knowledge of the viscosity of his different slags, so that the only requirement imposed at the furnace is a chemical analysis of the slag. If the slag composition is known, our data will furnish complete information regarding the viscosity of the slag at all practicable furnace temperatures.

Dr. Sosman's suggestion that the viscosity or fluidity of pure oxides and of mixtures of two or three oxides should be examined is an excellent one. Such work has been in progress for the last 4 months and shows promise of very valuable results.

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Matte Granulation at Herculaneum, Mo.

BY S. PAUL LINDAU, B. S.,* HERCULANEUM, MO., AND HENRY B. SMITH,† RIVERMINES, MO.

(New York Meeting, February, 1917)

THREE years ago it was decided by the management to granulate the matte that is produced in the smelter of the St. Joseph Lead Co. at Herculaneum, Mo., thereby doing away with a large amount of labor in handling the matte and in subsequent crushing. On Mar. 1, 1915, the granulator was put in operation and up to Jan. 1, 1916, 18,735 tons had been granulated. The four blast furnaces have a daily output of about 280 tons of pig lead and 100 tons of matte assaying about 11.0 per cent. Pb. It is expected that projected improvements in roasting methods will eliminate more sulphur from the charge, and cut down the matte-fall to about one-half of the present figure.

The blast furnaces are tapped into movable forehearths or settlers, 6 ft. by 4 ft. by 22 in., the slag overflowing into 27-cu. ft. slag cars which are hauled to the dump by electric locomotives. Previous to the adoption of granulation, the matte was tapped from the forehearths into small slag pots which were dumped after cooling. The matte cakes were broken with sledges, and transported to the crushing plant for fine crushing and screening. Here the matte was reduced to ¼-in. size before being fed to the roaster bins. A Wedge and a Holthoff mechanical furnace are used for preroasting before sintering on the Dwight and Lloyd machines. The matte is returned to the furnaces as an iron flux until the gradually accumulating copper content reaches about 7 per cent., after which it is put aside to be concentrated in a small copper blast furnace, the resultant high-grade matte being sold.

When the matte does not settle readily from the slag, two settling devices are often used, the second one being a large catch-pot, which gathers any matte that may overflow from the forehearth, the slag then overflowing from the catch-pot into the slag cars. When full of matte, the catch-pot is removed, allowed to cool, and the contents broken and crushed.

With the present plan of granulating, the matte is tapped into 6-cu. ft. iron ladles. These are transported to the granulating plant by a 5-ton

*Testing Engineer, St. Joseph Lead Co.
†Chemist, Doe Run Lead Co.

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overhead electric traveling crane, the runway of which extends the full length of the blast-furnace building. A cylindrical container receives the matte from the ladles. This container is a specially designed Traylor

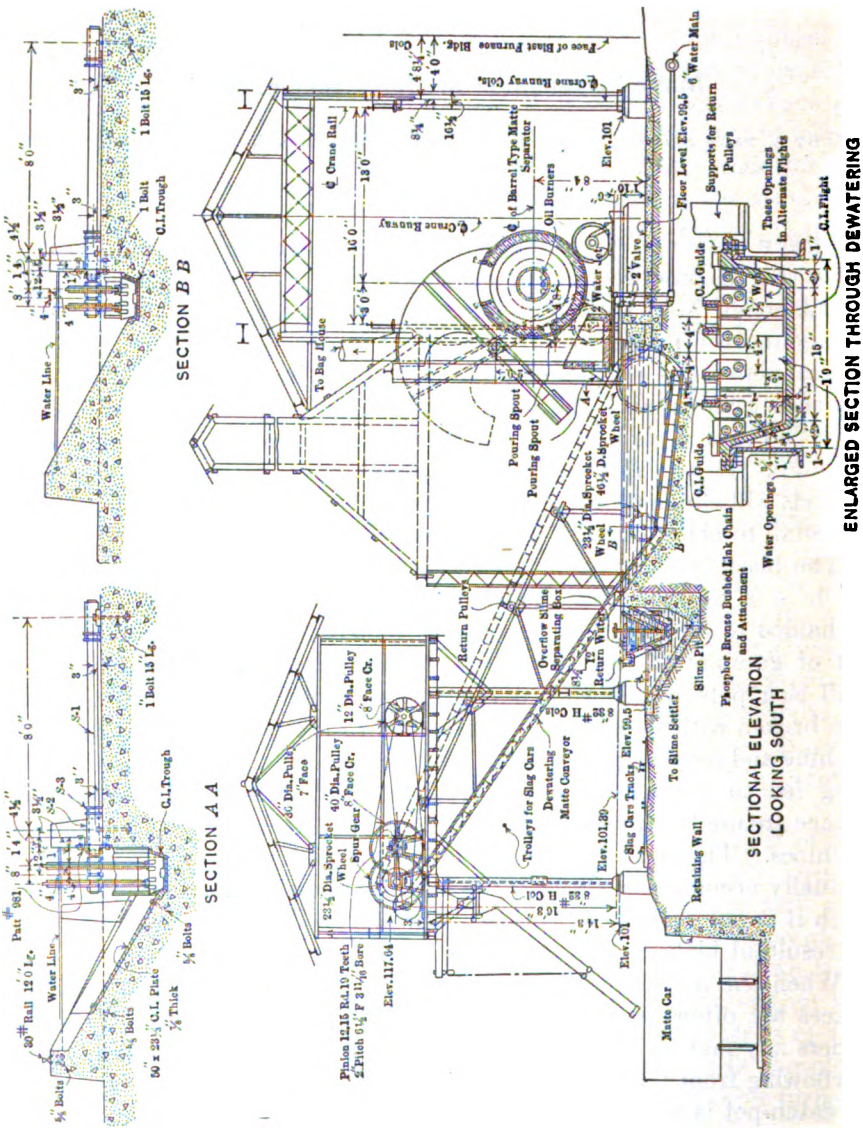

FIG. 1.—MATTE GRANULATING PLANT OF THE ST. JOSEPH LEAD CO., HERCULANEUM, MO.
ENLARGED SECTION THROUGH DEWATERING CONVEYOR TROUGH

copper converter minus the tuyères and with a 20-in. opening at one end for the flue and oil burners. It measures 77 by 120 in. inside, is lined with magnesite brick 9 in. thick and can be tilted by means of a 50-hp. motor. A steel flue 18 in. in diameter, leading from one end, conducts

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any fume to the bag house. Just under this flue are placed two oil burners for heating. Generally one is found sufficient to keep the matte in a molten state. The consumption of 18° to 20° Bé. crude oil is 250 gal. per 24 hr., or 2 to 2½ gal. of oil per ton of matte granulated. Air at 30 lb. pressure is supplied to the burners for atomizing the oil.

The granulation is accomplished by pouring the molten matte through two superimposed flat jets of water shooting horizontally into a concrete tank lined with cast-iron plates (Fig. 1). The stream of matte is disintegrated into small shot-like particles before reaching the body of water. The stream of molten matte is accurately directed onto the jets of water by a so-called pouring box, the spout of which is 6 in. above and 12 in. in front of the upper nozzle. This box is lined with common brick and the matte is poured directly into it by revolving the container. The pour holes are slots 2 in. wide by 8 in. high.

A dewatering drag conveyor removes the granulated particles of matte from the tank. The floor of the tank slopes down to the trough in which the conveyor operates, at an angle of about 30°. It is placed to one side, out of the line of the streams from the nozzles. It elevates the matte over the slag track and discharges into standard-gage railroad cars, which are weighed, and their contents sampled and emptied into the roaster bins.

In the end of the concrete tank opposite the nozzles, a V-sh ped settling box takes the overflow water. This box prevents the loss of the coarser slime, while the finer material settles out in a series of settling tanks, one overflowing into the other. An excelsior filter finally clears the water before it enters the circulating pond. There are two series of settling tanks, one in use while the other is being cleaned out.

Operation and Construction

The matte in the cylindrical container is kept fluid by one or both of the two oil burners before mentioned. The burners are placed so that the flame shoots slightly upward in the container. An oxidizing flame is used, though there would be some practical advantages if a reducing flame could be employed. It was found impossible, however, to maintain the required temperature with a reducing flame. If the matte remains in the container too long, an oxidized scum is formed which interferes with the pouring.

The matte is discharged from the barrel container through an opening in the side, 2 by 8 in., provided with a spout delivering into the pouring box which rests on the concrete wall of the tank directly over the water nozzles. This pouring box is made of sheet iron and is lined with common firebrick; inside dimensions are 18 in. by 18 in. by 3 ft. The matte tends to chill and build up in the box, but a narrow passage about 3 in.

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wide is easily maintained along the path of the stream. This box in turn discharges through an opening 2 by 8 in., the matte being directed by a cast-iron spout so that it meets the horizontal jets of water at an angle of 70° to 80°. All the matte should be broken up and be pre-chilled before striking the main body of water. As a rule the stream of matte is broken up by striking the upper jet while the lower jet insures further cooling. It was found that if some of the matte missed the jets or if the rate of pouring was too rapid, thus preventing the thorough pre-chilling of the matte, some of the semimolten particles united again into large lumps, frequently causing explosions in the tank. Likewise, too rapid pouring will promote the formation of large granules up to 1 in. in diameter, which are detrimental to good roasting. Too slow pouring allows the matte to chill before leaving the pouring box, thereby causing the opening in the spout to gradually freeze. The right speed was obtained after a little experimentation. A stream of matte as large as 2½ in. in

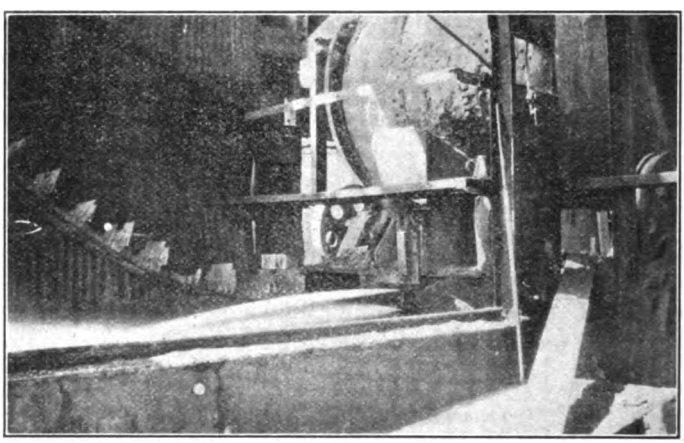

FIG. 2.—GRANULATION TANK, SHOWING JETS OF WATER, POURING BOX AND DRAG CONVEYOR.

diameter where it strikes the jets has been granulated satisfactorily. A hot matte makes a better product for roasting, because it is more uniform and finer. A good product is that of which 75 per cent. passes a 1/16-in. screen. The average rate of pouring is 3.2 cu. ft. of matte per minute.

The water jets are delivered through rectangular nozzles, the openings being ⅜ by 3¾ in. with the nozzles 5 in. apart. The supply of water is 100 gal. per minute under a head of 40 ft. The granulating tank is constructed of concrete, 17 ft. long and 7½ ft. wide, the floor being plated with 1-in. cast-iron plates, at a 30° slope into the conveyor trough. This degree of slope has been found sufficient to cause the granulated particles to run into the trough. The drag conveyor is driven

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by a 10-hp. motor at a speed of 30 ft. per minute and runs in a sectional cast-iron trough 4 in. deep and 15 in. wide. A very small amount of water, about 5 per cent., passes over with the matte into the railroad cars, drainage being assisted by notching the conveyor flights alternately in the center and on the ends, thus allowing the entrained water to escape and flow back into the tank. The tail sprocket wheel and the idler sprocket wheel (Fig. 1) are both under water. The shafts of these wheels extend through stuffing boxes in the sides of the tank, to the bearings on the outside.

Some lead settles out of the matte while in the container, about 30 to 60 pigs of 65 lb. each, according to the condition of the furnaces, being poured out daily. In case lead goes over and is granulated with the matte, it manifests itself by a sputtering and popping on the surface of the water. After the matte has been poured down to the lead level, the operator turns the container backward and pours the lead into a ladle, whence it is molded into pigs.

To prevent metal losses, the fumes are caught by a swinging hood which fits over the charge opening and connects with the blast-furnace flue leading to the bag house. The hood may be swung back so as to uncover the charge opening, when the crane is ready to pour a ladle of matte into the barrel container.

This installation requires only two men per shift for operation, one to operate the container and one the crane. The forehearth tappers at the furnaces attach the crane hook to the ladles. Under the old method of handling the matte by hand-pots, the cost was 87 c. per ton, which included hand-breaking. Crushing and screening amounted to 56 c. per ton in addition, which brought the total cost to $1.43 per ton. Granulation costs only 75 c. per ton, which makes the total saving of $1.43 − 0.75 = $0.68 per ton of matte granulated.

Summary

Matte granulated per 24 hr., tons	100
Rate of pouring, cubic feet per minute	3.2
Oil consumed per 24 hr., gallons	250
Oil per ton of matte granulated, gallons	2.5
Water used per minute (under head of 40 ft.), gallons	100

Screen Analysis on Granulated Matte

Inch-Mesh	Per Cent.
On ¼	5
On 3/16	4
On ⅛	14
On 1/16	35
Through 1/16	42
	100

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Notes on Flotation—1916*

BY J. M. CALLOW,† SALT LAKE CITY, UTAH

(New York Meeting, February, 1917)

THE results obtained by pneumatic flotation throughout the country on all classes of ore, and the tonnage now being treated by this particular method, speak for themselves. Its advantages over the so-called agitation process are now established by facts and data from practice. The comparative efficiency of a rapidly revolving paddle, as a means for aërating an ore pulp, and of a cycloidal or turbine blower for the same purpose, would seem to be so obvious as to leave no room for discussion.

Practitioners in this art are to be congratulated on the acquisition of the valuable literary contributions that have recently been made on the theory of flotation by Messrs. Ralston, Van Arsdale, Taggart and Beach, and others, and the very valuable and able articles by Messrs. Laist and Wiggin on "Flotation Concentration at Anaconda, Mont.," by Dr. Rudolf Gahl on the "History of Flotation at Inspiration," and by David Cole on "The Advent of Flotation in the Morenci District." It is not within the province of this article to attempt an answer to the various issues raised in regard to the theory, or to defend the one advanced by me a year ago. But since discussion usually benefits the profession, a reply will be made to some of the statements made by the last-named writers.

COMPARATIVE SENSITIVENESS AND ATTENDANCE

Concerning the article, "Flotation Concentration at Anaconda, Mont.," by Laist and Wiggin, a little explanation is in order. It should be stated that, with the exception of about 24 days at the end of the testing period, the Callow experimental plant at Anaconda was not run under my direction. This perhaps accounts for some of the conclusions drawn on page 499 of the descriptive paper. The conclusion that the use of acid seems to be of considerable advantage, calls for some comment. While the tests conducted by the Anaconda Copper Co. do show acid to be of considerable benefit, this is a consequence of using acid sludge as a flotation agent. My belief has always been that the Anaconda

* Originally presented at a meeting of the Utah Section on Dec. 16, 1916.

† Consulting Engineer.

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ores (at least the reground sands) can be floated with coal and wood tar products in a neutral or alkaline circuit, but it was not until the 24-day period, referred to above, that an opportunity came to demonstrate it. Even in this short time, the results obtained very closely approached the best of those using an acid circuit. Still better results would probably have been obtained had sufficient time been available for the necessary experimenting, but the plant was shut down, because the grinding mill was required elsewhere.

The statement is made that "on account of utilizing the grinding mill as an agitator, the Callow machine requires less power than the Minerals Separation machine." By using a neutral circuit, the tube mill could have been used as an emulsifier, and the low-power consumption of the Callow scheme taken full advantage of, effecting a saving estimated at about 2,000 hp. for the eight operating sections, at the same time avoiding all the destructive effects of an acid circuit, which must amount to another appreciable sum. The grinding mill as an emulsifier is an advantage to a large extent thrown away with the Minerals Separation machine, but is taken full advantage of with the Callow, consequently it would appear that the conclusion that the Callow machine takes less power than the Minerals Separation could well have been made without the preliminary qualifying statement.

Referring to the statement that "the Callow machine is more sensitive and requires more attention than the Minerals Separation machine," the fact that at the Inspiration plant one man per shift handles four 800-ton sections of Callow machines, or 3,200 tons, while at the Miami Copper Co.'s plant two men per shift handle a like tonnage in 60 cells, or 1,600 tons per man, is sufficient evidence that Callow machines, at least at these plants, are neither sensitive, nor require much attention. At Anaconda on eight Minerals Separation machines, treating 3,000 tons per day, four men per shift are employed, each handling the equivalent of 800 tons; local opinion at Inspiration is that two 800-ton sections (consisting of two 10-compartment, and four 6-compartment Minerals Separation machines) is the limit that one man can properly attend. In other words, 1,600 tons per man with Minerals Separation machines compared with 3,200 tons per man in the Callow sections of the same plant is the limit of a day's work. It will be evident, from these comparisons, which machine is the more sensitive or requires the most attention.

Concerning the paper, "History of the Flotation Process at Inspiration," by Dr. Rudolf Gahl, a few comments are in order. It should not be overlooked that W. B. Thompson was the principal factor in its early application here. I had just completed the last of the gravity flowsheet tests, when he brought the subject of flotation to the attention of his fellow directors, and it was immediately arranged that I should go to London with 20 tons of Inspiration ore, and watch tests by the Minerals

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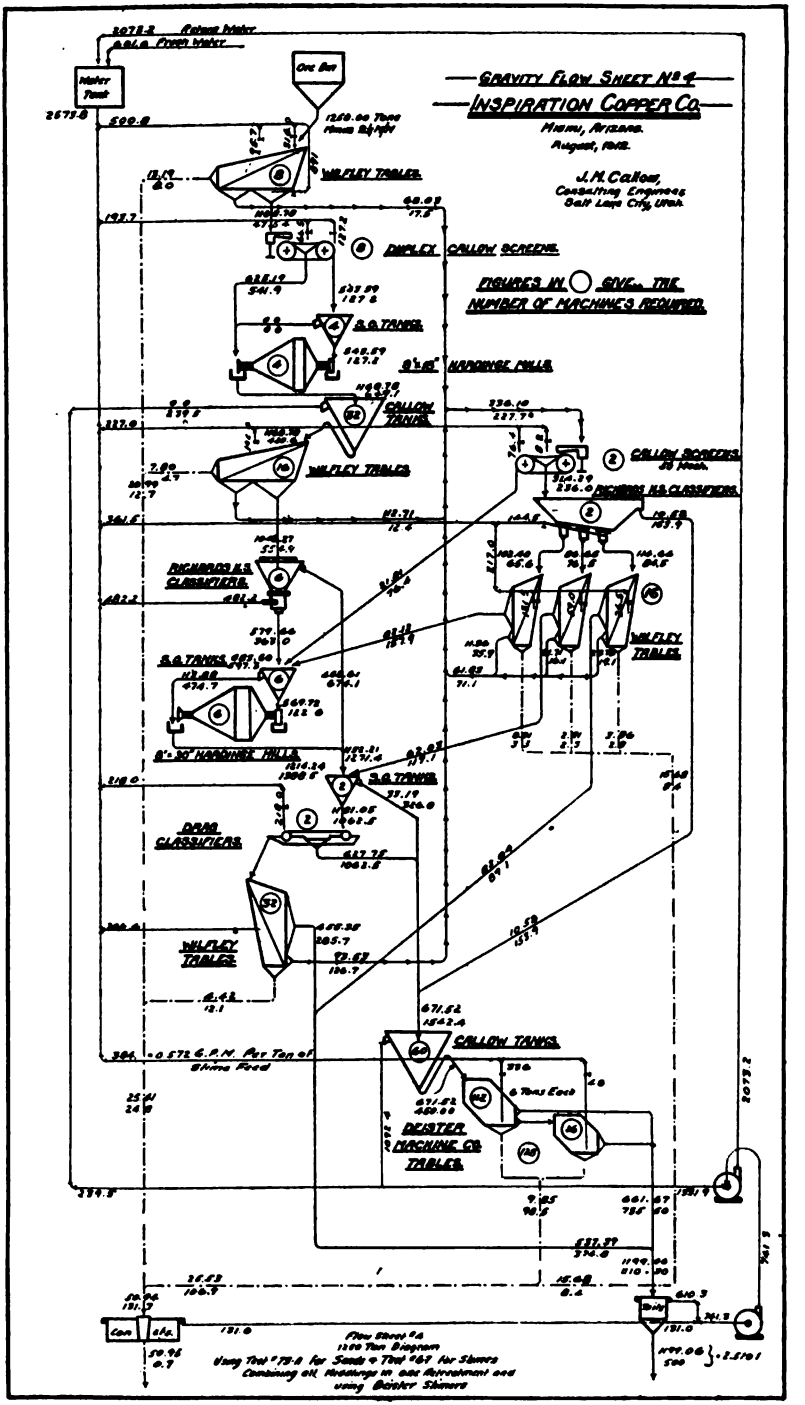

FIG. 1.

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Separation Company men. This, however, was not to be, and neither the ore nor myself ever got farther than New York.

Gravity vs. Flotation Results at Inspiration

It might be interesting here to give some of the results obtained by this gravity flow sheet which the "all-roughing" system carried out to its logical conclusion. The results on three representative samples from the Joe Bush orebody are given in Table 1. The flow sheet eventually developed is given in Fig. 1.

The Wilfley tables employed had gradual terminating riffles, so that wherever clean mineral was available, it could be immediately removed. The middlings only were retabled. They were hydraulic classified, and re-treated on the secondary or cleanup tables. The chief virtue of the scheme was the gradual impoverishment of the slimes owing to the re-

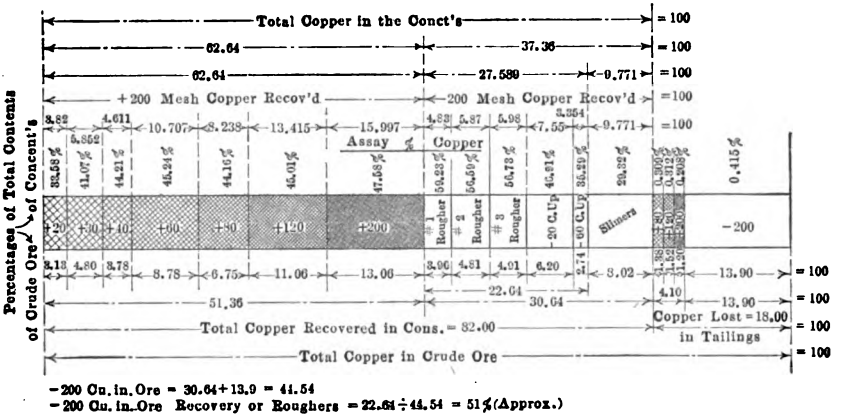

Fig. 2.—Diagram Illustrating the Recoveries and Losses of Copper by Sizes, Showing also the Relative Recovery of the −200-Mesh Values by the Roughing Tables and by the Slime Machines.

peated roughing table treatment. One proof of this impoverishment is that while the slime-table feed of this flow sheet would assay as low as 0.7 per cent. copper, the corresponding feed by an all-classification system (Miami Copper Co.'s, for instance) would assay 1.5 per cent. to 1.7 per cent. copper. This fact is also graphically shown in the diagram, Fig. 2, which shows just where the −200-mesh copper was recovered, and that $^{27}/_{37}$ or nearly 75 per cent. of all this −200-mesh copper was recovered on the roughing tables. There are also other points about the diagram that will be of general interest on close study.

The above constitutes my reply to Mr. Cole's statement on page 631 in his discussion of the "History of the Flotation Process at Inspiration", in which he refers to my results on slime treatment.

A comparison of some of our gravity results with the flotation results in the test plant, and with those obtained on the full tonnage in August,

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1916, are given in Table 2. The comparison speaks well, I think, for the gravity work done, but it should perhaps be pointed out that the gravity results were on Joe Bush ore, which contained more chalcocite, less iron, and less oxide than the present regular mill feed. A composite screen analysis of the tailings from the three selected tests Nos. 68, 71, and 72, and those of the present practice, are compared in Table 3.

The gravity flow sheet called for 6 tons of water per ton of ore in circuit, and the present Inspiration practice takes about 5 tons so that neither the differences of recovery, crushing, or the water required, are so very conspicuous. The more self-evident benefits of the present practice are simplicity, and the much less space required. This latter cannot be better illustrated than by the fact that the present mill building, which was originally designed for 5,000 tons capacity when using the gravity flow sheet, is now treating within the same walls and roof, over 16,000 tons per day.

It will be evident from a study of all the published results, and also from Dr. Gahl's paper and the discussions which have followed it, that the most urgent problem facing the company is the recovery of the oxide copper in the tailings. The recovery of even 50 per cent. of the oxides lost (8 lb. per ton in August, 1916) would represent over $2,500,000 a year on a 15-c. market, and raise the total copper recovery from 75 to 85 per cent. Further on in this article is described the oxide recovery method used by the Magma Copper Co., which should be of interest, and will be a partial answer to Dr. Gahl's remarks under this heading.

TABLE 1.—*Comparison of Gravity with Flotation Results, Inspiration Copper Co.—Gravity Flow Sheet*

	Heads		Tails	Concentrates	Recovery
	Total Cu	Oxide Cu	Total Cu	Total Cu	Total Cu
J. B. Dump test No. 68	1.84	0.10	0.47	36.89	75.91
J. B. R.O.M. test No. 72	1.96	0.10	0.30	38.25	80.95
Oxidized ore test No. 71	1.94	0.27	0.54	39.40	73.20

TABLE 2.—*Flotation Flow Sheet*

	Heads		Tails	Concentrates	Recovery
	Total Cu	Oxide Cu	Total Cu	Total Cu	Total Cu
Callow results test plant, November–December, 1914	1.560	0.300	0.330	30.95	79.49
Inspiration Mill results, 2d half of 1915	1.702	0.226	0.373	32.67	79.95
Inspiration Mill results, August, 1916	1.564	0.392	0.417	30.22	74.36

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TABLE 3.—*Comparison of Crushing, Gravity Experiments, and Present Inspiration Practice*

	Tailings Gravity Tests Nos. 68, 70, 71	Tailings Inspiration, Present Practice
Mesh	Per Cent.	Per Cent.
+ 48	3.00	2.90
+ 65	5.00	7.90
+100	9.00	12.60
+150	9.00	12.60
+200	14.00	5.80
−200	60.00	58.20
	100.00	100.00

DEVELOPMENT OF INSPIRATION FLOTATION MACHINE

It was on the invitation of the Inspiration Copper Co. that I installed the first pneumatic cells to be operated at their test plant in competition with Minerals Separation machines. At this time the tailings were assaying 0.5 to 0.6 per cent. copper, and the concentrates 23 to 25 per cent. copper, and were really inferior to those obtained by gravity. In the course of some 8 or 9 months of continuous competition, my results showed a final tailing of from 0.25 to 0.3 per cent. copper, and a concentrate of 30 per cent. and better. A summary of 2 months' results (November and December, 1914) showed a recovery of 2 lb. of copper per ton in excess of the best results of the other competitors, and a saving in power of some 2½ c. per ton, or all told some 26 c. per ton, which on the present tonnage of 16,000 tons per day, and a 14-c. market, is equivalent to some $1,500,000 per year, which, concisely stated, was my contribution to the history of flotation at Inspiration. A contract in the following January, for the use of my apparatus, was the result.

Concerning the so-called "Inspiration flotation machine" so fully illustrated and described in this paper, the development of which Dr. Gahl has explained, it can be fairly claimed that as all its essential features were anticipated and, moreover, reduced to practice prior to the installation of any flotation at Inspiration, it is a Callow machine in every essential. Neither metallurgically, nor mechanically, has this modification shown any advantage that I know of over my standard type; the blankets require more attention, and more frequent renewals, and on any feed containing a larger percentage of sand than the Inspiration feed now contains, they would be unworkable. The larger unit machine is a positive disadvantage, since when renewing blankets, 400 tons of capacity has to be lost or diverted, whereas on a smaller subdivision of units, as in the Callow cell, only 50 tons or less has to be diverted.

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Callow Cells Adopted by Arizona Copper Co.

Concerning the paper, "The Advent of Flotation in the Clifton-Morenci District," by David Cole, a word in passing. The Arizona Copper Co.'s mill, as the result of the competition referred to, is now operating with a full equipment of 63 Callow cells. It is believed that the tables of results in Mr. Cole's paper are apt to be misleading, though doubtless not so intended, since neither in May nor June was there any direct competition between machines, neither the feed, nor the contributory flow sheet, nor the crushing being the same to both machines. The actual competition commenced on July 10, and ended on July 28, and even then only on the last 10 days were the conditions truly parallel. The decision of the company in favor of the Callow machine was made for the following reasons: That it would make a higher recovery of from 1 to 1.2 lb. of copper per ton; it was simpler to operate, and the operating costs would be lower—two men as against eight; its power costs were lower—3.6 hp. against 5.9 hp. per ton.

In the statement as to the floor space required, it should be pointed out that the vertical mill height required for the C-B machine from the feed inlet to the tailings outlet is 12 ft., for the Callow it would be 6 ft. Machines have recently been designed by which this is easily reduced to 3 ft. Generally speaking, vertical mill height is more at a premium than floor space. However, this is a consideration quite secondary to recoveries, attendance, and power.

Magma Copper Co. Installations

Three different wet milling methods on the ores from one mine is somewhat unique, and it is thought, therefore, that a description of them may be of interest, especially that relating to the flotation of copper oxides.

Description of Ores

The milling ores of this company have kindly been described for me by the manager, W. C. Browning, as follows:

"All ores occur in a porphyry-filled fissure vein. Three classes of milling ores are being mined, as follows:

"*First.*—A copper sulphide ore which contains varying amounts of bornite, chalcopyrite, pyrite, and chalcocite, impregnated or ribboned through a gangue of altered, siliceous porphyry or altered diabase.

"*Second.*—An oxidized ore from the upper levels of the mine, which contains malachite, chrysocolla, and at times a small amount of cuprite, native copper, chalcocite and covellite. This ore is an oxidized product of the sulphide ores.

"*Third.*—A zinc sulphide ore, containing the black form sphalerite, galena, pyrite and small amount of chalcopyrite. This ore occurs in separate shoots from the copper ores. The gangue is usually a very siliceous, altered porphyry. Both copper and zinc ores usually carry an ounce of silver to the per cent. copper or zinc."

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Sulphide Copper Plant Flow Sheet

A flow sheet is given in Fig. 3, which is a combination of hand sorting, gravity, and flotation. The 6 by 4½ Marcy mill, here shown, was the first machine of this type installed commercially. Some early troubles were experienced from overspeed, and too much pulp dilution. Since then it has given excellent service. The pulp density is now 60 per cent. solids, and the revolutions 22 per minute.

As first installed, flotation followed gravity, with Dorr tanks for thickening the feed in the interval. The principal defect of this arrangement was the large amount of pulp storage ahead of flotation, so that adjustment of the oil, when fed in the tube mill, could not be followed in the flotation plant. This was changed and flotation was sandwiched in between the first and second tabling. The lower cells are still retained in operation as "gleaners." These gleaner cells, at the present time, contribute something less than 5 per cent. to the total recovery and take care of any irregularity in the operation of the main plant.

The distribution of labor is as follows:

		Total Men
Ore sorting and coarse crushing	1 shift of 12 men	12
Table floor	3 shifts of 2 men	6
Flotation	3 shifts of 1 man	3
Filter presses and concentrate settling tanks	3 shifts of 2 men	6
Shift bosses	3 shifts of 1 man	3
Ore loaders	1 shift of 4 men	4
Watchmen	2 shifts of 1 man	2
Total operating		36

For repairs, general cleaning up, tailings dam upkeep:

Carpenters	2
Head repairmen	1
Repairmen helpers	5
Roustabouts	16
Sampler	1
Total	61

which equals 3.6 tons per man.

The distribution of power is as follows:

	Hp.	Shifts
Sorting plant	45	1
No. 6 4½ Marcy mill	45	3
5 by 12-in. tube mill	45	3
Concentrating machinery and miscellaneous	80	3
Flotation (15 cells)	80	3
Pumping water	15	3

6,720[1] hp.-hr. per ton

which equals 22.8 kw.-hr. per ton.

[1] The above is "motor" horsepower-hours—not net horsepower-hours.

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The ratio of water to ore in the tailrace is about 7½ to 1.

The mill was designed for 150 tons per day. On occasions it handles 250 tons per day. The original laboratory tests on this ore promised an

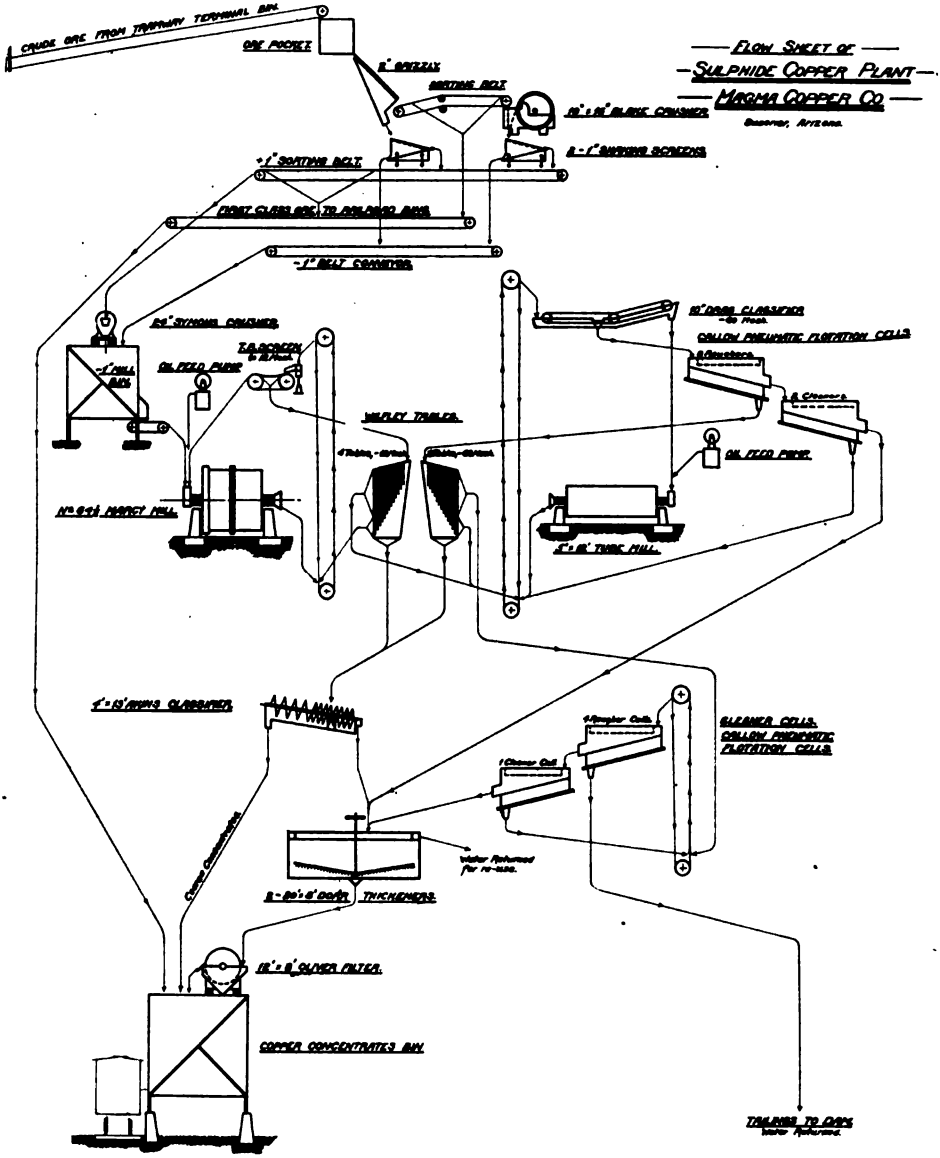

FIG. 3.

86 per cent. recovery on a 5.21 feed to the mill. For actual mill recoveries see Table 4; and for milling costs see Table 5.

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Part of the flotation concentrates are now settled by Dorr continuous thickeners, and part in intermittent decantation tanks.

Magma Zinc Plant Flow Sheet

This flow sheet, given in Fig. 4, is substantially the same as the one at the sulphide copper plant. The main difference is that a drag classifier is used in closed circuit with a Marcy mill instead of a Callow screen. This mill crushes to 48 mesh instead of 10 mesh, as in the sulphide mill. A 6-ft. Hardinge mill, used for the final crush, is loaded with steel pebbles. The final crushing is now to 150 mesh. The plant was designed for 50 tons a day, which it will handle with ease.

For commercial reasons, the plant is now temporarily treating about 75 to 100 tons per day of sulphide copper ore, with results that are fully up to the standard for the sulphide copper plant. The only change necessary was to change the launders to the tables handling the zinc flotation concentrates. With the addition of the two more flotation cells, it is expected to raise the tonnage to 150 tons per day.

This ore was first tested by the General Engineering Co. with a combination of roasting and magnetic separation, gravity, and flotation. This gave excellent results, but the process was complicated and costly. Persistent experimenting with flotation eventually led to the present flow sheet.

TABLE 4.—*Magma Copper Co. Results for Month of October*, 1916

Sulphide Mill

	Average Tons per Day	Assay in Copper		Recoveries, Per Cent.	
		Total	Oxides		
Hand-sorted ore	31.035	15.500	....	38.9	= 92.7 per cent.
Mill feed	187.565	4.042	....		
Mill concentrates	50.670	13.165	....	53.8	= 88 per cent. of mill feed
Mill tails	136.895	0.660	0.11	7.3	= loss
Crude ore	218.600	5.672		100.0	

Month of September

	Average Tons per Day	Assay in Copper		Recoveries, Per Cent.	
		Total	Oxides		
Hand-sorted ore	17.830	18.000	....	28.97	= 93.39 per cent.
Mill feed	188.590	4.173	....		
Mill concentrates	49.563	14.400	....	64.42	= 90.02 per cent. of mill feed
Mill tails	139.027	0.529	0.14	6.61	= loss
Crude ore	206.420	5.672		100.00	

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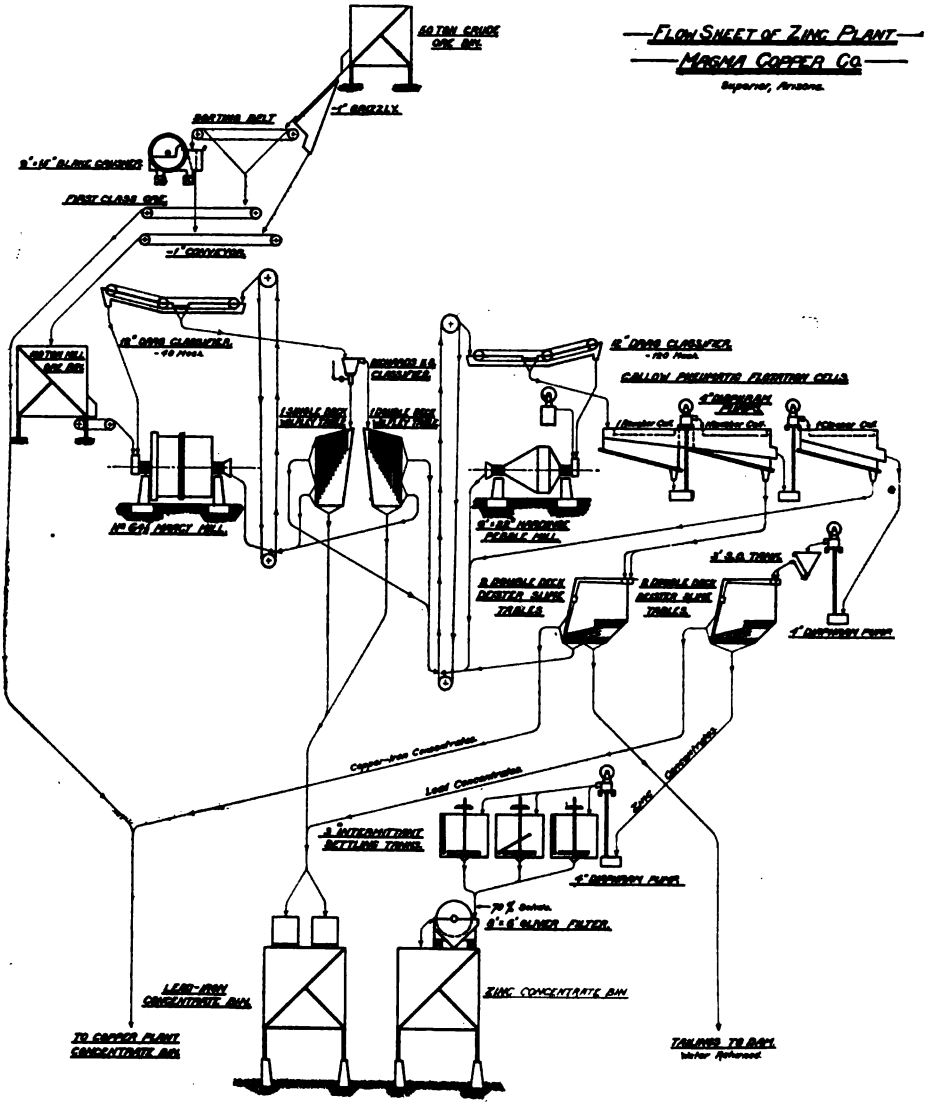

FIG. 4.

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TABLE 5.—*Magma Copper Co., Sulphide Mill, Milling Costs for September and October*, 1916

	Costs per Ton Crude Ore	
	September	October
Ore sorting	$0.2130	$0.230
Coarse crushing	0.0879	0.150
Marcy mill	0.0790	0.087
Tube mill	0.0637	0.086
Callow screen	0.0143	0.010
Tables	0.1840	0.200
Flotation	0.3210	0.345
Filter pressing	0.0746	0.072
Total	$1.04	$1.18

The above costs include all items of direct labor, supplies, maintenance, and power, but no general expense or overhead charges.

It was found that No. 34 Gravity fuel oil in combination with General Naval Stores No. 17 oil had the property of floating the zinc in preference to the iron. These tests have been substantially duplicated in practice.

Some trouble was experienced at first in making both the desired grade of zinc and tailings. Frequent changes in the grade of ore, and the percentage of iron and zinc, owing to unavoidable conditions at the mine, made it difficult at first to get the desired results. It was also found that returned water from the sulphide mill pond contained sufficient coal-tar products to float the iron, thereby reducing the grade of zinc. Trouble was experienced at first with the oils which had lost some of their volatile constituents, due to hot weather. After correcting these conditions, results steadily improved.

The use of a small quantity ($\frac{1}{10}$ lb.) of copper sulphate was decidedly beneficial, reducing the tails from 2 to 3 per cent. zinc without materially affecting the grade of the concentrates. Prior to the use of copper sulphate, acid was tried and, while it lowered the tailing, it also lowered the concentrates. The dilution was 6 to 1, and was an important item in maintaining the grade of concentrates. The cells were sometimes run in parallel, and sometimes in series. No differences in results were noticed.

The mill proper was operated with two men per shift. As it was found necessary to operate the sorting and crushing plants only a few hours per day, no labor was here employed regularly; two men were taken from the crushing department of the sulphide plant for this work. Some iron and copper was hand-sorted at this plant, and, together with the iron concentrates from the −150-mesh tailing tables, was shipped with the regular product of the sulphide plant.

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The intermittent settling system, as indicated in the flow sheet, was decidedly successful, delivering a 70 per cent. solid feed to the filter with a clear overflow from the tank.

A summary of results is given in Table 6, showing the difference in results with and without copper sulphate.

Magma Sulphide Filming Plant

The process in use in this plant is covered by Schwarz' U. S. patent No. 807501, which is the first disclosure of the use of a soluble sulphide for converting an oxide of a metal into a superficial sulphide, and afterward recovering it by a flotation process.

Our earliest experiments were made with H_2S gas as the filming agent. A plant of 25 tons daily capacity was built. In this the gas was applied to the pulp by introducing it into the bottom of an open tank, having a mixing agitator. The results were encouraging, but the consumption of gas prohibitive—as much as 8 or 10 lb. per ton. The ore treated was the tailings of the Magma sulphide mill, which at that time carried considerable oxides. Occasional recoveries of 60 per cent. (of the total copper) were madé, but the results were erratic owing to the difficulty of getting uniform filming of the pulp with this method of applying gas. Then followed an interval of several months when we used sodium sulphide, calcium sulphide, and calcium sulpho-hydrate in an endeavor to avoid the use of gas, on the assumption that it was objectionable owing to the danger of its poisoning the surrounding atmos-

TABLE 6.—*Magma Copper Co. Zinc Plant Results With and Without Copper Sulphate*

Without Copper Sulphate, Oct. 1 to 19, 1916	Zinc	Iron	Lead
Heads	15.38	7.80	3.89
Zinc concentrates	39.55	7.96	8.16
Lead concentrates	12.43	22.60	20.04
Tails	6.50	4.62	0.81

With Copper Sulphate, Oct. 20 to 31, 1916	Zinc	Iron	Lead
Heads	12.50	8.80	3.50
Zinc concentrates	38.50[1]	9.16	6.57
Lead concentrates	15.93	24.01	12.60
Tails	3.23	4.56	0.78

[1] The lower zinc concentrates in the latter period are due to lower heads and not to use of copper sulphate.

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phere. This was true when attempting to use it in an open tank, much of it being lost in the atmosphere. During this time a number of theories were advanced, exploited, and abandoned. One of these was that natural and artificial sulphides could not be floated together, and that H_2S interfered with the flotation of the natural sulphides. Our experience now is that H_2S in the proper quantity really promotes flotation of natural sulphides in company with the filmed oxides, and also that it is immaterial whether the oiling is done before or after filming, and whichever plan is followed is merely a matter of convenience.

In treating Magma sulphide tails, in which the principal losses were sulphides, the introduction of the gas not only filmed the oxides present, a goodly percentage of which was recovered, but it also raised an entirely new crop of refractory sulphides. An all-sulphide sample of regular Magma ore was tested with and without H_2S. The results are given in Table 7.

The commercial plant was shut down and we again reverted to laboratory work, which resulted in our adherence to H_2S in preference to any other agent on this particular ore, and also to a complete change in our method of applying it. The plant was reconstructed, crushing machinery and additional cells added in accordance with the flow sheet shown in Fig. 5, for the purpose of treating Magma oxidized ores on a commercial scale.

The gasing method used at present introduces the gas into the suction of a centrifugal pump in the manner indicated in the flow sheet. This has proved effective, greatly reducing the gas consumption, giving more uniform recoveries, and has removed all danger from the poisoning of the atmosphere; in fact, the commercial results now being obtained date from the first use of this expedient.

TABLE 7.—*Effect of H_2S On Natural Sulphides*

Magma Sulphide Ore

No H_2S Gas	Heads	Tails	Concentrates	Recovery
Test 1	2.45	0.22	11.82	92.70
Test 2	2.45	0.25	9.62	92.20
Average	2.45	0.235	10.72	92.45
With H_2S Gas to Excess				
Test 1	2.45	0.28	9.40	91.30
Test 2	2.45	0.20	8.41	94.10
Average	2.45	0.24	8.90	92.70

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Manufacture of H_2S Gas

The present method of making H_2S gas is to heat sulphur and oil in a retort. Various proportions of sulphur and oil have been tried, our present proportions being 1 of sulphur to 2½ of oil. The temperature in the retort is kept at a uniform 300° C. At times the making of gas

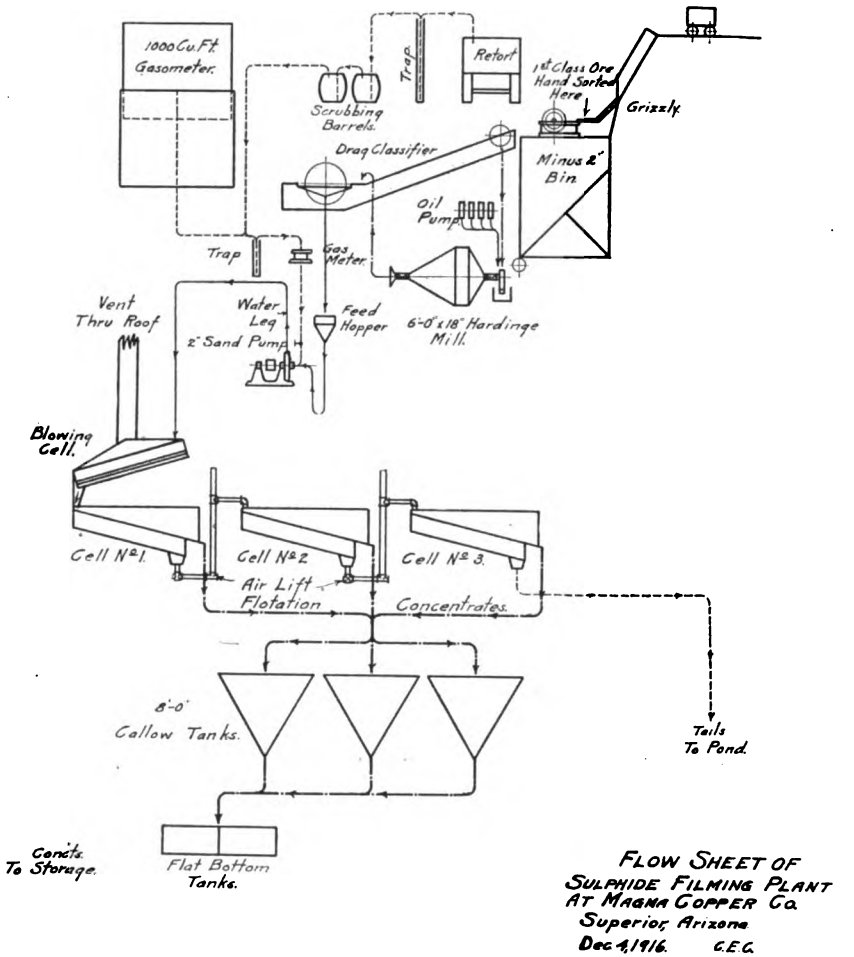

FIG. 5.

has given considerable trouble, owing almost entirely to changes from time to time in the quality of the oil used. Satisfactory results have been obtained at all times with California crude oil, but Texas oil always gives trouble, making a gas containing what we believe to be hydrogen persulphide, which interferes with flotation, and can always be identified by its eye-burning properties. This is in a measure overcome by careful

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scrubbing, sulphur being precipitated in the scrubbers with free H_2S liberated.

The California crude oil has the following fractional analysis:

Specific gravity 0.9311 or 20.36 Bé. at 15° C.
Flash point—open dish—108° C.

Fractionation:

Temperatures	Distillate, C.c.
100° C.–150° C.	1.2
150° C.–175° C.	1.6
175° C.–200° C.	3.6
200° C.–225° C.	9.6
225° C.–250° C.	22.0
250° C.–275° C.	30.8
275° C.–300° C.	25.2

Free gas in a pulp is fatal to flotation, hence the use of the blowing cell at the head of the first flotation cell. Experiments indicate that heating the pulp slightly before gasing is beneficial.

The cost of manufacturing the gas by this method will, of course, vary greatly according to local conditions. Those at Magma are abnormal. Sulphur is costing nearly 3 c. today, and oil nearly 1 c. per pound, f.o.b. Superior, and costs on a basis of 30 tons per day, and maximum of 3 lb. per ton, stand as follows: 90 lb. of sulphur at 2.74 c., $2.51; 225 lb. of oil at 0.914 c., $2.16; total, $4.77 = 15.25 c. per ton, or approximately 5 c. per ton for each pound of sulphur per ton required by the ore.

On Magma sulphide tails, the gas consumption varies from ½ to 1½ lb. sulphur per ton. On strictly carbonate ore, assaying 3 or 4 per cent. copper, 3 lb. is an average figure, and on the latest test with a mixed carbonate and silicate ore, assaying from 4 to 5 per cent. copper, 2 lb. of sulphur per ton. The fuel required for heating the retort is almost negligible. There are no cost figures, since so far we have been burning scrap lumber left over from construction. The labor item is unduly heavy since the retort is situated some 600 ft. away, and one man has to be held in reserve for the purpose. He could as easily make gas on one shift for 500 tons per day. With sulphur and oil at moderate prices on a 500-ton scale, using 2 lb. of sulphur per ton, my estimate of total gas cost is as follows:

1,000 lb. of sulphur at $45.00 per ton = 2.25 c. per pound =	$22.50
2,500 lb. of oil at $1.75 per barrel = 0.436 c. per pound =	11.00
1 man at $4.00 =	4.00
Extra fuel for heating retort and sundry repairs =	2.50
	$40.00 per day

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Thus the probable cost is 8 c. per ton of ore, or 4 c. per ton for each pound of sulphur required for the ore.

Other methods were tried—one using powdered coal in a separate retort instead of the oil mixture, and another in which the oil instead of being mixed with the sulphur was dripped into the sulphur retort with a force feed lubricator. The usual iron matte and sulphuric acid method was also tried but none of these methods had anything to recommend them over the one adopted. Iron matte was never seriously considered on account of the recent high price of acid.

The flow sheet given in Fig. 5 is self-explanatory: The 6-ft. by 16-in. Hardinge ball mill has a capacity of 35 tons per day when loaded with balls requiring 35 hp., or 45 tons per day when loaded up to 50 hp. crushing from crusher run to 83 per cent. −150 mesh.

The power requirements are distributed as follows:

1 6-ft. by 16-in. Hardinge ball mill	35 hp.
1 8 ft. by 14⅞ Root blower, No. 1 (400 cu. ft. at 5 lb.)	20 hp.
1 7 by 14 Dodge crusher (1 shift only)	
1 2-in. centrifugal gasing pump	
1 4-in. diaphragm pump	
1 10-in. belt drag classifier	
1 Oil feeder	
1 Ore feeder	
	55 hp.

This is the equivalent of 37.6 hp.-hr. per ton, = 28 kw.-hr. per ton.

The plant is operated by four men for the three shifts, the extra man on day shift crushing the ore and hand sorting out any first-class ore there may be at the same time.

Magma Results Summarized

Table 8 gives the results of gasing the sulphide mill tails, the feed in all these experiments being the feed to the lower cells or gleaner cells, as they are described on the flow sheet. The soluble copper in these tests varied from 0.3 to 0.45 per cent. and the average for the entire period was 0.35 per cent. Attention is directed to tests Nos. 63A and 67, giving results on this feed without gasing; further comparisons of more direct kind are given in Table 10.

The results on strictly carbonate ore are given in Table 11, and on a mixed carbonate-silicate-sulphide ore in Table 12. The latter tabulation gives a complete record, and shows the contribution that hand sorting makes to the total recovery. The carbonate results, for the most part, were on the rejections from the hand sorting done at the mine, but in all these results the heads, referred to, are actual mill feeds, and the effect of hand sorting is, therefore, not shown. On the mixed ore, gravity tables on the flotation tails would have added considerable to the total recovery but on the straight carbonate ores they would have served no purpose.

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TABLE 8.—*Summary of Results in Treating Sulphide Mill Tails by Filming, Magma Copper Co., Superior, Ariz., 1916*

Test No.	Heads	Tails	Concentrates		Tonnage Oils, Per Cent. Weight							Retort Mixture		Remarks on Gas, Etc.
			Rougher	Cleaner	Recovery[1]	Rate	C.T.	C.T.C.	F.O.	P.T.O.	C.P.O.	S	O	
56	1.10	0.64	8.25	12.10	65.0	.....	60	40	..	..	...	23	77	
57	1.32	0.96	8.60	12.00	31.0	.....	60	40	..	..	...	23	77	
62B	1.15	0.88	10.50	14.00	25.0	.....	60	40	..	..	...	23	77	Excessive heat on old gas charge, test 57.
C	1.15	0.69	10.50	14.00	43.0	.....	60	40	..	..	...	23	77	Moderate heat, 1st firing, 0.77 lb. S per ton.
63A	1.40	1.21	11.90	18.30	15.1	.....	60	40	..	..	...	23	77	No gas used.
B	1.40	0.96	12.90	15.10	34.0	.....	60	40	..	..	...	23	77	Same retort as 62C, 2d firing, 0.36 lb. S per ton.
C	0.74	0.58	5.10	8.40	24.4	.....	60	40	..	..	...	23	77	Same retort, 3d firing, poor gas.
64	1.24	0.69	5.40	9.80	50.9	.....	60	40	..	..	...	23	77	Gas from iron matte.
66	0.91	0.63	7.00	8.20	33.2	29.40	60	40	..	..	...	23	77	Gas from iron matte.
67	0.58	0.47	5.00	.....	21.0	29.40	60	40	..	..	...	23	77	No gas used.
68	0.83	0.49	6.70	12.50	44.2	31.20	60	40	..	..	...	23	77	Slow continuous firing, trying effect of oils.
69	1.16	0.83	12.20	.....	30.9	.....	..	40	60	..	...	26	77	Slow continuous firing, trying effect of oils.
70A	0.94	0.34	7.90	15.10	67.1	28.80	55	36	..	9	...	23	77	Slow continuous firing, trying effect of oils.
B	0.66	0.37	5.00	15.70	58.1	21.60	60	40	..	..	...	23	77	Slow continuous firing, trying effect of oils.
71B	0.91	0.44	7.90	15.70	54.5	34.00	55	36	..	9	...	23	77	Slow continuous firing, trying effect of oils.
72	1.08	0.47	7.20	11.50	60.5	16.50	..	..	..	..	100	23	77	Slow continuous firing, trying effect of oils.
73	0.88	0.44	7.40	17.10	53.3	27.20	55	36	..	9	...	23	77	Slow continuous firing, trying effect of oils.
74	0.86	0.49	7.70	.....	45.8	17.30	60	40	..	..	...	23	77	To try out different oil mixtures.[2]
75A	0.76	0.38	3.80	8.80	55.6	31.20	32	20	..	25	25	23	77	To try out different oil mixtures.
75B	0.76	0.31	3.50	12.30	65.8	31.20	30	45	10	25	...	23	77	To try out different oil mixtures.
75C	0.76	0.38	7.40	.....	52.5	31.20	30	20	25	25	...	23	77	To try out different oil mixtures.
76	0.80	0.47	4.10	17.20	46.6	31.20	30	45	..	25	...	23	77	Trouble with gas.
77	0.69	0.29	3.80	12.00	62.6	24.15	30	45	..	25	...	23	77	Repetition under better conditions.
78[3]	1.00	0.35	4.85	14.80	70.1	29.30	30	45	..	25	...	23	77	Repetition under better conditions.
79	1.23	0.42	7.40	11.25	69.9	30.90	30	45	..	25	...	23	77	Repetition under better conditions.
80	1.01	0.49	4.85	12.90	57.3	27.30	..	..	..	..	...	..	..	But with overheated gas.

[1] Based on rougher concentrates only. [2] For complete analysis of heads and tails see Table 9. [3] Pulp was already oiled and additional oils had very little effect on results.

C.T. = coal tar. C.T.C. = coal-tar creosote. F.O. = fuel oil. P.T.O. = pine-tar oil. C.P.O. = crude pine oil.

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TABLE 9.—*Screen Analysis*

Test 78

	Heads				Tails			
	Per Cent. Weights		Assays, Per Cent. Cu		Per Cent. Weights		Assays, Per Cent. Cu	
Screen Size	Per Cent. Indi.	Total Cu	Oxide Cu	Sulphide Cu	Per Cent. Indi.	Total Cu	Oxide Cu	Sulphide Cu
+ 60	8.3	0.63	0.16	0.47	10.8	0.490	0.14	0.350
+100	25.6	0.77	0.29	0.48	25.6	0.420	0.21	0.210
+200	20.8	0.75	0.35	0.40	24.4	0.350	0.16	0.190
−200	44.8	1.32	0.67	0.65	39.2	0.310	0.17	0.140
Total...	100.0	1.00	0.44	0.56	100.0	0.368	0.17	0.198
Assay...	....	1.00	0.44	....	....	0.350	0.17	0.180

TABLE 10.—*Effect of H_2S on Mixed Sulphides and Oxides in Magma Sulphide Tails*

	Heads	Tails	Concentrates	Recovery
No H_2S	0.86	0.67	9.86	23.70
	0.78	0.60	9.63	24.70
Average	0.82	0.635	9.74	24.20
With H_2S	0.81	0.30	5.05	67.11
	0.78	0.29	5.51	66.38
Average.....	0.79	0.295	5.28	66.24

In all these tests pine-tar oil was used, the tonnage rate was 25 per day and the sulphur consumption 1½ lb. sulphur per ton of feed; the soluble copper was 0.3 per cent. The concentrates "with H_2S" are rougher concentrates. The arrangement of plant did not permit of cleaning the filmed concentrates. The concentrates "no H_2S" were cleaned in the regular way.

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TABLE 11.—*Summary of Results in Treatment of Oxide Ore by Filming at Magma Copper Co., 1916, by Flow Sheet* (Fig. 5)

Carbonate Ores

Date	Test No.	Tonnage Rate	Heads	Tails	Concentrates	Ratio	Recovery
July 8.....	150	35.00	2.78	0.86	9.50	4.50	75.94
July 9.....	151	30.00	3.15	1.06	9.72	4.18	74.31
July 12.....	154	26.00	2.92	1.17	7.02	3.34	72.00
July 13.....	155	30.00	3.24	1.22	11.66	5.16	69.72
July 14.....	156	33.00	2.92	1.35	10.26	5.67	61.94
July 15.....	157	.....	2.92	1.08	8.21	3.89	72.27
July 16.....	158	28.00	2.75	0.83	8.30	4.14	77.25
July 18.....	159	30.00	2.99	0.70	8.32	3.32	83.78
July 20.....	161	30.00	3.08	1.08	9.18	4.05	73.62
July 23.....	164	31.00	3.92	1.62	10.80	3.95	69.76
Average.....	....	30.33	3.06	1.09	9.34	4.22	73.08

NOTE.—Best results in July tests were obtained with a consumption of about 4 lb. sulphur and 10 lb. fuel oil per ton feed.

Date	Test No.	Tonnage Rate	Heads	Tails	Concentrates	Ratio	Recovery
Aug. 9.....	175	25.0	3.04	0.71	14.85	5.41	80.49
Aug. 10.....	176	30.0	3.63	0.81	16.07	5.40	81.82
Aug. 11.....	177	27.0	5.46	1.25	17.36	3.82	83.22
Aug. 12.....	178	25.0	5.46	1.8?	22.74	5.72	72.81
Aug. 13.....	179	27.0	5.12	1.43	16.80	4.17	78.68
Aug. 14.....	180	25.0	5.04	1.85	16.91	4.72	71.09
Aug. 17.....	181	25.0	4.23	1.62	18.48	6.46	67.64
Aug. 18.....	182	25.0	11.66	2.65	26.50	2.65	85.76
Aug. 19.....	183	25.0	6.73	2.67	22.26	4.82	68.62
Aug. 20.....	184	25.0	8.35	2.45	20.67	3.09	80.12
Average.....	....	25.9	5.87	1.72 ?	19.26	4.63	77.025

NOTE.—Sulphur consumption on tests 181, 2, 3, 4 was 6 lb. per ton. In the other August tests, a minimum of 2.25 and maximum of 3 lb. per ton.

Date	Test No.	Tonnage Rate	Heads	Tails	Concentrates	Ratio	Recovery
Sept. 25.....	185	28	4.90	1.33	14.8	3.77	80.0
Sept. 27.....	186	27	5.50	0.79	17.8	3.61	90.0
Sept. 28.....	187	30	5.00	1.50	18.1	4.74	76.4
Sept. 29.....	188	25	3.10	0.61	13.9	5.30	84.7
Sept. 30.....	189	24	2.80	0.54	13.2	5.60	84.1
Oct. 1.....	190	23	2.30	0.58	15.1	8.40	77.8
Oct. 2.....	191	24	2.40	0.68	14.4	8.00	75.0
Oct. 3.....	192	27	2.60	0.41	14.3	6.30	86.0
Oct. 3.....	192	27	2.70	0.36	14.9	6.20	89.2
Oct. 4.....	193	..	3.60	0.61	10.3	3.60	89.6
Average.....	....	26	3.49	0.74	14.68	5.55	83.28

NOTE.—The September and October results were obtained with a sulphur consumption of 2.8 minimum and 3.5 maximum per ton.

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TABLE 12.—*Summary of Results in Treatment of Oxide Ores by Filming at Magma Copper Co. by Flow Sheet* (Fig. 5)

Mixed Carbonate, Silicate and Sulphide Crude ore

Description	Oct. 14 to 27, 1916				Nov. 1 to 10, 1916			
	Tons	Assay, Per Cent. Cu	Contents	Recovery on O.F.	Tons	Assay, Per Cent. Cu	Contents	Recovery on O.F.
Crude ore received for period	173.300	5.61	972.0	100.0	193.10	5.19	1,002	100.0
High-grade sorted out	3.772	22.70	85.6	8.8	10.98	26.35	289	28.9
Mill feed	169.528	5.22	886.4	91.2	182.12	3.91	713	71.1
Mill concentrates	31.300	16.90	592.0	60.9	25.30	18.70	474	47.4
Mill tails	138.228	2.14	294.4	30.3	156.82	1.52	239	23.7
Sum of high-grade and concentrates	35.072	19.30	677.6	69.7	36.28	21.06	763	76.3
Mill ratio	5.4 to 1				5.02 to 1			
Mill recovery	66.5 per cent.				66.5 per cent.			
Sulphur consumption per ton crude	4.66 lb. per ton. High consumption due to				2.06 lb., including all line losses and leaks.			
Oil consumption per ton crude	faulty oil used. Figure therefore of little value.				5.2 lb.			
Flotation oils used	Coal tar..... 30 per cent. Coal-tar creosote..... 30 per cent. G.N.S. No. 17 40 per cent.				Coal tar Coal-tar creosote Pensacola, No. 400.			
Composition of heads	Carbonate..... 1.01 Silicate..... 0.89 Sulphide..... 3.33 Total Cu..... 5.22				Carbonate..... 2.110 Silicate..... 0.865 Sulphide..... 1.490 Total..... 4.460 (3.91)			
Approximate screen analysis of crushing	+150 = 13.6 per cent. −150 = 86.4 per cent.							

NOTE.—No tables were used in this flow sheet. The results would have been improved if there had been.

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TABLE 13.—*Comparison of Laboratory Miniature Tests with Larger Scale Operations, Consolidated Utah-Nevada Corporation*

	Mill Results, Aug. 14, 1916	Miniature Results on Daily Head Sample of Aug. 14, 1916,		5-Ton Laboratory Test of Aug. 14, 1915	Miniature Results Prior to 5-Ton Laboratory Tests
		Salt Lake City Water	Mine Water		
Assay of heads, per cent. Zn	17.20	17.56	17.56	17.54	17.53
Assay of total zinc concentrates, per cent. Zn	41.80	40.48	40.17	42.41	41.91
Assay of total tails, per cent. Zn	5.60	4.89	4.89	3.64	4.71
Ratio of concentration	.....	2.91	2.81	2.87	2.94
Recovery of Zn in Zn concentrates	77.80	78.58	79.07	84.12	81.48
Assay of all table zinc concentrates, per cent. Zn	42.60	40.63	40.63	41.32	41.77
Assay of flotation zinc concentrates, per cent. Zn	37.00	39.80	38.52	46.77	43.19
Assay of zinc in total iron concentrates, per cent. Zn	10.10	14.30	.....	12.88	10.06

While on carbonate ores the results may be considered very satisfactory, there is still considerable work to be done on the mixed ores. Several points are still obscure, which will take time, further experimenting and research to overcome. These are: (*a*) Uniform quality of gas, and what are the interfering elements in poor gas? (*b*) Why are silicates and the coarser sizes of carbonate mineral more difficult to film than fine carbonates?

The present results are encouraging, and positive enough to lead us to believe that in due time all such ores as these will be as successfully treated by this process. As to the treatment of oxidized ores other than copper, so far we have not been able to film zinc carbonates at all but lead carbonates are comparatively easy. We have had no success with gas on these, but sodium sulphide gives excellent results. The following figures are the average of 13 different experimental runs made, treating a lead carbonate tailings dump at the Prince Consolidated, Pioche, Nev.: heads, 6.97 per cent.; tails, 1.85 per cent.; concentrates, 38.81 per cent.; ratio, 7.21 to 1; recovery, 77.28 per cent.

CONSOLIDATED NEVADA-UTAH CORPORATION PLANT

Another illustration of the possibilities of a combination of gravity and flotation processes on a complex mixed lead-zinc-iron sulphide ore is the plant of the Consolidated Nevada-Utah Co., at Pioche, Nev. The following description of the ore, together with much of the following data, has been furnished through the kindness of Mr. Van Wagenen, general superintendent.

"The ore is a typical complex sulphide, consisting of pyrite, galena, sphalerite and chalcopyrite in a quartz matrix. The gangue is, of course, quartz, but the vein carries fragments, large and small, of the quartzite hanging wall, cemented intimately by quartz amidst the ore proper. All the sulphides are distinctly crystalline; they are argentiferous in varying degree, the silver occurring both native and as argentite.

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Secondary sphalerite is present at certain horizons, and the carbonates are in evidence in increasing quantity upward from water level. By weight, the vein as mined and milled is in the neighborhood of 50 per cent. sulphides. Zinc and silver are the principal values."

This ore was first tested on miniature machines, and afterwards in a 5-ton lot by The General Engineering Co., who designed and built the commercial plant. The plant has been in operation since about April 16, 1916. For the 8 months elapsed since the initial start, the mill has run 87 per cent. of the total time. No material changes in the original flow sheet have been necessary, except the addition of one extra table for re-treating the flotation tails, and one extra flotation cell, so as to practice series treatment. Taking into account the complex mixture of the ore, the results, for a mechanical method, may be considered very satisfactory.

The mill water pumped from the mine has a temperature of from 100 to 120° F., and to find out if this was having any detrimental effect on the results, the mill was sampled at the vital points every ½ hr. for a period of 2 days. A sample of the heads so obtained was sent to The General Engineering Co. A summary of the three results is given in Table 13, comparing the mine water and Salt Lake hydrant water tests on miniature machines (1,000-gram charges) with mill results for the period, also comparing the preliminary miniature tests with the 5-ton test sample. The comparison speaks well for the reliability and value of miniature tests. The 6 per cent. difference in recovery might easily be accounted for by a difference in the samples, especially in the proportion of mine to dump ore in the 5-ton test of April 15 and the ore milled in August, 1916.

All the essentials of the plant will be plain from a study of the flow sheet shown in Fig. 6 without further description. The tonnage averages 53 tons per day. The power required for crushing and conveying material into the mill bin is 10 hp. for 8 hr., and for all other purposes 95 hp. for 24 hr., equivalent to 46 hp.-hr. per ton = 34.5 kw.-hr. per ton.

A screen analysis of the final tailings made in July was as follows:

Mesh	Per Cent.
+ 60	2.6
+100	11.3
+150	20.3
+200	8.5
−200	57.3

Commercial results for the months of September and October, 1916, are given in epitomized form in Table 14.

Flotation Oil Mixture

Two standard Callow cells are used as roughers and one standard size for the re-treatment. All froth is cleaned in one half-size cell. The one

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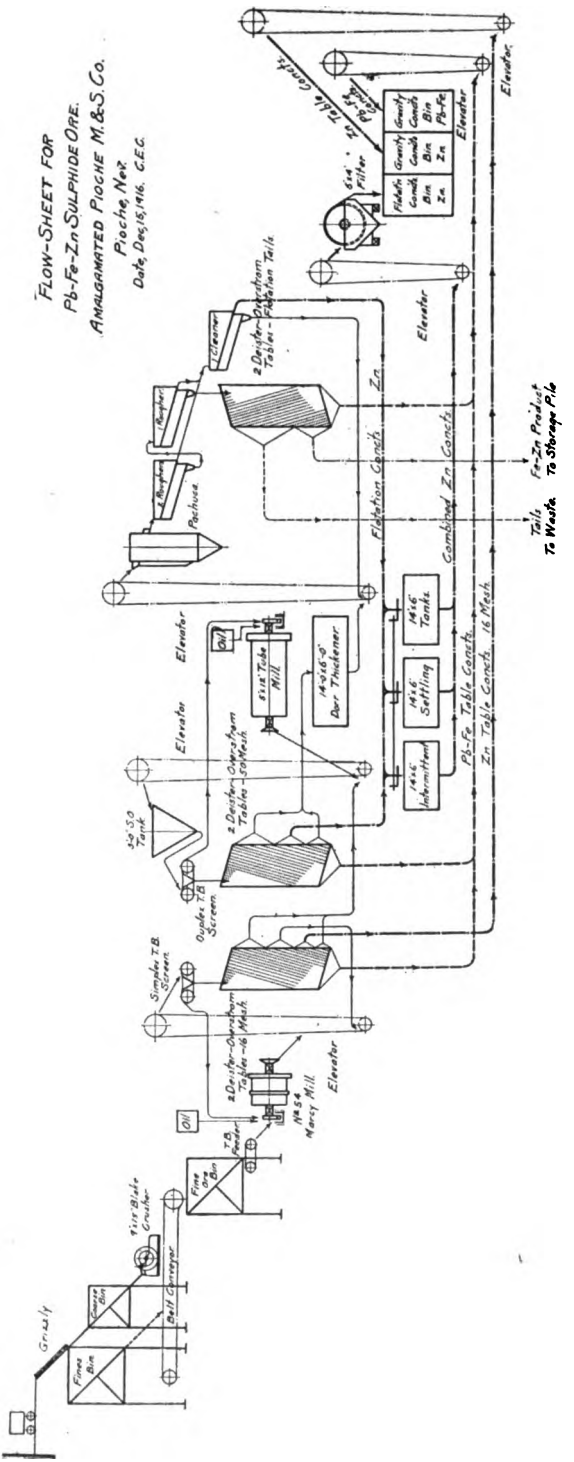

FIG. 6.

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re-treatment cell lowers the tails practically two units. For dropping lead, iron and insoluble, a special oil at one time was used, being made by adding 1 part H_2SO_4 to 2 parts of No. 33 gravity fuel oil. It has been discontinued. There is a tendency for the solutions to foul and kill the froth. At the first appearance of this condition, the clear thickener overflow is turned into the tail race for about 30 min.; this is found to be more satisfactory and cheaper than the use of chemicals. The oil mixture now in use consists of 70 per cent. G.N.S. No. 28 (coal tar), 10 per cent. G.N.S. No. 8, and 20 per cent. Pensacola No. 80. The coal tar (G.N.S. No. 28) is fed to the Marcy mill. Adding tar to the pebble mill was only of slight value while adding it to the Marcy mill lowered tailings about one and one-half units. A mixture of No. 8 and No. 80 is added to the elevator, elevating flotation feed sufficient for frothing, No. 8 alone having been found best added to the rougher tails to give the desired froth on the second series.

The lead concentrate made originally was too "zincy" to market, so that it is now cut in two, making a lead concentrate and an iron concentrate. The former is marketed and the latter is piled up awaiting further treatment.

The lead concentrate assays approximately:

Au	Ag	Pb	Cu	Zn	Fe	Insol.	S
0.42	60.00	14.00	0.50	7.00	33.00	1.40	40.00

TABLE 14.—*Commercial Zinc Results, Consolidated Utah-Nevada Corporation*

September	Tons	Gold	Silver	Lead	Zinc	Iron	Insoluble
Heads	1,567.9	.....	11.5	....	15.90		
Concentrates	385.0	0.135	25.6	1.35	41.66	9.1	15.3
Tails	.....	.....	1.5	....	5.20		
October							
Heads	1,575.60	.....	9.00	....	15.70		
Concentrates	495.00	0.10	19.95	1.34	42.45	9.75	11.5
Tails	.....	.....	1.10	....	5.30		

Results thus far in December are much better than any time heretofore. The month, so far, has averaged close to:

	Tons	Gold	Silver	Lead	Zinc	Iron	Insoluble
Heads	52 to 53 per day	.....	9.0	....	18.0		
Concentrates	15 per day	.....	20.0	....	43.0	7.5	9.0
Tails	.....	.....	1.0	....	4.8		
Float concentrates	.....	.....	.....	....	46 to 47		

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The iron concentrate assays approximately:

Au	Ag	Pb	Cu	Zn	Fe	Insol.	S
0.24	30.00	4.00	0.50	24.00	33.00	2.50	40.00

About 4 per cent. of the flotation feed consists of this latter material. The zinc and iron are in chemical combination. Floating this would mean an unsalable product, while dropping it means high tails.

Handling of Flotation Concentrates

For thickening the flotation concentrates prior to filter-pressing, three 6 by 14 tanks, fitted with Goldfield agitators, are used. Two would have been sufficient for the tonnage. These are run intermittently, the system being to fill, settle, and decant in rotation. This is the same scheme as that used at the zinc plant of the Magma Copper Co., also in a recent installation at the Bingham and New Haven Copper and Gold Mining Co.

The filtering is done on a 4 by 6-ft. Portland filter, the capacity being sufficient so that the filter operates only about one-third of the time. The blow is set so that it operates just above the scraper; an overflow pipe prevents the pulp level from rising too high. A Gould wet vacuum pump is used, giving an average vacuum of 17 in. The filter cake averages a thickness of about ¾ in., moisture seldom being above 8 per cent. Filter concentrates are mixed with table concentrates, which have been drained in the bins, for shipment. Car samples vary from 5 to 9 per cent. in moisture. Mr. Van Wagenen reports that this method of handling flotation concentrates is admirable, and one of the best features of the mill.

When the canvas needs cleaning, a water connection is made to the air pipe, and the canvas is rapidly cleaned simply by brushing.

The following brief summaries of the results from a few other plants, using Callow flotation, may possibly add to the interest of this paper.

Walker Mining Co.'s Plant, Portola, Cal.

The important primary minerals are chalcopyrite and bornite, occurring associated in a heavy diorite gangue, containing much magnetite.

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Because of this excessive amount of magnetite, water concentration is not practical. The ore is exceptionally well adapted to the flotation process and yields a concentrate practically free from magnetite. Covellite occurs in small quantities, but so far no chalcocite has been found. Some of the ore that occurs near the surface has been oxidized to cuprite and much of the chalcopyrite near this zone has been tarnished, giving it the appearance of bornite, though this is only a thin film covering.

The tonnage treated has averaged 75 tons per day, treated on four roughing cells run in parallel. All rougher concentrates are re-treated on one full-size cleaner. The following table gives the results for September and October, 1916:

Heads	3.14 per cent. total copper
Tails	0.52 per cent. total copper
Concentrates	18.10 per cent. total copper
Recovery	86.03 per cent. total copper.

The recovery of the sulphide copper varies from 90 to 93 per cent.

Ratio	6.53 to 1.

The regular oil mixture is coal tar, 63 per cent.; coal-tar creosote, 33 per cent.; Pensacola No. 80 or No. 350, 4 per cent.

Screen Analysis

Mesh	Per Cent.
+ 80	0.0
+100	7.0
+200	20.0
−200	73.0

Needles Mining and Smelting Co.'s Plant, Needles, Cal.

This is a small plant treating the zinc-lead slimes, partly from the wet concentrating plant, and partly from some old slime ponds. Trouble has been experienced from time to time owing to lubricating oils, which were allowed to get into the ponds at the time they were originally impounded. There is no trouble when treating regular mill slimes.

The output is about 40 tons per day, treated in three rougher cells operated in series. All rougher concentrates are re-treated on one half-size cleaner, the cleaner tails being returned to the third rougher cell. The following table gives some random results:

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	Averages for October and November, 1916		Mill Slimes		Pond Slimes	
	Pb	Zn	Pb	Zn	Pb	Zn
Heads	1.80	8.80	1.3	9.5		
Tails	0.90	2.90	0.6	1.7	2.0	3.4
Concentrates	6.50	37.20				
Recovery	....	72.60				
Ratio	5.85 to 1					

The regular oil mixture is coal-tar creosote, 45 per cent.; fuel oil, 25 per cent.; crude pine oil, 30 per cent. Acid is necessary to the extent of about 25 lb. per ton of feed.

McKinley-Darragh-Savage Plant, Cobalt, Ont.

To show the possibilities of flotation on silver ores, a brief description is given of the above plant. Some of the mills in this district, notably the Nipissing, are handling their ores by cyanidation, and in other mills by a combination of concentration and cyanide. Generally speaking, it may be said that flotation will give from 1 to 1½ oz. better tails than cyanidation will, using the same degree of comminution. The principal problem involved in a wider application of flotation to this district is the disposal of the concentrates. The present smelting and freight charges on a 300-oz. concentrate varies from $20 to $25 per ton. A great many different methods have been tried for a local treatment for these concentrates. The most encouraging results have been obtained by a chloridizing roast and using the Holt-Dern roasting process. A movement is now on foot among some of the operators to install an experimental plant of this description. It is confidently expected that this will be the ultimate solution for the local treatment of Cobalt flotation and other concentrates, effecting a saving of $10, and possibly of $15, per ton from their treatment charges over smelting.

The feed to the flotation plant consists of slimes resulting from mining operations and primary crushing, which are separated in the jigging plant, and mill slimes resulting from crushing in stamps and one pebble mill. The mill treats 200 tons per day, approximately 50 per cent. of which reaches and is treated in the flotation plant; 93 per cent. of the feed to the flotation plant is finer than 200 mesh. The chief economic minerals recovered by flotation are argentite, proustite, pyrargyrite and metallic silver.

The plant consists of two triple length Callow roughers, operating in parallel and two standard length Callow cleaners, operating in series, the tailing from cleaner No. 1 being recleaned in cleaner No. 2.

Table 15 gives a summary of typical results.

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TABLE 15.—*Flotation Results at the McKinley-Darragh-Savage Mines, Ltd., Averages by Weeks*

Month	Week	Heads Oz.	Tails Oz.	Concentrates Oz.	Per Cent. Extraction
June	2nd	8.04	2.01	310	75.00
	3rd	8.91	1.75	386	80.30
	4th	7.74	1.64	363	78.80
July	1st	8.65	1.90	334	78.00
	2nd	8.03	2.10	231	73.80
	3rd	5.86	1.40	172	76.10
	4th	6.10	1.41	233	77.00
August	1st	6.20	1.48	309	76.10
	2nd	6.43	1.71	269	73.40
	3rd	5.88	1.66	233	71.70
	4th	6.71	1.53	260	73.20
September	1st	7.68	2.55	349	66.80
	2nd	6.55	2.16	272	67.00
	3rd	5.30	1.35	206	74.50
	4th	6.29	1.91	211	69.60
October	1st	8.05	2.35	238	70.80
	2nd	10.05	3.74	331	62.78
	3rd	8.03	2.60	279	67.62
	4th	8.07	3.04	177	62.30
November	1st	5.64	2.13	309	62.20
	2nd	4.71	1.38	248	70.70
	3rd	4.73	1.53	204	67.60
	4th	4.65	1.70	178	64.00
December	1st	5.36	1.60	220	70.70
	2nd	6.92	1.08	217	84.50
	Average	6.78	1.909	262	71.37

HANDLING FLOTATION CONCENTRATES

Everyone who has used flotation appreciates the difficulties of taking care of the flotation concentrates, and the problems connected with their preparation for the smelters. So far, the area or tank volume necessary for a continuous settling system has never been decided on, from the fact that the area or volume required apparently varies with almost every different concentrate. The requirements for the continuous filter now in everyday use are severe, requiring a pulp of at least 50 per cent. solids and as much thicker than this as is possible. The overflow must be clear, and the accumulation of froth incident to a continuous settling plan must be taken care of. Our experience has not been satisfactory with the continuous plan, and it is for this reason that in all our recent plants we have been installing the intermittent system. Until shown to the contrary, we think that this offers the best solution, in that with it one has complete control of the necessary density for the filters; there is no danger from losses in the overflow; the froth which accumulates during

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the filling of the tank is completely disposed of at each cycle of the operation, and therefore cannot accumulate.

The agitator for stirring the contents of the tank during the discharging period is copied from those used at the Goldfield Consolidated mill.

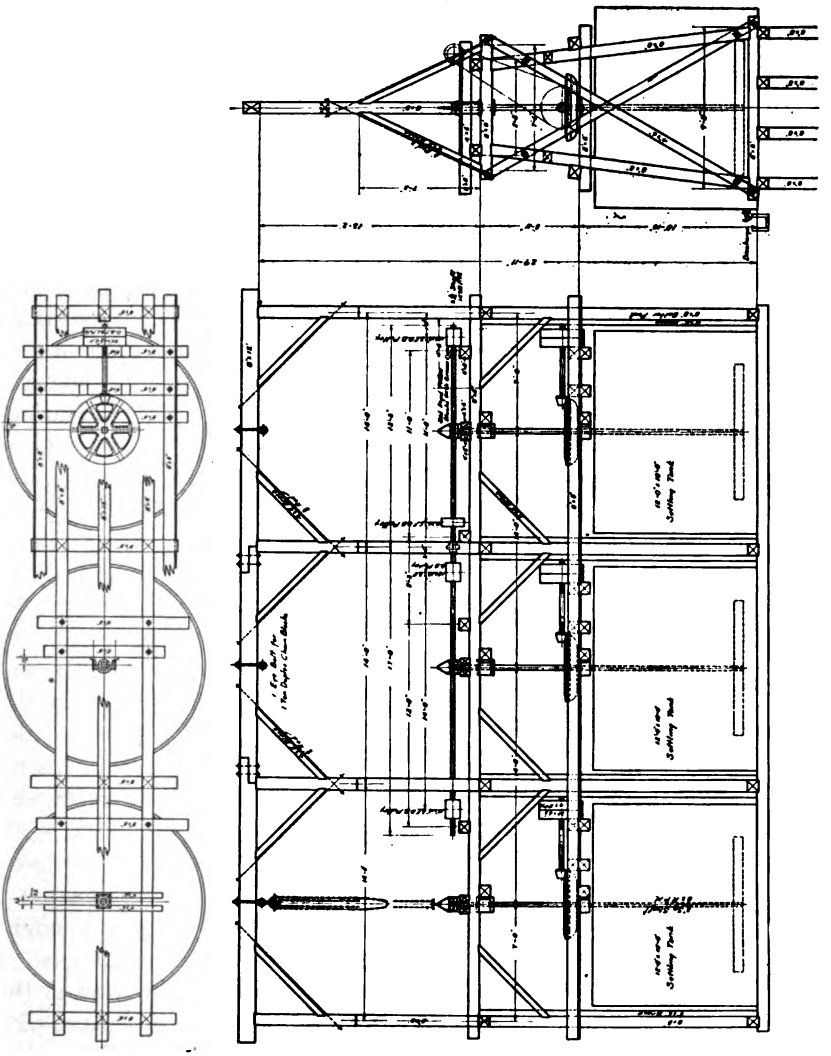

FIG. 7.—AGITATORS FOR 12 × 10-FT. TANKS, 50-TON ZINC PLANT, MAGMA COPPER CO.

As shown in Fig. 7, it consists of arms secured to a square revolving shaft, suspended by a chain block, and passing through a square hole in the driving gear. It is simple, inexpensive, and gives no trouble whatever. The thickened pulp may be drawn off from a central bottom discharge. More recent practice is to draw off through a valve or molasses gate on the side of the tank, or better still, with a diaphragm pump.

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Conclusions from Late Practice

While the fundamental elements employed in flotation are few, yet the practice of the art is not so simple as it looks. It is a process wherein perhaps more than in any other, small causes bring about big results. The number of combinations possible with a list of even one-half dozen oils, and the various acids, alkalies, and salts, the different degrees of dilution and temperature, all of which are dealt with in varying quantities, can best be appreciated by remembering that with four different oils, three oil percentages, two pulp densities and two changes of temperature, the number of possible commutations would be 59,284. This statement, of course, is not to be taken too seriously, or to mean that 60,000 experiments are necessary on any sample of ore, but merely to illustrate by means of an exaggeration that it is evident that flotation experiments take time as well as patience to work out. It cannot be too strongly urged, moreover, that all flotation operations should be in charge of men whose powers of observation have been especially trained, and who not only have had the necessary training with the process in all its various phases, but who also have a practical working knowledge of the machines in operation. I especially emphasize this point because the only disappointments that we have ever suffered in our various installations have been where this advice has not been followed. This is not peculiar to flotation but is an axiom applicable to all technical operations, but one which is too often overlooked to the detriment and loss of all parties concerned.

In conclusion, acknowledgment of obligations is made to Mr. Browning, Mr. Van Wagenen, and the other managers who have so kindly furnished much of the information used herein. Special acknowledgment is made of the help received from J. W. Thompson and of his resourcefulness and ingenuity in developing our present filming system, and also to my other able assistants in other flotation operations.

Discussion

H. A. Megraw, New York, N. Y.—One interesting thing that might be brought out in this connection is the cost of making sulphide gas. I had a communication the other day which suggested that, in view of the high cost of oil, sulphur and sodium sulphide, we might make use of a well-known chemical reaction to form this sulphide gas by the use of aluminum sulphide. It is difficult to see any advantage in this, however, since aluminum has been affected just as much as any other commodity by the rise in prices, due to war conditions.

Rudolf Gahl, Miami, Ariz. (communication to the Secretary*).—Mr. Callow refers to my paper, History of Flotation at Inspiration, which

* Received Feb. 19, 1917.

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was presented at the Arizona meeting of the Institute. These notes of Mr. Callow's contain much valuable information on flotation practice, as is to be expected from an article written by an author of Mr. Callow's experience in this branch of metallurgy. Nevertheless, I cannot help contradicting a number of the statements made in the references to my paper.

In passing, I will also say a word about the reference in Mr. Callow's paper to the article by Messrs. Laist and Wiggin entitled "Flotation Concentration at Anaconda, Mont." Mr. Callow calls attention to the fact that evidently Callow flotation machines are not very sensitive, inasmuch as one man per shift handles four 800-ton sections of Callow machines or 3,200 tons per day at the Inspiration plant. While it has been the experience of the Inspiration company that the four sections of Callow machines in the Inspiration concentrator are easily handled by one American operator per shift, it is only fair to state that each man operating four sections is assisted by two Mexican helpers who constantly clean the bottoms of the machines. The tonnage per section, by the way, whether equipped with Callow, Inspiration or Minerals Separation machines, is considerably in excess of 800 tons. Mr. Callow further says that local opinion at Inspiration is that two 800-ton sections consisting of two 10-compartment and four 6-compartment Minerals Separation machines (these machines are of the Hebbard type) is the limit that one man can properly attend. I do not hesitate to say that I consider it perfectly feasible to have four such sections operated by one man, perhaps assisted by one helper. The Minerals Separation machines of this type have undoubtedly an advantage as far as easiness of operation is concerned, because no porous bottoms have to be taken care of. If Mr. Callow would point out that with neutral pulp the blanket cells require less repair and require fewer shutdowns, I would readily agree with him.

Mr. Callow mentions that when his machines were installed the tailings obtained in the Inspiration test mill contained 0.5 to 0.6 per cent. copper, the concentrates from 23 to 25 per cent. copper and that in the course of some 8 or 9 months of continuous competition his results showed a general tailing of from 0.25 to 0.35 copper and a concentrate of 30 per cent. and better.

I have discussed the difficulties that arose when the attempt was made to install the flotation process at Inspiration on a quasi-working scale, and how they were overcome. Besides the introduction of more suitable machinery, that is, flotation machines utilizing injected air, several other changes had to be made which contributed very heavily toward the improvement obtained.

Mr. Callow further says that a summary of 2 months' results, November and December, 1914, showed a recovery of 2 lb. copper per ton in excess of the best results of the other competitors. It may be fair to

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state here who the other competitors were. They were the Standard Style Minerals Separation machine in November, which was intentionally overloaded to establish the point that a tonnage representing the rated capacity is an overload and a Minerals Separation machine designated as "Subaeration machine" in December which, as I pointed out in my paper, was soon abandoned and replaced by a simpler and better type of machine. No differences of the magnitude quoted by Mr. Callow resulted when improved machines had taken the place of the two Minerals Separation machines just mentioned, either one way or the other. In the later contests the differences in the results obtained by machines of different designs were very small indeed. I will not in any way deny that when Mr. Callow, upon the invitation of the Inspiration company installed a number of his cells in the Inspiration test plant and showed that they effected a much better recovery than the Standard Minerals Separation machine, he rendered a very important service which in my opinion certainly justified the company in buying the rights for the use of his apparatus for the treatment of a limited tonnage of ore.

Mr. Callow claims that the blankets of the Inspiration flotation machine require more attention and more frequent renewals than the blankets of the Callow machines. In reply I beg to state that the same number of men are employed per Callow section as per Inspiration section for the purpose of keeping the blankets in good condition and that there are no figures showing that there is any difference in favor of the Callow machine as far as renewals are concerned as long as the comparison is made under the same conditions. I wish to point out that the conditions under which the Callow blankets in the Callow sections at Inspiration work are somewhat different from those met in the Inspiration machines. In the Inspiration sections the whole feed passes through one machine, sand and slime mixed. In the Callow sections there are primary cells receiving the regular mill feed and secondary cells treating slime only. Only the primary cells of the Callow sections treat the same feed as the Inspiration sections. In the experience of the company there is no difference in the wearing of the blankets in the primary Callow machines as compared to the blankets in the Inspiration machines. The blankets in the secondary Callow machines treating only slime feed, as mentioned before, wear somewhat better. The item of the blanket upkeep, is however, so small in both types of machines that it deserves no serious consideration.

Another statement of Mr. Callow's is that with a feed containing a larger percentage of sand than the Inspiration feed now contains, Inspiration machines would be unworkable. I beg to reply that very often in daily operation the Inspiration machines have received pulp containing more oversize than they had originally been designed for. Frequently the average of the whole mill has been 4 per cent. on a 48-mesh,

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which means that some of the sections carried 5 and 6 per cent. on an average of a whole day, and a great deal more for shorter periods. I want to say, furthermore, that re-treatment of table middlings in flotation machines of the Inspiration type has been adopted, that is, of a feed containing sand alone and no slime, or at least very little of it, and that no difficulties whatever are found in treating such a feed. We have, furthermore, considered the re-treatment of all of our table tails, that is a product which is practically all sand, on flotation machines of the Inspiration type and have, in order to satisfy ourselves that we would not find mechanical difficulties in treating a product of this nature, made tests regarding this point. We have found none.

Another of Mr. Callow's statements is that the larger unit machine, that is, the Inspiration machine, is a positive disadvantage, since when removing blankets 400 tons of capacity has to be lost or diverted, whereas with a small subdivision of units as in the Callow cells only 50 tons or less has to be diverted. I wish to reply to Mr. Callow that when Inspiration machines were installed, care was taken of this point and an air box designed which could be removed from the flotation machine while the same was in operation, so that Mr. Callow's statement that 400 tons' capacity has to be lost or diverted is incorrect. As I pointed out in my paper, however, we soon found that our flotation machines stand overloading very well and that it was not a great detriment to temporarily switch the load from one flotation machine to another. Since that time we have made it a practice to divert the feed when repairs are made. It is a little easier to make repairs on an empty machine than it is with a machine running. I want to assure Mr. Callow, however, that whenever we want to we can make arrangements to change the flotation blankets without diverting any feed, and I am inclined to think, therefore, that the disadvantage which Mr. Callow attributes to the larger machine does not exist, but that the machine presents the advantage of not requiring any diversion of feed, should any one using it desire to change bottoms in this manner. It has the additional advantage of having a solid bottom, thereby obviating leaks, and of not requiring calking when changing air chambers. Metallurgically speaking, our experience is that the Inspiration machines work just as well as the Callow machines, if not better. I consider it a decided constructional advantage that it requires materially less mill height than any other air-bottom flotation machine that I know of, for instance, the Callow machine. Mr. Callow admits the advantage of reduced mill height in his reply to Mr. Cole contained in the article under discussion, although he modifies his statement by maintaining that this consideration is quite secondary to recovery attendance and power.

To Mr. Callow's further assertion that "inasmuch as all the essential points of the Inspiration flotation machine were anticipated and, more-

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over, reduced to practice, prior to the installation of any flotation at Inspiration, it was a Callow machine in every essential," I want to reply that to decide this question it is necessary to establish the characteristics of the Callow machine. If the Callow machine were the original pneumatic flotation machine, I would admit the validity of this argument, but as far as I know it is not. As I pointed out in my paper, the application of a porous bottom for flotation work had been patented in England to Minerals Separation Ltd. much earlier, and the use of this principle could not, therefore, constitute a peculiar characteristic of the Callow machine. The Callow machines that were demonstrated to the Inspiration Consolidated Copper Co., in addition to the characteristic of a porous bottom, contained the further characteristic that the bottom was inclined. This naturally cannot be considered as a discovery, as all the surfaces over which ore passes in any concentrating mill are inclined. Furthermore, the cells contained a float valve each for the regulation of the pulp level in the cells. I presume that neither this nor the combination of these two characteristics will be considered a discovery by Mr. Callow. From my viewpoint, Mr. Callow, contemporaneously with some others, rendered a very important service to American metallurgy, the importance of which cannot easily be exaggerated, when they showed that the Minerals Separation Co. had neglected the development of one type of their machines at the expense of another. To call the type of a flotation machine which they originated, but neglected to put into practical operation in America and even to patent in America, a Callow machine is, I believe, not justified.

Concerning Mr. Callow's priority statement, it is my impression that when the first flotation machine was installed at Inspiration or the first pneumatic flotation machine was patented, there was no such a thing as a Callow cell.

The Inspiration flotation machine makes use of the principle that ore pulp somewhat of the character of the Inspiration pulp, that is pulp containing some oversize on 48-mesh, can be passed over a horizontal or nearly horizontal bottom by proper arrangements of baffles. This may not be much of a discovery, still it seems to me that it contains the elements of invention to a certain extent, inasmuch as everybody at the first glance is of the opinion that baffles would have the effect of retarding the flow of the pulp. This is the only point that is claimed as new in the construction of the machine outside of the air chamber, which I think has points of originality and the advantage that it can be removed without diverting the flow of feed through the machine. I do not hesitate to say that the machine utilizes the principle patented in England to Hoover and the Minerals Separation Co. As far as I can see, it does not embody any invention of Mr. Callow's. We use, however, the same kind of a porous medium, that is, canvas.

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Mr. Callow also calls attention to the use of hydrogen sulphide to assist in the recovery of copper carbonates and silicates as practiced by the Magma Copper Co. and offers this process as a partial answer to the question of treating such ores, which I discussed in my paper. Mr. Callow deserves great credit for describing this application of the flotation process which, as his figures show, gives encouraging results. As he knows himself, it is only a partial answer and the Inspiration company has been fortunate enough to receive several partial answers lately. The company has before it the dilemma, however, of deciding which partial answer is the best, and is holding out in the meantime for a final answer.

G. D. VAN ARSDALE, New York, N. Y.—If we divide the practical operation of flotation into three steps:

1. Oil mixing,
2. Aeration,
3. Separation of froth and pulp,

then ideally the machine arrangement should be such that in step 1 no particle of mineral can escape coming into contact with oil, and in step 2 that each particle of mineral, presumably coated with oil, shall have sufficient opportunity to come into contact with an air bubble. Recoveries will accordingly be lowered by a lessening of efficiency of either of these operations, and costs will be raised if the apparatus used for either purpose is less efficient mechanically than it should be.

It seems sufficiently obvious, as stated by Mr. Callow, that blowing finely divided air through a pulp is mechanically a much better way of mixing air and pulp than agitation with a paddle. It would not seem impossible, however, to devise a machine in which the same contact of finely divided air and pulp could be effected without the necessity of blowing the air through such a depth of pulp, which of course means power consumption.

Mr. Callow says, in comparing the so-called "Inspiration machine" with his: "The larger unit machine is a positive disadvantage since when renewing blankets 400 tons has to be lost or diverted, whereas on a smaller subdivision of units only 50 tons or less has to be diverted." This disadvantage does not seem so obvious to me.

If the diversion or loss is expressed in terms of cost per ton treated, and if we have two machines in parallel, one of 50 tons and the other of 400 tons capacity, and if we assume that the blankets on both are renewed at the same time, then it is true that during the time of shutdown eight times more pulp has to be diverted from one machine than from the other but during the operation period the larger machine will have treated eight times as much pulp. Therefore, the loss, if expressed per ton treated, will be exactly the same in both cases. In other words, there

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will be a disadvantage of the larger machine only if the tonnage cost per blanket renewal is greater for it than for the smaller machine.

If the cost of renewal of a blanket in the larger unit is eight times the cost of renewal in the smaller, then the disadvantage will be proportional to the greater frequency of renewal, but if the cost of renewal is less than eight times the larger blanket can still be renewed more frequently at an equal or even a less cost per ton.

Mr. Callow's note on the advantage from the use of copper sulphate is important. It has been known that very minute amounts of certain addition agents may exercise either a deleterious or favorable action on flotation. Practically it has been found that a substance whose action is very favorable on the flotation of some ores may exactly reverse this on others. For example, sodium sulphide is in use in some mills and has the result of increasing the recovery. The advantage from its use seemed so obvious that a quantity was ordered for use in a southwestern mill. Very unexpectedly it was found that the addition of even very small amounts to the flotation feed at this mill had the effect of producing a white froth carrying no mineral whatever. This result is the more curious because at another mill it has been found that considerable benefit is derived from the use of small amounts of caustic soda.

I believe therefore that we must divide ores for flotation into three classes:

1. Those requiring acidity of pulp.
2. Those requiring alkalinity of pulp.
3. Those requiring neutrality of pulp.

There seems no doubt that this varying action is to be referred mainly to the influence of the constituents of the gangue.

We have been investigating this question for some time, using for the purpose artificial ores made up of mixtures of as nearly pure minerals as possible, with various gangue minerals. When this work is finished and is supplemented by tests on actual ores of varying gangue constituents we can then correlate extractions with gangue constituents and possibly find a way to eliminate the influence of some of those that are found injurious. Definite results have already been obtained but are not as yet sufficiently complete for publication.

This brings up another very important practical point, which so far as I know has not been thoroughly investigated, namely, the composition of mill waters and the effect of dissolved salts on flotation.

In cyanide mill practice we find in many cases greatly reduced efficiency from "foul" solutions due to the gradual accumulation of impurities by solution from the ores, and there seems no reason why the same thing may not occur in flotation practice. In our own work we have had unexplained differences between the results of test mill and

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operating mill practice, and an investigation is in progress to determine the possibility of this being the correct solution.

Mr. Callow's description of the Magma sulphidization work is very interesting. We have also found in our work that H_2S does not interfere with flotation except when it is present as free H_2S.

We have a simple way of eliminating this, and our flow sheet is different in some details from Mr. Callow's. We have obtained very good extractions on partly oxidized copper slime tailings but find it impossible to make a high-grade concentrate in one operation although we can do so by a recleaning cell.

Our method of generating H_2S contemplates the use of low-grade copper matte and acid, which for our conditions I believe will be cheaper than the Magma method.

Metallurgically, sulphidization by H_2S and flotation seems entirely practical, and apparently its use will be decided on the question of the advisability of working with large amounts of a very poisonous gas, together with the relative advantages and disadvantages of other alternative methods for partly oxidized ores.

To my mind the most important conclusion in Mr. Callow's paper is that pointed out on the last page. Flotation is the resultant of an extremely complicated set of conditions, all of which are important, and while gravity concentration may in many cases be safely left to the "millman," it will in most cases pay very well to have the flotation end of a mill under the direct control of a competent flotation metallurgist, whose time will be fully occupied.

Finally, the profession is always indebted to the man who will take time to write a summary and review like the paper under discussion, and I think there is no doubt that flotation is still in such an unsettled state that it will be a wise prophet who can accurately forecast from our present knowledge either the final forms of method or apparatus, or even the final field or extent of its use.

E. R. Ramsey, Denver, Colo. (written discussion).—Under the heading of "Handling Flotation Concentrates," Mr. Callow says that his experience has not been satisfactory with the continuous method of settling or dewatering the concentrates before filtration, and that he has for that reason adopted the intermittent system in his more recent installations.

The continuous system, to which reference is made, and which is in use in practically all of the large flotation plants, involves the use of the Dorr continuous thickener. The flotation froth with sufficient water to carry it through the launders is fed to the thickener in the usual way and the thickened concentrate drawn off at the bottom continuously into the filter, which is in most cases the revolving drum type.

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Flotation concentrates are very difficult to settle, particularly copper concentrates, and require a very large settling area compared with that of the gangue material from which they have been separated. For copper sulphide concentrates the average of a number of plants would show about 40 sq. ft. of settling area required per ton of concentrate per 24 hr. In some instances this is considerably less and in others it will run over 50 sq. ft. For zinc concentrates this area is considerably less and will average in the neighborhood of 12 to 15 sq. ft. per ton. The character of the original ore itself, particularly of the colloids, as well as the quality and quantity of oils used, seems to have an important effect on the settlement of the concentrate, and it is usually found that the froth collecting on top of the thickener tank will carry an appreciably greater quantity of gangue than the average concentrate.

At a few plants some difficulty has been encountered in the handling of the froth which accumulates on the top of the thickeners. However, I do not feel that this difficulty can be attributed to the continuous system itself, but rather that it is due to crowding the thickener capacity. In every case that has come under my observation, where the thickener was being fed beyond its capacity with the resulting dirty overflow, difficulty was encountered with the froth building up and tending to work over the sides of the tank and into the overflow launder. As against this, where the tank is being fed at or below capacity and a clear overflow is obtained, very little, if any, difficulty is experienced with the froth, as it usually builds up to a certain extent, dries out on top and apparently breaks down from underneath as fast as it accumulates. An explanation for the froth accumulation when a dirty overflow was being obtained, would be that the fine particles of concentrate rising to the top of the tank attach themselves to the bottom of the froth layer and gradually build up faster than they disintegrate.

At the Magma plant, to which reference is made, the Dorr thickeners were operated in series, which arrangement experience at other plants has conclusively shown is inefficient compared with parallel settling. A comparison of this method of thickener operation with the intermittent system as later installed, shows that the latter had a definite advantage in settling area, and consequently, capacity.

At some other plants the original thickener installation was insufficient for the tonnage of concentrate to be handled, but in a majority of cases the tonnage of flotation concentrate has been increased beyond the capacity of the tank without providing additional settling area. The tray thickener, as developed by the Dorr company in the past year, offers a most satisfactory means of obtaining a large settling area with a small amount of floor space. The tray is of proven merit in settling slime, giving the full 100 per cent. increase in capacity where properly installed, and makes a flexible and compact installation.

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The first installation of a tray for handling flotation concentrate was recently made at the plant of the Old Dominion Copper Mining & Smelting Co., and the latest advices from this plant indicate that the thickener and tray are working at full capacity without any indications of overload. This installation consists of a 40-ft. tank with a single tray of the connected type. Settling tests indicated an area of 40 sq. ft. per ton and a discharge product of 57 per cent. solids. Present feed consists of about 608 tons per day at 8 per cent. solids. Approximately 63 per cent. solids is being obtained in the discharge. The overflows from both tank and tray are clear, and by the use of paddle arms and sprays on the surface of the tray compartment no trouble is experienced with froth accumulation. This method of handling the froth is said to be very effective.

A number of schemes for handling the flotation froth are being tried which promise satisfactory results. With the froth taken care of, the continuous system should offer the most satisfactory means for thickening concentrates in that the installation can be made with the minimum of floor space and head room, particularly where the tray is used; would require less attention than the intermittent system; less horsepower, although this is not of great importance; and would give as thick, if not thicker, discharge, with reasonable attention given to its regulation.

There has been considerable discussion as to whether the continuous settler would give as thick an average product as could be obtained from the intermittent tank. The Dorr company in the past year has made experiments on a large number of different slimes and concentrates, which show that stirring near the end of the thickening period will give a thicker final product than can be obtained with undisturbed settling. No samples have failed to give this result and in some cases the difference is very marked. The same result has been shown in actual operation. In practice the Dorr thickener provides this stirring with the plows, in raking the thickened material to the central discharge, and should, therefore, with a proper regulation of the discharged product, give a greater density than in intermittent settlement.

At the plant of the Timber Butte Milling Co., the zinc flotation concentrate is settled in Dorr thickeners to approximately 70 per cent. solids, and in the same district the Butte & Superior and the Anaconda Copper Mining Co. are obtaining from 60 to 65 per cent. solids in their underflows on zinc concentrate. On copper flotation concentrates the density of discharge will average somewhat less, the Inspiration and Anaconda thickener plants giving around 60 per cent. solids. At the Arthur plant of the Utah Copper Co. one 75-ft. thickener has a capacity of over 200 tons of flotation concentrate per day, giving a clear overflow and a thickened product averaging about 60 per cent. solids. The concentrate thickener discharges at Chino and Ray also average about 60 per cent.

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solids. The area required for lead flotation concentrate is about the same as for zinc, namely, 12 to 15 sq. ft. per ton. The St. Joseph Lead Co. at three of its plants in Missouri obtains about 65 per cent. solids in the discharge from the thickeners on lead concentrate.

On flotation concentrate where a large settling area is required, as a rule very little depth is necessary for thickening, so that a number of trays could be installed in one tank without an excessive depth. In view of the marked economy which can be made in initial and operating costs and in space required, it is probable that future installations will be made along this line.

E. E. FREE, Baltimore, Md.—I am not competent to discuss at all the operating details in Mr. Callow's paper or indeed anything that has to do with the practical use of the flotation process, since I have had no experience whatever with that end of the matter. The thing which I do have to say I say very diffidently and tentatively. It has to do entirely with the theory of the flotation process. So far as it relates to Mr. Callow's paper at all, it is to the incidental observation of the assistance sometimes obtained by the use of copper sulphate in the solution.

In order to explain what I am about, let me assume that the essential thing in flotation is the attachment between the mineral and the oil. Other things of course are necessary—the froth formation, etc.—but the essential thing about the flotation process is that a given oil will attach to certain minerals in the ore mixture and not to the other minerals. This is commonly ascribed to what is called "interfacial tension," which means nothing whatever. All that we really mean is that the oil adheres to some minerals and does not adhere to other minerals.

Now there is a whole series of experimental results which are closely related to this sort of thing, namely, the experiments on what is called adsorption. At the present time there are two theories as to the cause of adsorption. First, there is a physical theory which imagines the adsorption as the same sort of condensation as would be produced by increase of pressure on a gas. For instance, if one imagines a gas condensed on the surfaces of charcoal particles by extreme pressure, one has the action of the charcoal in adsorbing the gas, which is actually observed. The second theory ascribes adsorption to a quasi-chemical union between the adsorbed substance and the material that is adsorbing it.

The adhesion which is the essence of flotation is closely related to adsorption and probably due to the same causes,[1] but that does not get us very far practically, and the thing I particularly want to speak of is not the adherence between the oil and the mineral but the effect which

[1] See Bancroft: *Metallurgical and Chemical Engineering*, vol. 14, pp. 631–635 (June 1, 1916).

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might be produced on this adherence by the previous adsorption on the mineral surfaces of films of some other substance. All mill waters carry small amounts of dissolved substances. The particles of ore and gangue are ground in, or mixed with, this water before coming into contact with the flotation oil. Undoubtedly they adsorb out of this mill water, dissolved substances which are then held in the form of adsorbed films on the surfaces of the solid particles. They might, for instance, acquire adsorbed films of sodium chloride from the mill water.

It is probable that this previous adsorption of dissolved substances on the surfaces of mineral particles will have important effects on the tendency to adherence between the mineral and the oil and therefore on the possibility of flotation. The adsorbed films may have detrimental effects, or may have beneficial effects, depending upon whether the different minerals are made more alike or more different in their tendency to adhere to the oil.

It is known that these adsorbed films are affected importantly by many different things, especially by the presence in the solution of small quantities of other dissolved substances, and it seems to me that in this fact there lies a possible explanation of the effects of copper sulphate and similar substances. If copper sulphate in the mill water has an effect in displacing or modifying in some way the films of adsorbed substances formed on the particles of ore or gangue, then the copper sulphate may affect importantly the subsequent tendency of the ore or gangue to adhere to the flotation oil. There are similar implications in connection with the small chemical differences between different flotation oils which differences seem to have such great practical effects. Perhaps the effects of these small differences may depend upon an action of some sort on the previously formed adsorbed films.

Essentially, all that I have to suggest is that the adsorption of substances on the particles of the ore or gangue prior to the application of the flotation oil may have much importance to the working of the process, perhaps even more importance than the properties of the mineral and the oil themselves. It seems not improbable that some of the surprising anomalies which, I understand, are encountered in working the flotation process may be related to something of this kind.

J. W. Bell, Montreal, Que.—I have been very much interested in the remarks of the last speaker, and would like to ask him if he thinks that adsorption may have an important bearing on the fact that at times the use of an acid seems essential to secure good flotation, or in fact to get any flotation. Of a number of ores on which we have made laboratory experiments, in every case the use of sulphuric acid was necessary either to get any froth or to get a clean froth. The information we have just received suggests the possibility that when the acid is added to an otherwise non- or difficultly-floatable ore, the adsorbed material may be dissolved and thus permit the adherence of the oil to the mineral particles.

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E. E. FREE.—It is very difficult to give a categorical answer to this question. There are no generalizations with regard to adsorption which I regard as sufficiently well established to be willing to state them. So far as one can be sure at present, every case of adsorption must be considered individually. However, it appears to be true that the concentration of the hydrogen ion usually has an important effect upon all of the adsorption phenomena occurring in the solution. Accordingly, one would expect that the presence of sulphuric acid would disturb importantly the adsorptive behavior of all substances present. I do not believe that it is possible to predict in advance whether addition of sulphuric acid would be beneficial or detrimental in any special case.

Incidentally, I do not believe that it is quite correct to speak of sulphuric acid as "dissolving" an adsorbed film. Whatever adsorption may be it is not the same thing as, for instance, the coating of a particle of oxide with a sulphide film. Such coating probably involves a chemical change which penetrates a distance of some molecules into the particle. If that film is removed by acid an appreciable portion of the particle is removed. The ordinary case of adsorption does not seem to be analogous to this and it is better, it seems to me, to speak of sulphuric acid or other substances as "displacing" an adsorption film rather than as "dissolving" it. According to Langmuir's theory[2] the adsorption film consists of a layer one molecule thick of one substance held on another substance by a quasi-chemical attraction. A third substance, for instance sulphuric acid, might displace this film and replace it with a layer of hydrogen ions or of sulphuric acid molecules or of SO_3 ions or of something else. Of course, all of this is very tentative and theoretical. As I said in the beginning, I do not think it possible to make any generalizations from which one could predict, without experiment, the behavior of any particular case.

H. W. DUBOIS, Philadelphia, Pa.—I have been at several of these flotation law suits and I think it is one of the most unreasonable things to ask judges, even the most expert in the interpretation of patent law, to pass intelligently upon such subjects without a technical advisor, such as is provided in English Courts. The subject is so highly technical, that the mere use of obscure terminology is used with evident success in clouding the subject in such a way that I do not see how it is possible for us to look to our Courts, constituted as they are, for a really fair interpretation of the patent situation. The legal situation is quite similar to trying to play a game of baseball with an umpire who had never seen the game. You might instruct a man effectively in baseball-umpire law during the progress of a game but I doubt if the ordinary baseball

[2] Irving Langmuir: *Journal of the American Chemical Society*, vol. 38, pp. 2221-2295 (November, 1916).

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fan would like to be compelled to accept his decisions; I doubt if it is possible for the judges of even our highest courts to sufficiently inform themselves upon such a complicated subject, so that they can intelligently pass upon it.

There is one point I would like information upon and that is, is it universally true that the best recoveries are obtained only when less than 1 per cent. of oil is employed? In my own experience I have found that much more than 1 per cent. of oil can be used with certain ores, without affecting the recovery. It is doubtless true that some classes of ores give better results with less than 1 per cent. oil, employing a certain prescribed method of treatment, but other methods of treatment may allow variations in quantities of oil that can be employed, in order to obtain the same recovery. The so-called critical amounts of oil below 1 per cent. in amount, in which such effective results are claimed, is not true with all classes of ores, as far as my own experience goes.

My problem of concentration has been very unusual, owing to high transportation costs, in which I have been required to concentrate a 5 per cent. copper ore which is mostly bornite. As is well known, bornite usually has an oxidation of the surface which in some cases seems to interfere with an effective coating of the oil, thus diminishing the flotation effect. This tarnish probably acts like the non-metallic surface of the ordinary gangue minerals. We tried a good many experiments and found that no matter how we varied the character or amount of oil, it did not seem to give any satisfactory recovery. Fortunately the gangue was calcite and small additions of sulphuric acid immediately produced sufficient CO_2 bubbles to float effectively the particles which were not raised by the air bubbles.

G. D. VAN ARSDALE.—In the work which I have done I have not been able to find that a "critical" point exists at 1 per cent. of oil, and I do not see any theoretical reason why there should be a "critical" point at this, or in fact at any, percentage of oil.

The difficulties which Mr. DuBois speaks of in the use of more than 1 per cent. of oil in my opinion may be perhaps obviated by a different apparatus construction or by different details of manipulation so that with an amount of oil greater than 1 per cent. under certain conditions it will be possible to get fully as good or even better results than with less than 1 per cent.

THE CHAIRMAN (E. P. MATHEWSON, Toronto, Ont., Can.).—One of these experts came to us during the experimental stage at Anaconda, and said that he could float our minerals. We put him in charge of the first commercially sized unit we had and for 3 weeks he labored with all his lore and experience, but his results were abominable. He was on the point of giving up when either he had an inspiration or some member

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of the staff suggested to him that he try a little sulphuric acid. Then the adsorption was removed and everything went smoothly.

Another thing, as to the amount of oil being exactly 1 per cent., I know that there were some very excellent concentrates produced when considerably over that amount of oil was used but, of course, the company that I was with, having made an arrangement with the patentees of the process, got on the economical side as quickly as possible and found how little oil we could get along with. We used considerably less than 1 per cent.

J. W. BELL.—The question raised by Mr. DuBois is at least of scientific interest and may be very important in its practical aspect. I recall one laboratory experiment in which nearly 2 per cent. of oil was used to secure a good result. In the concentration of an ore, consisting of calcite, barytes and chalcocite, by tabling, it was found that only about a 25 per cent. copper concentrate could be made. Practically all of the barytes was contained in the concentrate, and it was of interest to devise means for raising the grade of the concentrate by eliminating the heavy spar. By strongly heating the concentrate, the decrepitation of the barytes permitted removing much of it by screening but the separation could be made much more effectively by flotation. By using 38 lb. of wood tar oil, it was found that 94 per cent. of the copper in the original concentrate could be obtained in a flotation concentrate containing approximately 50 per cent. copper.

Mr. DuBois's last experiment reminds me of some recent experiments I made to see whether it would be possible to float the sulphides in the Porcupine ores. As is well known, at the Hollinger mill, table concentration is used to separate the sulphides from the gangue and as the ore is ground to 200-mesh, before concentration, it seems reasonable to suppose that a large part of the mineral must go into the tailings. Just as a matter of scientific interest, therefore, a few tests were made on ore similar to the Hollinger ore to see if a good separation could be made by flotation. It was found to be impossible to float the sulphides in a neutral solution, but it so happened that before the oil was put in, the acid was added and it was observed then that a splendid flotation was made. Very clean concentrate was floated to the surface of the spitzkasten, but of course the froth was extremely weak and to have removed the concentrate it would have been necessary to use quite a heavy overflow.

The test described merely illustrates the flotation of sulphides by CO_2 gas as in the Potter-Delprat process, but it is possible that the effectiveness of the use of acid in an oil flotation process is partly due to the liberation of CO_2, which seems to be an extraordinarily effective gas in the flotation of mineral particles.

H. W. DUBOIS.—Japan (I am informed) has passed a law by which no flotation patents will be allowed, solely in the interests of the best con-

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servation of their natural resources. That is interesting, if true. I also am informed (and this, I think, is reliable information) that applications, with the identical wording of the fundamental flotation patents which were granted to the Minerals Separation company in the United States, were submitted previous to the U. S. applications, to the German government. After an investigation, the German patent office decided that there was nothing new in these claims and therefore patents were not allowed.

It is a matter of undoubtedly a good deal more importance to us as mining men to know, if these flotations patents are going to be sustained by our highest courts, whether the practice in the past of demanding excessive royalties is to be maintained. In some countries, if a man has a patent and wants royalties which the industry considers unreasonable, those affected can go to the government and get an adjudication as to some reasonable rate of royalties. That is not true in our country; but I think we should take, as mining men generally, a little more interest in the subject to prevent statements which are not true from being quoted as truth before our highest courts, and apparently accepted by them with such confidence that decisions are being rendered along such lines as are going to greatly burden the mining industry, on account of the unreasonable royalties demanded by the parties having their patents sustained by the courts.

DAVID COLE, El Paso, Tex. (communication to the Secretary*).—I notice that Mr. Callow takes issue with me on certain points I have previously contributed to the *Transactions*, to which I would reply as follows:

Regarding Inspiration: Mr. Callow's refinements of gravity methods made just prior to the advent of flotation in the Southwest were intended to be "down to the minute," and the gravity flow sheet for Inspiration ores which he brought out at that time as the result of his long campaign of experimentation was regarded by many as the last word in wet gravity work. It was not my intention to discredit this good work when I said "he threw small light on slime treatment." "Minus 200 mesh," however, is not synonymous with "real slime." Repeated treatments by gravity methods will result in the separation of a very large percentage of the fine granular sulphides in sizes much finer than 200 mesh, and I gave him due credit for that. The unavoidable losses were made in the "real" slime which I mentioned in the paragraph which Mr. Callow refers to as my "statements on page 631," and his refinements were not a solution to the real slime problem.

Mr. Callow has called attention to the fact that his best results at Inspiration in gravity work were obtained on *Joe Bush ore* and he presents a table, viz., Tests Nos. 68, 71, and 72, and asks us to consider and

* Received Mar. 29, 1917.

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compare these results, made by his refined gravity methods, with the results obtained by flotation methods in the new mill "during the second half of 1915 and in August, 1916." Of course, the ore treated in the mill was what happened to come. *It was not Joe Bush ore*, and therefore comparisons of detail figures are of no value.

In regard to Mr. Callow's statements under the caption "Callow Cells Adopted by Arizona Copper Company," I would say that those who have been sufficiently interested to read my paper and his remarks will notice what amounts to contradictory statements on several points which I cannot allow to go unchallenged. I was on the ground constantly and got my information at first hand. The tables published were the official ones. Mr. Callow was not present during the period and has been laboring under that disadvantage.

As stated in my paper, page 667, the "feeds were not identical," and I would call attention to the fact that the contributing flow sheet depends upon methods of preparing the feed. My request to have the feeds made identical was not heeded. The C-B cell would do anything the Callow cell would do, plus the one of not being embarrassed by coarse sands. The C-B cell was designed at Morenci especially to fit the conditions in the plant where there is inadequate room for the usual apparatus for preparing the feed. It was made with a view to treating all of the tonnage comfortably in the space available, and at a cost for construction amounting to not more than one-third the amount needed with the use of the Callow cell. Therefore, the question of previous preparation was important, and I proposed that both units be operated simultaneously on exactly the same feed, first on the feed as prepared for Callow treatment, and then on the feed as prepared for C-B treatment. Of course, the C-B cell would handle the Callow feed, but the Callow cell would fail on the proposed C-B feed, and that is the reason the feeds or contributing flow sheets were not made identical.

In my paper, on page 662, I said: "the blower used with the C-B apparatus was an old one designed for not more than 3½-lb. pressure; with 6-lb. pressure the shafts were deflected and the machine had to be water-jacketed to keep down the heat (impellers rubbed the sides); it was much larger than necessary and a great excess of air was blown off from open valves. Therefore, no record of the amount of air used or power required could be even approximately obtained." Mr. Callow's statement that the C-B apparatus required 5.9 hp. per ton, cannot, therefore, have a foundation in fact.

The labor used in the testing was identically the same for the C-B as for the Callow apparatus. Much more labor was used on each unit than was necessary.

The statement that the Callow apparatus would recover from 1 to 1.2 lb. of copper per ton more than the C-B recovery would have been called

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to my attention in detail at the time, if it had been a fact capable of demonstration, and the C-B unit under discussion would have doubtless gone to the scrap heap, which is not the case.

FLOW SHEET OF NO.4 MILL (500 TON) OF ARIZONA COPPER CO.
CLIFTON, ARIZONA

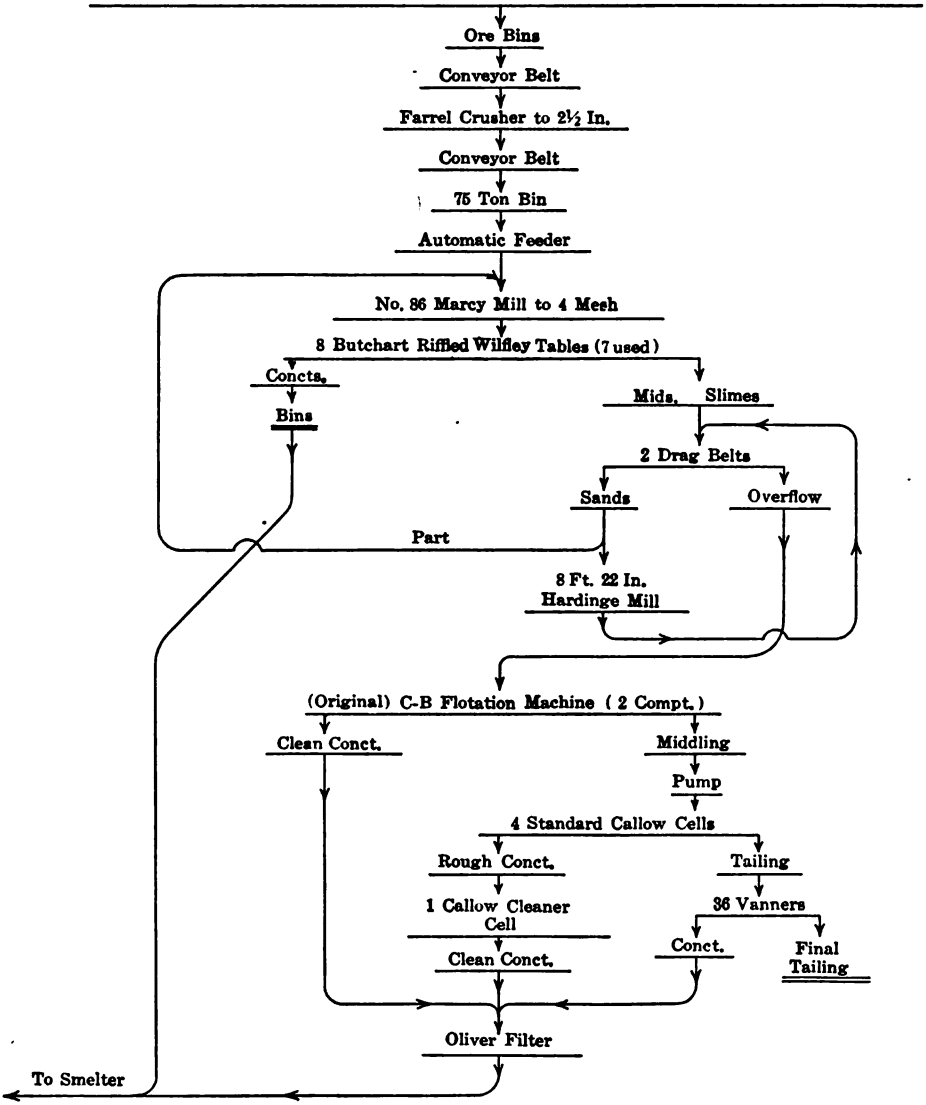

FIG. 1.

Power, efficiencies, recoveries, and labor requirements were so nearly parallel in the two systems that they were not factors in the Arizona Copper Co.'s decision to use the Callow apparatus in its No. 6 concentrator.

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It will interest those who follow this discussion to know that the Arizona Copper Co. operates two separate concentrating plants for sulphide ores. One of these mills is at Morenci. The other one is of later design and construction and is at Clifton. The ores for both are derived from the Morenci mines and are substantially the same in character.

The Clifton mill has a capacity of 500 tons per day and employs the flow sheet shown in Fig. 1 accompanying this discussion.

In this flow sheet it will be noted that the original C-B machine (moved from the No. 6 mill) occupies a prominent position, handling the whole tonnage on two of its three cells, making a clean concentrate in very large percentage of the whole, which goes to the filters for dewatering. The Callow cells are used in a second treatment of the pulp, and the reject from these cells is finally passed over vanners for final treatment before going to waste. This makes a superior flow sheet for "Morenci" ores and the results obtained compare favorably with the results obtained in the larger mill at Morenci in which Mr. Callow's cells were "adopted." Whether C-B treatment of the reject of the first C-B treatment would be inferior to the Callow treatment practised is not known, but we have every reason to believe that the results would be parallel.

As throwing additional light upon the power requirements of the rival systems of practising pneumatic flotation, I would say that at Cananea one 3,000-cu. ft. Roots blower, using 98 hp. input to motor, furnishes air for three triplex C-B machines and three duplex cleaner units. The total area of aerating cells is 307 sq. ft. The tonnage handled is 1,200 tons per 24 hr., and one Mexican operator and a helper per shift is found to be sufficient labor to handle the equipment. If the labor received $4 gold per 8-hr. shift, the cost per ton for this item would be 2 c. The power used in blower, it will be noted, is 2 hp.-hr. per ton treated—this with 5 to 6 lb. pressure of air, including also the centrifugal pumping required in returning rejections from the cleaner.

ARTHUR CROWFOOT, Morenci, Ariz. (communication to the Secretary*).—In order to set at rest any doubts in regard to the reasons for selecting the Callow pneumatic flotation process for use in the concentrators of the Arizona Copper Co., Ltd., at Clifton and Morenci, I am presenting the following facts gained from the results of test work carried out on the Callow and Cole-Bergman types of flotation machines at Morenci.

Prior to the comparative test of merit between the Callow and Cole-Bergman machines, many months' time and a large amount of money had been expended in the development and testing of the Cole-Bergman

* Received May, 1917.

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machine, and it was necessary to decide quickly what make of machine was best to install.

The Callow pneumatic flotation process was recommended for installation in our concentrators for the following reasons:

The results indicated:

1. That the Callow system made the highest extraction of copper values from the feed treated.

2. That the Callow system was simpler to operate and could be operated at a lower cost.

3. That the Callow system operated with a lower air pressure. (The question of the actual power consumption of the two types of machines was not definitely settled at this time.)

During the official test period the Callow flotation apparatus showed an additional recovery of from 1 to 1.5 lb. of copper per ton more than the Cole-Bergman machine, this difference being maintained by further treatment of the tailing from both machines.

The simplicity of the operation of the Callow flotation system is one of its notable features, two operators per shift being able to handle 90 standard cells and blower equipment.

At the time of the test we were, unfortunately, unable to obtain exact comparative data on power consumption by Callow and Cole-Bergman flotation machines for the reason that the Cole-Bergman machine used a blower which was working at a higher pressure than that for which it was designed, resulting in an unnecessarily high horsepower consumption. It was considered doubtful, however, on account of the wide difference in the power results obtained, that the Cole-Bergman power consumption could be brought down to as low a figure as the Callow power consumption, and it was evident that it could not be brought to a lower figure; this assumption has since been sustained by the operation of the Callow and Cole-Bergman machines in the No. 4 concentrator at Clifton.

On account of the indefinite data on power consumption, this point was not considered in influencing the decision to adopt the Callow machine.

The above facts were determined from test runs made at Morenci, during which the feed to both types of machines originated in the discharge of Hardinge pebble mills treating secondary table middlings.

With the exception that the feed originated from different mill units, it was identical for both machines. In the Callow system, the mill discharge was sent to a Caldecott cone, the spigot discharge of which was returned, via Wilfley tables, to the original mill for regrinding, the overflow of the cone going to the flotation machines.

In the case of the Cole-Bergman machine, the mill discharge was sent through a special form of Pachuca tank to the first compartment of the flotation machine attached to the bottom of which were three classi-

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fier cones. The spigot discharges of the classifiers were returned, via Wilfley tables, for regrinding in the original mill as in the case of the Callow machine, the rest of the feed passing through the two remaining compartments of the machine. In both cases the cleaner middling was returned to the system.

The final decision made in favor of the Callow machine was also influenced by the fact that while the Cole-Bergman machine possessed much merit, further development appeared to be required before it could be considered perfected, and for this there was not time, while the Callow machine was already standardized.

The feeding into a flotation machine direct of a Hardinge mill product that has been ground in the mill to pass a 20-mesh screen appeared to me to be an unnecessary and unscientific method of procedure, for the following reasons:

The mill product contains a considerable percentage of material upon which the flotation machine can do no useful work. This material consists of middling grains that contain included grains of mineral, and also free mineral grains too heavy to be lifted by the bubble column of the flotation machine. All of this material must be removed from the flotation machine, and sent: first, to tables so that the heavy grains of free mineral may be removed; second, to a regrinding mill so that the sand may be crushed to a fine size in order to liberate the included mineral grains; and third, back to the flotation machine again. (The use of tables as mentioned above is dependent upon whether the amount of coarse free mineral grains contained in the coarse sand is sufficient to make a gravity concentration process profitable.)

Now the question arises: if these included mineral and heavy free mineral grains are not benefited by being passed through the flotation machine, why should they be sent to the flotation machine before they are properly prepared for treatment? It certainly seems advisable to remove this material before passing the feed on to the flotation machine, by the use of some form of outside classifier, such as a Dorr, Aikens or Allen cone, which can be controlled to advantage, rather than to feed the discharge of the mill directly into the flotation machine and trust to a system of hydraulic classifiers over which we have imperfect control, to remove the sand requiring regrinding and the mineral grains which are too heavy to be lifted by the bubble column.

In our flotation practice we find that we cannot obtain satisfactory results if we send to the machine a feed containing more than 3 to 4 per cent. of material that will rest on a 48-mesh screen; therefore, I can think of no good reason for sending coarser material to the flotation machine, but can see a very good reason for separating the coarse feed from the fine while it is still outside the flotation machine and sending it to the

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tables where it belongs, recovering the coarse free mineral and then returning the rest to the regrinding mills to be ground to pass 48-mesh.

I look upon the work of the preparation of a flotation feed as the work of a grinding and classifying system which must work independently of the actual flotation operation. The actual flotation work should start with a properly prepared pulp; any attempts so far made to unite the grinding and flotation systems by classifying out the coarse sands in the flotation machine have resulted, to the best of my knowledge, in frequent choke-ups, the use of additional water, more skill and attention required on the part of the operator to keep the system running, and in inferior metallurgical results.

My reasons for not heeding a request to feed the Callow cells with a minus 20-mesh mill product are obvious from the foregoing statements.

The Cole-Bergman machine was removed to our 500-ton mill at Clifton, and operated over a long period of time in parallel and in series with Callow machines. This machine has recently been replaced by a second unit of four Callow cells.

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The California Gasoline Industry*

BY W. R. HAMILTON,† A. B., SAN FRANCISCO, CAL.

(Arizona Meeting, September, 1916)

THE knowledge of the existence of petroleum in Southern California dates back to the days of the missions. The presence of asphaltum and semisolid bitumen was reported at Santa Barbara in 1792, but no serious attempt was made to develop oil until Professor Silliman's optimistic report in 1865 started California's first oil boom. Wells were drilled in many counties of the State, but the equipment was unsuitable and the drilling difficult and no production was obtained except in Ventura County. Even there the production was of little importance commercially, and not until the early '80s did it become sufficient to again attract attention. In 1888 the production for the State had reached a total of about 700,000 bbl. per year, practically all light oil produced in Ventura County, Pico Canyon, Los Angeles County and in the Puente Hills. The production of petroleum attained the dignity of an industry in 1895 when the Los Angeles City and Coalinga fields were discovered. The subsequent development has been remarkable, and in 20 years the production has increased from 1,000,000 to 100,000,000 bbl. per year.

The refining industry has kept pace with the production of oil. The Pacific Coast Oil Co., the predecessor of the Standard Oil Co. of California, was the virtual pioneer refining company of the State, having built a refinery at Alameda about 1880. When purchased by the Standard, about 1902, it was dismantled and the refinery at Point Richmond was built. The Richmond refinery is now one of the largest in the world, having a capacity in excess of 60,000 bbl. per day. A small refinery was built by the Union Oil Co. at Santa Paula in the early days of the industry and later many asphalt refineries were in operation in Southern California. The Pacific Coast Oil Co. was then, as has been its successor, the Standard Oil Co. of California, the principal refiner and marketer of light products, the most valuable of which was kerosene.

Before the opening of the twentieth century, gasoline of excellent quality was produced, though it was always a drug on the market and was produced for the simple reason that its presence rendered the flash and fire tests of the kerosene too low. The user of kerosene at the present

* Originally presented at a meeting of the Southern California Section on Feb. 1, 1916.

† Petroleum and Mining Engineer.

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date obtains a safer and better oil than he might receive had not the value of gasoline been increased by the increased demand.

The phenomenal development of the California oil fields in the earlier years of the twentieth century was principally in oils of such high specific gravity that they were used in the crude state as fuel oil. During the last 6 years, which included the advent of the gusher period of the Midway fields the production of refinable oils has increased greatly, while, strange as it may seem, the production of heavy non-refinable oils has decreased. During this period, from the beginning of the present century, the introduction and perfection of the automobile has changed gasoline from a despised and troublesome byproduct to the most valuable and important product of the crude oil. The production of light oils did not keep pace with the demand for gasoline and as a consequence the price of gasoline increased from about 16c. in 1904 to 25c. in 1910. The shortage was reflected in the prices paid for light crude, and in the feverish development period which resulted an enormous increase was shown in its production, until in 1914 the output of light oils capable of being refined was in the neighborhood of 50,000,000 bbl. The resulting gasoline being considerably in excess of the demand, prices began to fall in 1911 and, aided by importations of gasoline from the Dutch East Indies, the price rapidly declined to a low level of 11c. per gallon in the summer of 1915.

The late increase in the price of gasoline is ascribed to the following causes:

1. Decreased production of refinable oil. The decrease probably amounted to about 6,000,000 bbl. in 1915.
2. Steadily increasing consumption.
3. Discontinuance of gasoline imports due to improved European demand.
4. Heavy exports due to European war.

All of these conditions may reasonably be expected to exist for some months to come. The recent storm disaster of the San Joaquin Valley fields will undoubtedly have a serious effect on the production of January and February of 1916. When it is considered that the oil stored above ground is but equal to the production of a half-year, it is plainly to be seen that the shutting in of a large number of wells for a few weeks will have an important effect upon the status of the industry. There is no reason to expect a reduction in the rate of consumption. The acceleration of the growth of the automobile industry shows no decrease and, with the rapidly increasing mileage of improved highways of the State of California, an increase rather than a decrease may be looked for. It is unlikely that gasoline will be imported from the Dutch East Indies so long as it is possible to deliver it to the famished European market. The serious shortage of oil in the Eastern and Mid-Continent fields, which developed in 1915, and which resulted in cargoes of gasoline being shipped

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from California to Europe, does not as yet show evidence of alleviation, and the only prospect of production reaching consumption seems to rest in the discovery of another phenomenal field such as Cushing, Okla. Unless the Eastern fields can increase their production it is to be expected that further exports will be made from California.

Possibilities of the Immediate Future

There is no doubt that, unless the year 1916 shows a considerable increase in the production of gasoline, which is unlikely, or unless a satisfactory substitute for use in motor vehicles is provided, we face a serious gasoline shortage on the Pacific Coast.

Increased production of gasoline may possibly come from one of the following:

1. Increased production of light oil.
2. Increased production of casing-head gasoline.
3. Lowering the grade of market gasoline.
4. Innovations in refining methods, such as the Rittman, Burton, Snelling, Cosden, McAfee, Wells, Kelsey and Washburn processes.

1. There does not appear to be more than temporary relief to be looked for in increased oil production. All the principal fields have apparently nearly reached the acme of their productiveness, and while some may increase, the decline of the others will probably outweigh their gain. Unless the writer is greatly mistaken, the year 1914 will long remain the banner year of California's oil production.

2. The process of the production of casing-head gasoline, which is the recovery of the lighter hydrocarbons usually lost in oil production by volatilization, is of recent development. At present it accounts for less than 10 per cent. of the gasoline marketed in the State. Owing to the low price in 1913 and 1914, the growth of gasoline production from the gas industry has been greatly retarded, but several new plants are now under construction. However, the gas amenable to such treatment is limited, and it is unlikely that the new production of gasoline from this source can be expected to have any marked effect.

3. By lowering the grade is meant including higher boiling-point fractions in the gasoline as the cut is made at the refinery. Before the demand for gasoline was great, the gravity of marketed gasoline was about 65° and even as light as 72°Bé. The boiling point of the "last over" or the "end point" of the distillation of such a gasoline was little higher than 200°F. With diminished supply and increased demand, the gravity has been gradually decreased until the present standard is from 59° to 61°Bé. with an end point of from 320° to 380°F. The cut in the refinery distillation is, therefore, widened and the production of marketable gasoline produced is thereby increased. If no other way is found to supply

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the demand, this cut can be still further widened and still supply the motorist a satisfactory fuel. The production can probably be increased from 30 to 50 per cent. in this way.

4. The processes referred to show considerable promise. They depend for their success upon being able to break up the molecule of high boiling-point hydrocarbons into lower boiling-point molecules. The processes may be divided into two main classes, those which depend upon excessive heat or pressure or both and those which depend upon a catalytic agent.

The most notable of the former are the Burton and Rittman processes. The Burton process, which was patented by William M. Burton in 1913, is controlled by the Standard Oil Co. and is said to be in operation at its refineries at Whiting, Sugar Creek, and Neodosha. This process, after many difficulties had been overcome and much money had been expended in experimental work, was made successful and is now being installed in many Standard Oil refineries. Much difficulty was first encountered in producing a spirit that was of good color and odor. These have been corrected and the Burton process now gives the Standard Oil Co. an important handicap over all rivals. To date, it has not been successfully applied to California asphaltic oils where the presence of a large proportion of unsaturated hydrocarbons has introduced problems not found in the oils from the Eastern fields.

The Rittman process is the result of the research work of Dr. W. F. Rittman of the U. S. Bureau of Mines. It has already been successfully applied to the commercial manufacture of toluene and benzene. It differs from the Burton, Snelling, Cosden, Washburn and Wells processes, in that the action takes place after the liquid has been vaporized. By subjecting the vapor to pressure and heat, a re-arrangement of the molecules is effected, liberating carbon which is removed from the plant without difficulty. The process shows much promise and, while much remains to be done before it can be considered a commercial success, it has created a profound impression among refiners. Under the rulings of the Government, a Federal employee may not profit in this country from any discoveries or inventions during his connection with the Government. The foreign rights will be controlled by Dr. Rittman. The Government proposes to allow anyone to use the process under license, and it is probable that the increased value of gasoline and the handicap the "independent" refiners are confronted with on account of the Standard's control of the Burton process will result in an early attempt to perfect the process. It has been perfected as far as is possible in the laboratory stage and its future depends upon the results of its application on a practical scale.

Of the processes making use of a catalytic agent, the McAfee process,

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which is controlled by the Gulf Refining Co., is the most promising. The catalytic agent used is aluminum chloride, and results from experimental work are said to be excellent. However, this process, like the Burton, will probably be unavailable to other companies.

The future has generally cared for itself and will undoubtedly do so in the gasoline industry. While we shall probably have a gasoline shortage extending well into 1916 and perhaps into the early part of 1917, the result will be redoubled interest in the subject of perfecting such ideas as have been brought to light in the experimental work done in the above-mentioned processes. The research work of Dr. Rittman and other able scientists who are working on the problem, bids fair to develop a practice that will revolutionize the refining industry.

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The Diastrophic Theory*

A Contribution to the Study of the Mechanics of Oil and Gas Accumulation in Commercial Deposits

BY MARCEL R. DALY,† SEATTLE, WASHINGTON

(Arizona Meeting, September, 1916.)

THE writer has devoted a number of years to practical operations and to the study of geology in the oil fields. In consequence, he has been brought to investigate the theories advanced to account for the accumulation of oil and gas in commercial deposits. It is the result of these investigations and his personal conclusions that he wishes to sum up in this paper.

The writer is an advocate of the organic origin of petroleum found in pools. He has been led to believe that the present theories of oil and gas accumulations are incomplete and, in certain respects, incorrect, although they may embody certain elements of truth; that the forces that are called upon to explain the movement are only secondary forces in the process, and insufficient, by themselves, to cause this movement; and that the phenomenon of accumulation is of much larger order than heretofore admitted and bears an intimate relation with the general as well as with the local deformations of the crust and is a direct consequence and a mechanical effect of these deformations to which the term "diastrophic" has been applied. As a complement to this theory, the writer offers a new interpretation of the "rock pressure" and of the mechanism of the "sealing up" of this pressure in oil and gas pools.

The Anticlinal Theory

The theory most generally accepted to account for the accumulation of hydrocarbons in commercial deposits is the anticlinal or structural theory. This theory, the principles of which have been laid down by I. C. White, Orton, Winchell and others, and which has since been modified and perfected, is too well known to need a detailed statement. A clear interpretation of it may be found, in the paper of W. T. Griswold, in *Bulletin No.* 318 *of the United States Geological Survey,* and the reader is

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referred to it. In this theory, the *force* which is supposed to have caused the motion is the gravity of the hydrocarbons; and the principal *factors* which have intervened in the process are the structure and the general stratigraphic conditions of the rocks, their texture and porosity, and their water content. The oil and the gas are supposed to enter the porous rock that is to act as a reservoir, by some indefinite process. Any further movement toward accumulation is only considered as possible if a dip exists in this rock sufficient to overcome the friction, and practically impervious areas of the rock will be more or less perfect barriers against the movement. Then, if this rock is completely saturated with water, or if the hydrocarbons have entered it below the line of complete saturation, the oil and the gas will gradually move up the slope by the effect of buoyancy; the gas, with its lower specific gravity, occupying the higher places. Should the rock be dry, or if the hydrocarbons have entered the rock above the line of saturation, the oil will flow down, as long as gravity is sufficient to overcome the resistance to motion, and the gas will diffuse with the air or water vapor contained in the pores of the rock.

From this statement it will appear that the following assumptions are an absolute requisite for the anticlinal theory:

First, the structural deformations (dips, anticlines and synclines, domes, terraces, etc.) must be supposed to have existed previous to the introduction of the hydrocarbons in the porous strata; in other words, it must be assumed that structural deformation has preceded the movement toward accumulation. Indeed, in this theory, the preëxistence of structural deformation is the very basis of the action of gravity and, at the same time, the theory implies that the forces which have caused the deformations have had no bearing on the movement of the hydrocarbons.

Second, gravity or buoyancy is to be considered the sole agency through which accumulation has been brought about and, as such, is supposed to be adequate to explain accumulation under any condition of dip.

To which the following considerations may be added: The movement toward accumulation would have to take place in the porous rocks when they are already solidified and partially cemented. There would be no connection between the causes which have led: To the introduction of the oil in the porous strata; to the movement of the fluids in these same strata; and to the "rock pressure" itself. Further, this theory does not provide for any satisfactory explanation as to the mechanism of the "sealing up" of the pressure in the pools.

The first assumption is begging the question. To the best knowledge of the writer, no attempt has been made by the authors of the theory even to discuss the point or to prove the accuracy of the inference. It seems as if, from the start, it was admitted as self-evident that the gravity of the fluids was the only possible force entering into action. The writer will try to show later on why, in his opinion, this assumption

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falls short of truth and how its admittance may be explained by the fact that the problem has not been attacked from its true angle. On this first point, to say the least, the anticlinal theory is not established on proven ground.

The second assumption may be seriously contested. The force of motion due to buoyancy is a function of the sine of the dip. It would be maximum for a vertical stratum and null for a horizontal one. Many anticlines in the Eastern fields of the United States have a dip so low that a motion caused by buoyancy alone could hardly be understood. Dr. Ashburner has found a maximum dip in the Bradford region of 69 ft. per mile, or less than 1 ft. for 76 ft., while Carll has shown that the dip of the oil sands in the Venango belt, and the southern end of the Butler belt, rarely exceeds 34 ft. per mile (Sir Boverton Redwood), or less than 1 ft. for 155 ft. In West Virginia, the maximum dip of many anticlines is less than 1 ft. for 50 ft., which corresponds to an angle of about 1° 8′ 45″. For this last dip, the force due to buoyancy, which is supposed to move the oil up the slope, would be reduced to 0.0066 of the weight of the oil, as pointed out by Malcolm J. Munn, of the U. S. Geological Survey. A volume of 1 c.c. of oil would thus be submitted to a moving force of a few milligrams. The oil could be set in motion only if this force were greater than the resistance due to static friction, viscosity and surface tension. It is not possible, with the data now at hand, to submit the question to a complete mathematical treatment. As Van Hise points out,[1] "the pore openings in sandstones are for the most part capillary", and the flow of oil through the rock would have to obey the laws of flow through capillary tubes, which are not yet satisfactorily expressed. Further, the pore openings would be extremely irregular in shape and dimensions. But it is possible to reduce the problem to a simpler form. If, barring viscosity and surface tension, we would only consider the resistance due to static friction, we would obtain an inferior limit to the resistance. And if it were possible to show that the force of motion due to buoyancy is insufficient to overcome such a frictional resistance, in the case under consideration, it would be proof that the anticlinal theory can not be accepted, at least in its present form, and that other forces besides buoyancy are to be considered in order to explain the motion.

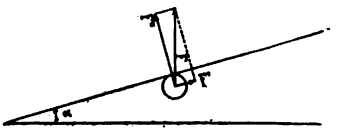

FIG. 1.

Let us imagine a globule of oil appressed against the roof of the sandstone stratum by the effect of buoyancy (Fig. 1). Let F be the force of buoyancy. This force may be decomposed into two components: F' parallel to the dip, which would cause the globule to slide along the roof, and F'' normal to the roof, which would press the drop of oil against it.

[1] A Treatise on Metamorphism, *Monograph* 47, *U. S. Geological Survey*, p. 138 (1904).

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The effect of this pressure is to create a resistance to the sliding, which is proportional to the pressure and directed down dip. This proportion, which remains constant when the nature of the surface remains the same, is the friction coefficient. Let it be c. The movement will become possible only if $c < \frac{F'}{F''}$, or $c <$ tang α, α being the angle of the dip. If the dip is 1 ft. to 50 ft., the tangent is 0.02 and the condition reads $c <$ 0.02.

There are no data available for the friction coefficient of oil and sandstone under the conditions stated. But one may get an idea of the order of magnitude of this coefficient by a simple experiment. Let us take a slab of some oil-bearing sandstone, some Berea grit, for instance. The lower surface of this slab must be planed, but not ground, so that the grains of sand remain entire. Let us immerse the slab in some salt water of proper density, in a horizontal position, and loosen a drop of oil from a syringe in the water, under the slab. The drop will rise to the bottom of it, and here it will remain if the slab is perfectly horizontal. Let us then incline the slab progressively and note the smallest angle of dip at which the drop will be set in motion. The tangent of this angle will be equal to the coefficient of static friction.

The writer has made this experiment repeatedly and has never succeeded in getting the drop in motion for any such angle as 2°, which would tend to prove that the coefficient is superior to the maximum limit required.

It is further to be noted that the spherical form is the general form of oil drops in an emulsion, where the particles of oil are, in great majority, very small; it is also the form which, every other thing being equal, offers the least resistance to motion. Hence, if the motion is shown to be impossible under this form, it will be equally impossible under any other form, and the whole theory falls through.

The change from a spherical form to an irregular one, by the flattening of the drops when compressed against the walls or when passing through irregular and narrow channels, the indentations, the penetration of the oil between the grains, the effect of viscosity, the capillary action—especially if gases be held in the emulsion—would be as many causes furthering an increased resistance to motion.

Now, the fact that the movement toward accumulation is not possible under the sole influence of gravity, with the low dips so characteristic of many oil regions, does not mean that gravity has had no influence in the process. It simply means that this force alone would be inadequate to explain the motion and that some other force or forces must have entered into play. But though gravity of hydrocarbons may not be a primary agent of their accumulation, it is a force which must be taken into account in any general theory of the process.

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The Hydraulic Theory

In opposition to the anticlinal theory of oil and gas accumulation through gravity, Malcolm J. Munn has propounded[2] another theory in which the moving force would be the hydraulic (not hydrostatic) pressure and capillarity of underground waters. This is termed *the hydraulic theory.*

"The fundamental idea of the hydraulic theory," writes Mr. Munn, "is that moving water under either hydraulic or capillary pressure has been the direct agent of accumulation of oil and gas pools. To this idea may be added another of equal value—the pools of oil and gas are held in place by water under hydraulic and capillary pressure which effectively seals up all the pores of the surrounding rock and prevents the dissipation of pressure by diffusion." This theory is very interesting and, as will be seen later on, the writer agrees with Mr. Munn on two points, viz., that water under hydraulic pressure has really been the primary agent of motion, and that the "sealing up" of the pools is a phenomenon of surface tension. But, as far as the writer can see, Mr. Munn has reduced the hydraulic pressure to that of underground waters circulating in the same way as they do now, and his statement relating to "hydraulic and capillary pressure" is rather undetermined.

"Capillary pressure" is a somewhat misleading term. If by this is meant the general action of forces due to surface tension, it is well to note that surface tension may create a resistance against the flow as well as a tendency toward it, as Mr. Munn himself seems to acknowledge when he comes to the "sealing up" of the pools. If it simply means the process of infiltration, the creeping of a liquid through capillary channels, this process has its limitations and would seem to be a process of dissemination rather than of concentration. On the other hand, hydraulic pressure of underground waters may be inadequate. Hydraulic pressure is a function of the square of velocity, and the velocity of underground waters is known to be extremely small. "The motion of the ground water as a whole," writes C. S. Slichter,[3] "is somewhat like the slow motion of very viscous sirup or the slowly creeping ice of a glacier." It has been shown, for instance, that the ground waters of the Arkansas River flow in gravels at a rate not greater than 3 to 5 m. a day. This would mean a velocity between 0.00348 and 0.0058 cm. per second, or less than from 13 to 21 cm. per hour. It is possible to show that, should the static coefficient of friction between oil and sandstone be greater than 0.02, as heretofore admitted, a velocity of 0.006 cm.-sec. would not overcome the frictional resistance of oil in an inclosed pipe, on a horizontal

[2] *Economic Geology*, vol. 4, No. 6, p. 523 (September–October, 1909).

[3] The Motions of Underground Waters. *Water Supply Paper No.* 67, *U. S. Geological Survey*, p. 35.

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plane, barring the additional resistances due to viscosity, surface tension, deformation, etc., which would interfere in this case. Though the analogy may not be entirely correct, it has a tendency to show that the influence of hydraulic pressure of underground waters is here doubtful and at least remote in importance. There will be always a great difference between the transportation of a material in the state of solution, which may be followed by subsequent re-deposition, and the transportation of insoluble solid material, as oil in water. Further, the theory is open to the same general line of objections as the anticlinal theory, and more so, as structural deformation, which is so intimately connected with the phenomenon of accumulation, becomes here entirely subordinate and incidental.

The Diastrophic Theory

The fundamental weakness of the theories which we have reviewed may perhaps be traced to the hesitation of the authors of these theories to choose deliberately between the different conceptions of the origin of oil itself. This origin once ascertained, it is evident that the surest way to determine how oil and gas may have accumulated in certain places, would be to try to follow the possible movements of the oil from the time of its first appearance in the strata, down to the pools where we find oil today. In order to be rational, a history ought to be complete; and we have no more right to limit the history of petroleum to the space of time during which the strata, in which we find it now, may have existed in the same present shape and condition, than we would have the right to reduce the story of a man to the last years of his life. It is further obvious that the history of petroleum will have to be entirely different, dependent upon its origin, either from emanations coming from the depths, or from organic decomposition in the strata themselves. In the writer's opinion, many reasons favor the theory of organic origin for the petroleum found in commercial deposits, though this does not mean that petroleum of a different origin does not exist elsewhere. Hence, this is a point on which we have first to agree, or else agree to disagree. Should the organic origin of the petroleum that is found in pools be granted, the following interpretation is offered for the mechanics of its accumulation.

Oil developed by the process of decomposition of organic matter—whether vegetal, animal or both—would have first to exist in the sediments which contain the parent matter, in the state of dissemination. As Orton remarks, "disseminated petroleum is well nigh universal, but accumulations are rare."

In order that such an accumulation of disseminated particles may take place, three elements are necessary: An adequate source of supply; a reservoir to hold the oil and accompanying gas under pressure, and a process of concentrating the disseminated material and conveying it to

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the reservoir. The source of supply will have to be ascertained in every individual case. It is supposed here to exist, for the sake of argument. The reservoir is easy to imagine, inasmuch as we are able to corroborate our theoretical views by actual knowledge. A porous stratum overlaid by an impervious one, may act as a reservoir. This is the general case. Or the reservoir may be constituted by a porous stratum highly inclined and cut off abruptly below ground by a fault, which thus seals the oil and gas and prevents their escape. This kind is found in some Californian fields and elsewhere. Or the reservoir may be constituted by joint cracks in a shale, as is the case in the Florence oil field, Colorado,[4] etc.

The interpretation of the process of concentration and eventual migration, *i.e.*, of accumulation, is more complicated. As a natural process, it must obey physical laws, and especially the laws which govern the motion of fluids and gases; but the *forces* which may cause the motion (gravity in its different forms, heat, surface tension), the *agents* through which these forces accomplish certain results (water, oil and gas in varying physical or chemical states), and the *factors* that intervene in the process (structure and texture of the rocks, porosity, stratigraphic conditions, water and gaseous contents, viscosity, capillarity, depth and time) may have or have had a widely varying range of influence, according to past and present conditions. These elements would have to be seriated, according to their probable rank of importance, and their relative degree of influence determined, before a tentative conclusion could be reached. It thus becomes evident that if some broad general principle may be laid down, a great number of variations are to be expected for each individual case.

A few general observations may help us from the start. According to the organic theory, which is accepted here, the ultimate source of the hydrocarbons is to be found in sedimentary organic deposits, and oil accumulations in commercial quantities (oil pools) are always associated with sedimentary strata. The immediate inference is that hydrocarbons must have been submitted, from the date of their origin, to the action of the forces that may have affected these same strata.

If we refer to a map of the known oil fields of the globe, we will notice, with Sir Boverton Redwood, that "whilst petroleum exists very generally distributed throughout the world, the principal deposits occur along well-defined lines, often associated with the principal mountain chains." This remark is important and deserves to be emphasized. It means that petroleum deposits are mainly associated with the lines of lesser resistance of the globe, with the general direction of geosynclines and consequently with the areas of general orographic movement and deformation. The distribution of the Eastern oil fields of the United States and Canada along the Paleozoic trough, or geosyncline, of the

[4] C. W. Washburne. *Bulletin No.* 381, *U. S. Geological Survey.*

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Appalachian region, the location of the Western oil fields of the Pacific coast, from Alaska to California, followed by the Mexican oil fields, the deposits of the West Indies, of the northern coast of Venezuela, Columbia and a part of the Andes, along the general trend of a Mesozoic syncline; the oil fields of Galicia, Roumania, of the Caucasus, of Burma, of the Islands of Java, Sumatra, Borneo and New Zealand, following the path of another syncline, are extremely suggestive.

This first observation may be completed by a second. An oil field is always accompanied by a certain amount of local structural deformation, which sometimes is reduced to simple undulations, as is the case of the Appalachian region, or may reach a stage of high disturbance, with contorted strata, as is the case of the Galician oil fields. But local deformation has been observed everywhere. It becomes thus difficult to escape the impression that structural deformation, general as well as local, is more or less connected with the phenomenon of accumulation, and our inquiry is thus directed toward the possible action of the forces which have produced these deformations in the sediments from which hydrocarbons have proceeded and their possible action on the hydrocarbons themselves.

As stated before, oil would have first to exist in the stratum where organic decomposition takes place, in the shape of a finely disseminated matter. The sediments themselves would have to be deposited under water, in lagoons, marshes, deltas or at the bottom of the seas, in the form of mud or ooze. Such a mud would be composed of extremely fine mineral particles, intermingled with water in large amount. The particles of oil would take the form of spherical globules under the influence of surface tension, and, as these globules would be larger than the mineral particles of mud, they would be mechanically held in the mixture. Experiments conducted by Murray Stuart, Assistant Superintendent of the Geological Survey of India, and others, have clearly established this capacity of muds to hold oil in sediments by purely mechanical action.[5]

The preceding condition may be termed the *first phase* of the process. It is characterized by a layer of mud or ooze, holding mechanically, between its very fine mineral particles, disseminated drops of oil and gas bubbles, in a "matrix of water," as Murray Stuart expresses it.

When the conditions of sedimentation which allowed the deposition of the mud have changed, some other material may be deposited over this first layer; say some sand. Then, another change in sedimentary conditions will occur, and a new layer of material will cover the sand; suppose another layer of mud; and so on, successive layers of various sediments will be piled up in the sedimentary syncline.

Now, the progressive increase of weight due to this accumulation of

[5] *Records of the Geological Survey of India*, vol. 40, Pt. 4, pp. 320–333 (November, 1910), and *Economic Geology*, vol. 7, No. 1, p. 91 (January, 1912).

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successive layers will cause a progressive compression to take place. The effect of compression on a mass of practically incompressible mineral particles imbedded in water is to draw these particles closer together, to reduce the space available between them, and to squeeze out more or less of the liquid contents. The limit of compression is reached when the particles come in contact. For this reason, the result would be somewhat different, according to the size of the component particles of the layers. Mud or ooze particles are exceedingly small. When, in the syncline, the lower layer of mud would be compressed, the resulting texture would be close, and the tendency would be for the fluids to be squeezed out to a great extent; whereas the grains of a layer of sand are much larger than the particles of clay, and such a layer may become incompressible when it still contains a great percentage of holes. It may thus act as a reservoir for liquids even under considerable pressure. The overlying sand stratum would then remain porous to a large extent, and the liquids, escaping from the underlying mud, would rise and become confined in it. This may be termed the *second phase* of the process, and is characterized by the progressive transfer of a large part of the oil and gas from the clayey layer in which they have originated to the overlying porous stratum in which they still remain distributed in a finely disseminated state. At this stage, the porous rock may be supposed to be saturated with an emulsion of oil and gas in water.

Finally, in the course of time, sedimentation is stopped in the syncline of deposition, and orogenic movement begins. Whatever may be the cause of this movement, it is a tendency toward a new adjustment which evinces itself by a lateral thrust. In the simpler form, one rim of the syncline remains immovable, and acts as a resistance, and the other rim is brought nearer to the first one by a movement tangential to the crust, which squeezes the syncline as between the jaws of a vise. The strata are bent and lifted above the level of the waters.

It is possible to get an idea of the order of magnitude of the thrust and to compare it with the forces due to buoyancy and hydraulic pressure invoked in the preceding theories, and which we have found to be insufficient to explain the migration of the oil. For this, we have to consider that this new force exceeds the resistance to crushing offered by the most resistant sedimentary rocks. This crushing strength becomes thus an inferior limit to the force. Limestone, for instance, may be crushed under a pressure of between 400 to 1,200 kg. per square centimeter; so the thrust is to be measured at least in hundred kilograms to the square centimeter. We have already seen that the force of buoyancy, by which a cubic centimeter of oil is supposed to be lifted along a dip in the anticlinal theory, is to be measured in fractions of a gram (in milligrams for a dip of 1 ft. for 50 ft.), and hydraulic pressure due to underground waters, with the velocity admitted, would be still less important. The magnitude

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of the force we are considering now—the tangential thrust—is then at least one million times greater per unit surface than the two other forces previously considered. Further, this force has left its imprint in all sedimentary strata, not only by the flexure of the strata or their relative displacement, but frequently by dynamo-metamorphic actions which it has brought about. These actions have taken place between the time the strata were lying at the bottom of the waters and the time they have acquired their present situation. If then, any oil was ever existing at the

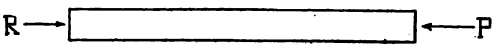

FIG. 2.

time of the deposition of the strata and has remained in some stratum since, it must equally have been subjected to the action of the thrust. The *third phase* of the process begins with this action of the thrust. But this needs a more complete analysis of facts.

When a square prism of a solid material, such as a piece of wood or an iron bar (Fig. 2), disposed horizontally on a support in order to avoid the action of gravity, and propped against a resistance R at one end, is com-

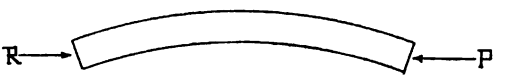

FIG. 3.

pressed at the other end by a force P, in the direction of its axis, the compression is instantaneously transmitted to the resistance, from end to end. Then, if the pressure is sufficiently increased, the prism will bend (Fig. 3). This flexion will extend the fibers on the convex side and contract them on the concave one, and there will be an intermediate layer which will be neither contracted nor extended by the flexion. This

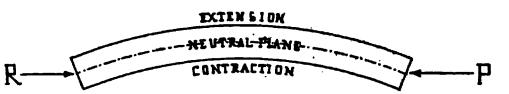

FIG. 4.

is the neutral plane (Fig. 4). Superimposed to this first effect, there will be a general contraction along the axis, which will affect the whole prism.

If, instead of a prism of solid material, we operate on a prism made of some homogeneous plastic material, like moist clay, the result would not be exactly the same. The compression at one end would not be immediately transmitted at the other end, but only progressively, in proportion as the inertia of the successive sections of the prism and the friction against the supporting stand would be overcome, the one after the other. And defor-

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mation would begin at the end where the thrust P is applied, even before any compression would become noticeable at the other end, if the prism were long enough.

Let us now imagine a prism composed of a series of parallel, superposed, horizontal and homogeneous layers, individually uniform in thickness, but variable in composition and resistance, such as clayey, sandy and calcareous layers would be. Suppose such a prism submitted to a uniformly distributed load, representing the load of superincumbent strata; the friction against the stand reduced by the interposition of a soft base, and proper precautions taken to avoid lateral deformation. And let us subject this prism to the same process of compression. Here again the compression would be transmitted progressively from the point of applied pressure to the point of applied resistance, and deformation would begin at the extremity nearer to the thrust. This deformation may vary, according to the relative thickness and disposition of the "competent" and "incompetent" strata, as they have been termed, but would generally take the form of a local bend, some sort of anticline, which may be overthrown in the direction of the resistance. If now

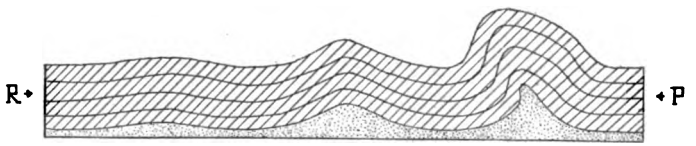

FIG. 5.

the pressure should be continued, waves would rise in succession, from right to left, decreasing in height, the farther they reach toward the resistance, and the flexed prism would finally take the general appearance of Fig. 5. The mechanical result would be: (1) A general compression of the prism, which would decrease from P to R, where it might disappear, and (2) deformations by bending, with appressed folds toward P and lower anticlines toward R; the folds being generally overthrown in the direction of the resistance R.

Now, we have already seen that when a material is compressible, compression will tend to draw nearer to each other the solid particles of which the material is composed. Compression would thus tend to a reduction of volume and to a reduction of porosity, or capacity for liquids. The first mechanical effect of our experiment, then, tends to reduce the capacity for liquids of the material at a decreasing rate from P to R, from the point of applied pressure to the point of applied resistance.

To this would be superimposed the mechanical effect due to flexure. Anticlines would be extended on their convex side and synclines compressed on their concave side. If, then, the thickness of the prism is sufficient, and if we consider only its upper portion, above the neutral

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plane, the general effect would be as follows: A general decrease of compression and a general increase of capacity for liquids from *P* to *R*, superimposed on a wavy succession of local increases and decreases of capacity. This would create a succession of zones of increasing capacity from right to left (from *P* to *R*), individually elongated in a transverse direction to the prism, or in a parallel direction to the folds. If then the porous strata were saturated with a liquid, the tendency would be for the liquid to move from the more compressed parts to the less compressed or decompressed ones. This movement would take place along the lines of lesser resistance; viz., (*a*) from the clayey layers to the sand strata, which would complete the concentration of the oil emulsion, already begun by the effect of vertical compression, into the sandy layers, and (*b*) along the sand strata themselves. Pools would be constituted parallel to the flexures, which would become more important the nearer they approached the resistance, or increasing from right to left.

Further, compression and consequent migration of the emulsion through the sand would have a tendency to allow oil drops and gas bubbles to coalesce. Temperature may play a part in the phenomenon, as pressure would generate heat. This heat may be negligible in the zone of reduced compression, but may be important in the highly compressed one. Rocks may afford sometimes a certain amount of heat without noticeable changes; but organic liquids would be more or less modified. A slight increase of temperature would lower the viscosity of the oil and facilitate its migration. A sufficient rise may partially decompose it, and complex reactions may occur if this decomposition should take place under pressure, in the presence of water vapor. This side of the question will have to be investigated more completely, should the general theory be admitted. For there could be found possibly a partial origin of the different grades of oil, and the source of a large part of the gas which accompanies oil in the pools today. The complexity of the products would still be increased by the filtration of the oil through certain rocks, which an excessive compression may allow, or which capillarity may induce in other circumstances.

At any rate, gas and oil concentration in the zones of decompression or of reduced compression would result; and water, oil, and gas may finally settle in the reservoirs according to density.

On the other hand, we have seen that the anticlines would be extended along their crests and the synclines contracted along their bottoms, if we consider only the part of the prism above its neutral plane. The liquid would thus be squeezed out from the synclines and would have a natural tendency to collect, by the effect of compression, in the higher and more open parts of the anticlines. Later, if, for some reason, water were reduced in amount, the oil would follow the level of the receding waters and sink toward the bottom of the synclines, as long as porosity would permit.

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It might occur, with a sufficient increase of the lateral thrust, that the material subjected to compression would break instead of bend, and that overthrusts and faults would result. These deformations would naturally take place in the more compressed region, on the side of the thrust. This would open a way of escape to the gas and to the liquids, and the affected parts of the oil-bearing strata might be deprived of their contents, by outflow or evaporation. The oil and gas would be lost and these parts of the layers would become dry.

As far as we have gone, we have considered theoretical views and results of laboratory experiments only. The material which we have been handling was simple, the individual layers homogeneous, of constant thickness and horizontally disposed; the forces were acting in the vertical plane of symmetry of the prism, etc. And from a starting point fraught with geometric simplicity we have reached simple and geometric results. The merit of this way of proceeding is that it has allowed us to understand more clearly than could be done otherwise, the trend of events when the elements of the problem are reduced to their simpler lines.

But in Nature, things are much more complicated. Let us consider, instead of a prism, a syncline of deposition and note the new set of conditions which may affect the result. Between the geometric dispositions of our previous scheme and the dispositions encountered in the field, we will remark at once the following differences:

The material will not be homogeneous, but varied to the extreme. Not only will the strata be different from one another in composition, texture, resistance, flexibility, etc., creating a lack of homogeneity in the vertical direction, but similar differences will be found in individual strata, so that the lack of homogeneity will become almost general.

The materials will not be geometrically disposed. The outlines of the compressed area, in horizontal projection, will be irregular, and its thickness at great variance from point to point. The layers will be incurved, presenting original bends of deposition. Their own thickness will be irregular, and the so-called "planes of stratification" will not be planes at all, but complicated surfaces.

If we consider one particular stratum, the pressure due to overlying formations will not be the same everywhere.

The thrust will be irregular, and to simple compression and flexion will be superimposed a torsional movement. Consecutive thrusts, varied in direction and importance may affect the same region. Further, an orogenic movement does not result simply in a series of more or less parallel folds, elongated in one direction; it is complicated by transverse or orthogonal undulations, which create successive elevated and depressed areas along the general strike of the folds; the anticlines have a tendency to be transformed into elongated domes, the synclines into elongated basins.

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Finally, breaks of greater or lesser magnitude frequently occur in the folded masses, and faults and overthrusts take place.

For these reasons, the resulting structure of a syncline subjected to orogenic movement becomes sometimes very intricate. Further, the superposition to the simpler effect of lateral compression of a series of incidental effects, derived from irregular forces which enter into play, would put the problem out of reach of mathematical treatment.

But there remains a way open to us. We may consider such a deformed syncline in nature, and try to ascertain how much the facts collected in the field may agree with the general lines of our theoretical views.

A good example of a folded chain may be found in the mountain system of the Appalachian province, which extends from New York to Alabama with a general northeast-southwest trend, and in the immediate neighborhood of which some of the most productive oil fields of the United States are situated. The thrust is supposed here to have taken place from the southeast. The whole Paleozoic series of strata to the floor of the crystalline Archean rocks—in some parts 40,000 ft. thick—have been involved in the system of flexure. The flexures are generally parallel to the main direction of the chain, and tend to be arranged *en échelons* in overlapping series. They are mostly unsymmetrical, *i.e.*, they bear a "rear and front structure," the steeper side generally facing northwest, away from the Atlantic Ocean. Bailey Willis, in his *Mechanics of the Appalachian Structure*, recognizes four districts in the Appalachian belt. The eastern portion of the system is a district of "close folding," extending nearly its entire length and terminated at the south by a district of "folding with cleavage." The western part of the belt, on the north, through central Pennsylvania and the Virginias, is a district of "open folding." South of it, and always on the western side of the belt, comes a district of "folding and faulting," which passes through eastern Kentucky and Tennessee to Alabama.

The district of "open folding" terminates, on the west, in large undulations of low dip and merges finally into the monoclinal or gently undulating structure of northwestern Pennsylvania and eastern Ohio.

Beyond the southern region of "folding and faulting," proceeding westward, comes the high plateau or tableland of Kentucky and Tennessee.

As a whole, and barring irregularities of detail, we find here the characteristics which have been outlined in our theoretical statement: Successive parallel folds, more closely appressed on the east (the side of the thrust) and decreasing in importance the farther we come west (toward the resistance), with a general tendency for an overthrow of the folds in the direction of the resistance.

All the oil and gas fields are located along the western side of this

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belt, on its outer margin; *i.e.*, on the side that is farther from the point of application of the thrust. From Pennsylvania down to Alabama, their main direction closely follows the direction of the chain, and individual pools are frequently disposed in rudely parallel rows, with their major axis parallel to the folds. This parallelism between the positions of the oil and gas areas and the trend of the mountains has long ago been pointed out, especially for western Pennsylvania.

Another remark is that the oil belt is especially developed in front of the region of "open folding," to the north, but rapidly decreases in importance as soon as it reaches the front of the region of "folding and faulting," to the south. The effect of faulting would be to relieve the strains and, in so doing, to reduce the amount of deformation farther west, as well as to open a way of escape to the fluids, if any. This double effect would reduce the tendency toward accumulation.

The Pennsylvanian oil belt offers a further interesting feature. When entering the State from the south, the mountain belt is at first bent eastward in its trend, and it takes again a northern course after crossing the Susquehanna. Fig. 6 represents this curve, which has the form of the letter *S*. Now, incurvations of this kind are frequently encountered in mountain chains, and they have been interpreted sometimes as the result of the resistance against orogenic movement due to an ancient mass of consolidated rocks (*Massif d'ancienne consolidation*) in the region *M*, acting as a pier which would deflect the direction of the thrust. The mechanical effect of such a deflection would be an increased pressure in the concave part of the curve, toward *A*, and a decompression in the region *B*, where the curve becomes convex. The fact that the principal oil areas of Pennsylvania and southwestern New York—which are among the most productive in existence—are located precisely in this zone of decompression and do not extend farther east in the compressed zone, is interesting.

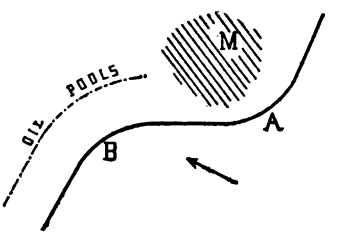

FIG. 6.

The same relations between orogenic deformations and oil-producing areas may be observed in other fields, as, for instance, in the oil fields of central Europe. The chain of the Alps, followed by the Carpathian Mountains, the Transylvanian Alps and the Balkans, are interpreted today as a system of folded chains. The belt develops from southern France to Galicia, Bukowina, and northern Roumania in the form of an arc, with its convexity directed northward. Its eastern extremity presents the well-known sigmoidal inflexion formed by the Transylvanian Mountains and the Balkans. The thrust has been directed from the inside of the curve toward the outside. Everywhere, the oil fields are

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located on the exterior slopes of the belt; *i.e.*, on the side that is farther from the point of application of the thrust, and the pools are distributed in rows parallel to the trend of the folds, frequently with their major axis in the same direction.

In these districts the deformations are no more the gentle anticlines and synclines or the terraces of the Appalachian region; the strata are highly disturbed, contorted and even faulted. The thrust in the Alpine belt has been much more powerful than in the Appalachian region, and in certain places overthrusts of considerable magnitude have taken place. In fact, the Alps proper and the Carpathians seem to have been submitted to folding and deformation at least at two different periods: First immediately after Oligocene times, and second during the middle Miocene; and the present state of affairs is the result of the cumulative effect of both thrusts. It seems reasonable to admit that the region has passed through a first stage similar in some respects to the one presented by the Appalachian belt, and during which the greater part of the accumulation may have been effected, before reaching the further stage of greater disturbance, in which a part only of the accumulated hydrocarbons seems to have been preserved, in consequence of special stratigraphic conditions.

Similar relations may be observed in many other fields.

The preceding remarks have led the writer to believe that the facts observed in the field agree with the theoretical views previously expressed, and the hypothesis which he proposes for oil and gas accumulation may be summarized as follows:

First Stage.—The oil proceeding from organic remains, perhaps still in process of decomposition at the very origin of the movement, is at first distributed in the water-laden sediments of the geosyncline of deposition in the state of disseminated particles.

Second Stage.—The increasing compression, due to the continuous accumulation of superimposed strata, expels an increasing amount of the water of deposition with its contents of hydrocarbons, from the original layers, which, at the same time, are the most easily affected by the compression (argillaceous or limy sediments), to some other layers less affected by it (especially sands and gravels). This displacement may take an upward or a downward trend, the only condition being that the fluids must move along the line of lesser resistance, from a more compressed to a less compressed zone. This movement may take place between two entirely different strata, or between two layers of the same group of deposits, provided the layer acting as a temporary reservoir is less compressible or more porous than the former one. (This latter condition obtains in the dolomitic layers of the Trenton rock.)

Third Stage.—As soon as orogenic movement begins, a more or less horizontal compression, due to the thrust, takes place and becomes added to the vertical pressure due to superincumbent weight. The result of the

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intervention of this new force is to create: first, a general increase of compression from the point of applied resistance to the point of applied lateral pressure; second, successive and parallel zones alternately compressed and decompressed, whose strike is normal to the direction of the thrust. The waters which saturate the strata are submitted to the effect of this unequal pressure and move from the highly compressed regions to the lesser compressed ones, carrying the hydrocarbons with them in their course, finally collecting in pools parallel to the folds. The movement would have to take place along the lines of lesser resistance, *i.e.*, toward and along the more porous layers of the formation (sandy layers, etc.). Pressure would reduce the viscosity of the oil, favor the coalescence of the globules and perhaps induce some chemical changes of the hydrocarbons. The more probable places of accumulation would be the crest of anticlines, the summit of domes, the rims of terraces, or, in the main, the places where a change occurs in the dip or along the strike of the strata, in the form of convex edges or arches; for the reason that at these places the local reduction of compression, buoyancy, and resistance to motion due to a change in the direction of flow, would act together and accumulate their effect.

When the fluids reach the zones of lesser compression, if the physical condition of the reservoir in which they collect is such as to hold the hydrocarbons and prevent their escape, an equilibrium is established, and the final pressure in the pools must be equal to the original pressure less the losses of head encountered on the way.

Further, a progressive settlement would take place in the reservoir, according to gravity; water would congregate at the lower places, oil would have a tendency to collect at its surface, and gas—either brought with the water or dissolved in the oil and further released by decompression, or simply produced from the oil itself—would reach the higher places.

Considered as a whole, the process would be a consequence of the mechanical principle of least action. In this process, the agent of transportation of the hydrocarbons would be water; the moving force would be hydraulic pressure created by vertical and lateral compression; and the extent of the movement would be variable in the extreme according to local conditions of the strata. But compression, especially compression due to the lateral thrust, whose action would be irregular and continuous and would have to be extended over a long period of time, would not act as the permanent head of water which nowadays is the ultimate source of the velocity and of the hydraulic power of circulating underground waters. Frictional resistance may reduce and even stop the motion of underground water once for all in a given direction; a thrust would act by a succession of jerks and repeat the effort again and again. There would be periods of activity, during which the compressive force would exceed the resistance of the rocks and deformations would

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ensue, followed by periods of rest, brought by the momentary relaxation of stresses due to deformation itself and during which the compressive forces would accumulate; and the thrust would become the source of a periodically renewed energy. The consequent hydraulic pressure would follow a similar wavy movement, with periods of maxima, to which a maximum of velocity would correspond, and periods of minima or of rest, where frictional resistance might bring the movement to a stop. The action of the liquids would thus become similar to that of a water ram, with a maximum of efficiency periodically renewed.[6]

Fourth Stage.—A new stage will be reached by the gradual reduction of the water contents of the strata, producing consequent changes in the level of complete saturation and in the local disposition of the pools, by gravity.

Fifth Stage.—Sometimes, a new period of folding may take place, in which the thrust may have or may not have the same direction as the previous one. New zones of compression and decompression may be created, and the liquids may be put again in motion. The results may become thus very intricate, especially if the strata are deformed to a large extent.

All gradations must be expected to be found in the oil fields between these two extreme sets of conditions. The Appalachian belt may represent the first set, which stops at the fourth stage of our description. The fields of central Europe would represent the further and more complex stage.

The research for oil becomes thus a problem of tectonic effects as well as of stratigraphy.

Rock Pressure

The origin of "rock pressure" has been traced to one of the following causes: Hydrostatic pressure, weight of superincumbent strata, gradual accumulation of the inclosed gas, capillary diffusion.

The hypothesis of hydrostatic pressure is untenable as a general one. Hydrostatic pressure would agree, to some extent, with the pressure found in Ohio and Indiana, but could not account for the heavy pressures encountered in western Pennsylvania, as shown by Prof. J. P. Lesley and J. F. Carll, nor for the pressure of most of the deep West Virginia wells. Further, this theory is contradicted by the well-known fact that flow and pressure are found to decrease in any given well with the age of the well. A certain amount of constancy in the flow and in the pressure ought to be expected under artesian action, which is not the case.

The weight of overlying strata, under *present* conditions of the rocks would be mechanically inadequate.

[6] Dynamical action of the water ram type seems to have played an important part in certain geologic phenomena; the liquid being an aqueous or a magmatic solution.

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The progressive accumulation of the gas may be a cause of pressure, and, according to David T. Day's experiments, capillary diffusion through fuller's earth bears a curious analogy with osmotic phenomena and the pressure due to this cause may be compared with osmotic pressure. But, if both theories may explain the origin of a certain pressure, they are not entirely satisfactory, in the writer's mind, for the following reasons: The progressive accumulation of the gas would rapidly be checked by the increasing pressure itself, and it has not been shown that such a limitation would not occur before reaching the high pressures encountered in some wells. On the other hand, the action of capillary diffusion seems to require certain physical conditions which are not met everywhere, and therefore it can not be admitted as a general cause. Further, none of the preceding theories explain the following facts: At least in the eastern fields of the United States, the rock pressure, in the main, increases with the depth of the "sand," and, at the same time, there seems to be a decrease of pressure with an increase of distance from the principal axis of the folding. The closed pressure in the Trenton limestone of Ohio and Indiana averages 200 to 300 lb. per square inch and only exceptionally reaches over 600 lb.; whereas the pressures in western Pennsylvania and in West Virginia, farther east, easily reach the double figure. In other words, it seems as if the pressure would increase, as a whole, in the same direction as the compression to which the rocks have been submitted at the time of their folding, both vertically and horizontally. A partial origin of the rock pressure, at least, would thus have to be traced to orogenic deformation. The two other forces—due to progressive gas increase and capillary diffusion—may have, and possibly have played a more or less important part in the final result, but this effect would have been produced later, and, in this respect, is to be considered as subordinate.

How Capillary Pressure Seals an Oil Pool

One of the most interesting problems involved in the study of oil and gas accumulation is the process by which gas or oil may accumulate in the pay streaks under heavy pressure, without this pressure being dissipated through neighboring rocks or through the sandstones. Imperviousness of the superincumbent strata and of the oil-bearing bed itself has been frequently advocated. But unaltered rocks of the type encountered in oil fields are never impervious. From 10 to 40 per cent. of their bulk is made of pore space, and the pore space of the inclosing beds of shales averages no less than 6 per cent., which means that every square foot of so-called impervious rocks contains an average of 8½ in. of holes (M. J. Munn).

In his "hydraulic theory," M. J. Munn has suggested[7] that "the

[7] The Anticlinal and Hydraulic Theories of Oil and Gas Accumulation, *Economic Geology*, vol. 4, No. 6, pp. 523 and 527.

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pools of oil and gas are held in place by water under hydraulic and capillary pressure which effectively seals up the pores of the surrounding rock, and prevents the dissipation of pressure by diffusion." "Pressure in pools," he writes, "is maintained by the expansive force exerted by gas. . . . Such gas could not diffuse because of the saturated conditions of the surrounding rocks." But Mr. Munn does not go further than a general statement, and no detailed explanation of the process by which such a "sealing up" is rendered possible has yet been given, as far as the writer knows. The following is proposed as a tentative explanation.

Hydraulic pressure is here discarded, this problem being a problem of statics rather than of dynamics, and in which the velocity is naught.

There is a peculiar and interesting phenomenon which appears to have escaped the notice of those who have tried to explain the "sealing up" of a pool. It has been pointed out by Jamin, a French physicist, and is illustrated by the experiment of "Jamin's tubes." If a capillary tube is incompletely filled by water and the water distributed through the tube in such a way as to constitute a string of droplets, a pressure may be applied to one of the extremities of the tube which will not be transmitted to the other end. In other words, the string of droplets will act as a resistance. If there is a large number of drops in the tube, the difference of pressure at the ends arising in this way may amount to several atmospheres. The explanation follows: A drop of liquid, will be limited on both ends by a meniscus (Fig. 7). The superficial tension which results in this form is caused by the tension of glass and air, glass and liquid, and air and liquid. The two first sets of forces are parallel to the axis of the tube, and, being equal and directly opposed two by two at both ends of the drop, neutralize each other when conditions of equilibrium are formulated. The third set of forces, caused by the tension of air and liquid, is tangent to the meniscus all around the tube, at both ends, as shown in the figure, and makes with the axis of the tube an angle θ which is the "angle of contact." This angle will have a finite value if the liquid is oil or ground water (which is not pure water, but a salt solution, in this case fouled with traces of oil). The weight of the drop may be neglected here, as its action is insignificant with regard to that of surface tension. When the pressure is equal at both ends of the drop, the meniscuses are identical, and so are the angles of contact θ and θ'. But if the pressure P at one end is increased, both meniscuses will alter their curvature. The meniscus in front of P will decrease its angle θ, which will tend toward zero, whereas the meniscus opposed to P' will increase its angle θ', which will tend toward 90°. The result of this deformation is to increase the force $T \cos \theta$ directed against P, and to decrease the force $T \cos \theta'$ directed with it. The dif-

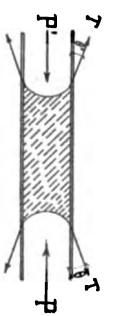

FIG. 7.

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erence will be a resistance against motion expressed by $T(\cos\theta - \cos\theta')$, which will draw closer to the limit T as the pressure is increased.

The same conditions would obtain in shales or clays capping a gas pool. There is nothing like a plane of separation between the gas of the pool and the water that fills the pores of the superincumbent rock; but there is a more or less irregular intermediate zone in which the gas and the water are commingled. The pores of the shale, by their juxtaposition, would constitute the Jamin tubes of the experiment, and these would be filled by the mixture, which would take the form of bubbles of gas intermingled with droplets of water.

Van Hise remarks[8] that "the majority of the particles of most clays, shales and slates are much smaller than 0.0012 mm., and therefore the openings of the rocks are subcapillary." He defines subcapillary openings as those which are 0.0002 mm. and less in diameter. Starting from these data, it is possible to calculate, with a sufficient degree of approximation, the thickness of the shales or of the clay that would seal up a given pressure. The writer has found that a few feet would be amply sufficient to seal up a pressure of 1,200 lb. per square inch.

It may be conceived that the same process would apply for the sealing of gas and oil pools laterally, along the dip and strike of the porous layers themselves, wherever the sealing is not already produced by changes in the nature of the "sand" from a pervious to an impervious one, or by the presence of water. Pore openings of sandstones are, for the most part, capillary; and should water not be present in the sand, oil may replace it for the Jamin tube effect. Calculation shows that the marginal zone thus constituted may reach a width of a few hundred feet. Pressure would decrease in this zone from the inside to the outside of the pool, progressively, a feature which is readily observed in the field.

Discussion

Eugène Coste, Calgary, Alberta (communication to the Secretary*).—This new theory to account for the accumulation of commercial deposits of oil and gas, is deliberately and admittedly based on the hypothesis that the origin of these products is organic. Mr. Daly says (page 738), "Should the organic origin of the petroleum that is found in pools be granted, the following interpretation is offered for the mechanics of its accumulation." But suppose the origin thus accepted by definition be incorrect, then what becomes of the diastrophic theory, Mr. Daly's interpretation? As Mr. Daly remarks himself (same page), "It is obvious that the history of petroleum will have to be entirely different, dependent

[8] A Treatise on Metamorphism, *Monograph* 47, *U. S. Geological Survey*, p. 138 (1904).

* Received July 31, 1916.

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upon its origin, either from emanations coming from the depths, or from organic decomposition in the strata themselves." Then why should one propound a theory, through which he is endeavoring to trace the history of petroleum, by starting with a definition of its origin? The origin should be the deduction, the forced conclusion of the theory, and not its starting point. Facts alone should be considered first, in any theory worth the name, until sufficient proofs have accumulated to permit of the deduction of a complete explanation for these facts. This would lead one to the origin through a real theory founded on facts, and not based on a preconceived idea perhaps true, but possibly fanciful. Mr. Daly's way can only lead on one road, namely the organic road, and, I repeat, it may be the wrong road.

In the case of the petroleum deposits, as I have endeavored to show in several papers on the subject, to which the reader may refer,[1] the facts clearly lead to the forced conclusion that their origin is not one of accumulations of hydrocarbons at first disseminated in the sedimentary strata but the reverse process of subsequent local infiltrations and impregnations of hydrocarbon emanations from the depths. This new theory, therefore, founded on an erroneous conception of origin cannot be a help in the solution of the petroleum problem, not any more than were the anticlinal theory or the hydraulic theory, both of which were also based on the same error, although Mr. Daly is inclined to think that the authors of these two theories did not accept the misconception boldly enough (page 738).

A careful reading of Mr. Daly's paper shows plainly the consequences of a wrong start, as it leads him to the following fallacies:

1. That oil and gas are (first phase of Mr. Daly's process) primary decomposed products of organic matter formed in the sediments shortly after their deposition at ordinary low temperatures, and held by them for a short period of time, until squeezed up into a porous rock deposited above just at the right time to receive them. This is a very different conception from the view held by other organists (geologists believing in the organic origin of petroleums) who hold, on the contrary, that only a very long time (aeons of ages) will cause the distillations of organic matter in the strata—this immensely long time finally accomplishing the same result as heat in ordinary distillation processes. But if Mr. Daly's view be correct, deposits of that kind should be frequent in nature, and yet no one has ever been able to observe them. As a matter of fact they do not and never did exist, as organic matter decomposes into entirely

[1] *Journal of the Canadian Mining Institute*, vol. 3, pp. 68–89 (1900), vol. 6, pp. 73–128 (1903) and vol. 12, pp. 273–302 (1909).

Trans., vol. 35, pp. 288–297 (1905) and vol. 48, pp. 504–517 (1914).

Transactions of the Institution of Mining and Metallurgy, vol. 21, pp. 91–192 (1911–1912).

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different products which either escape immediately out of the forming sediments, or finally transform into carbonaceous matter or coal without any other hydrocarbon but marsh gas being produced.

2. That porous rocks shortly after their formation (second phase of Mr. Daly's process) may be supposed to be saturated with an emulsion of oil and gas in water, the oil and gas having been squeezed out of the muds below into these porous rocks, and remaining there in some mysterious way, instead of continuing their migration to the surface. Again I will observe that we do not see anything of the kind in nature—wells near the sea shore often striking pure fresh water (entirely devoid of oil or gas and even of salt water) in the recently formed sands or porous rocks beneath.

3. That the supposed gas and aqueous contents of incompressible strata or layers (page 741 line 12) which according to Mr. Daly could not previously be compressed out to the surface only a few feet or a few hundred feet away, are now (third phase of Mr. Daly's process) compressed laterally for miles and miles to finally accumulate into far-away oil and gas pools. Apart from the fact that these porous layers cannot be both at the same time compressible and incompressible, we know that porous sand layers form in the sediments disconnected lenticular beds seldom continuous over large areas; this would certainly stop the ingenious long lateral migration of Mr. Daly, and would send the oil and gas out to the surface and so would the faulting and fissuring in the highly disturbed portions of the mountain chain considered through which the oil and gas are supposed to migrate laterally. One certainly cannot admit that the shales covering the sands which, according to Mr. Daly's first phase of the process, could give out so easily their hydrocarbon contents have now become so impervious over distances of many miles, as to make possible this long lateral migration through the sands preventing all the while during this long travel the vertical escape to the surface only a few hundred feet away.

Mr. Daly says that "a partial origin of the rock pressure would thus have to be traced to orogenic deformation." This is rather vague and is only based on the remark that the pressure in Ohio and in Indiana, in the Trenton Limestone wells, was much smaller than in Pennsylvania and West Virginia; but we must not forget that the depths at which this gas was found in Ohio and Indiana were also much less than in the wells where the double pressure was recorded in Pennsylvania and West Virginia. As a matter of fact, the gas pressures in all fields are principally function of the depth indicating plainly the source from the volcanic magma below.

Mr. Daly's explanation of how capillary pressure seals an oil pool is ingenious, but may I ask why it only acts to prevent the pressure from dissipating outward into the surrounding sediments and why it did not

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also prevent the reverse movement of the accumulation of the gas and oil inward from the surrounding sediments into the sands?

F. G. CLAPP, New York, N. Y. (communication to the Secretary.*)—The points of excellence in Mr. Daly's paper entitled "The Diastrophic Theory" are so numerous that it is unfortunate that he seems to have based his paper on a misunderstanding of the structural theory, as ordinarily understood. According to Mr. Daly, "the force which is supposed to have caused the motion is the gravity of the hydrocarbons." And again "Gravity or buoyancy is to be considered the sole agency through which accumulation has been brought about and, as such, is supposed to be adequate to explain accumulation under any condition of dip." So far as I know, no petroleum geologist believes these statements; so that Mr. Daly's paper must not be taken as a criticism of existing theories, but of his conception of them.

So far as I know, no petroleum geologist supposes the *motion* of the oil, gas and water in oil fields to have been caused by the force of gravity. To quote from an early issue of *Economic Geology*:[1]

"The 'anticlinal theory' is only one of the factors in the accumulation of oil and gas pools; and a geologist, in order to locate a pool, even approximately, has to consider every other particle of evidence found in structure of the subsurface rocks, changes in intervals, texture of the 'sands,' character of the overlying beds, differences in pressure, relations to water in the rocks, shape of the surrounding pools, and character of the oil in the vicinity; and every factor must be given its due weight, before a recommendation can safely be made. After all other factors have been considered, it might seem as if the 'anticlinal theory' has been lost sight of as unimportant; yet the fact remains that structure is the essential condition which finally determines the distribution and size of the pools when all other conditions are favorable."

Passing this technicality, however, we can wisely weigh Mr. Daly's remarks that oil fields exist only on the side of mountain ranges *away* from the source of pressure. Certainly this appears to be applicable in the case of the Appalachian fields, and might well explain several vexing questions; as, for instance, the absence of oil fields east of Pittsburgh, Pa., west of which they are so numerous. In this connection, it is important to raise several questions, as, for instance:

1. Why is natural gas found east of Pittsburgh in abundance, while oil is generally absent?

2. Why is gas found on the *inside* of the Transylvanian Basin in Hungary, while oil seems limited to the outside regions, in Roumania and Galicia?

3. How will this theory explain the presence of salt water in large quantities, in the regions away from the source of pressure?

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* Received Aug. 28, 1916.

[1] F. G. Clapp's Discussion of paper of M. J. Munn, March, 1909, *Economic Geology*, vol. 4, No. 6, pp. 565–570 (September–October, 1909).

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4. How about oil associated with faults, as in Oklahoma, California and Wyoming?

5. Can the diastrophic theory be effective in dips of ½° to 5°, in regions several hundred miles from any mountain belt or major axis?

While the theory as propounded may account for one cause of the movement of petroleum in certain directions, the question of whether this is the *main* cause of accumulation must depend somewhat on the answers to the above questions. In my opinion, they can not all be answered favorably to the theory. Of course, however, a mere theory of cause or origin can avail little in petroleum engineering. It is where the oil is found that counts. We know that it exists in definite types of structure, when these are affected by certain other modifying conditions; but that these same structures, with a different set of conditions, will hold no oil. Successful oil-location depends preëminently on *inference—i.e.*, a comparison of conditions in prospective fields with those in fields already known elsewhere in the world.

R. N. Pack, Washington, D. C. (communication to the Secretary*).—Mr. Daly's exposure of the frailties of the commonly accepted theories as to the mode of accumulation of oil is more acceptable than are most criticisms of this type, for after knocking our theories down over our eyes he does not leave us to grope blindly, but leads us to a brand new theory, both plausible and persuasive, that is to take the place of our old friends. Yet Mr. Daly's theory seems to be heir to some of the weaknesses characteristic of our old theories.

Most of the arguments that Mr. Daly raises against the anticlinal and hydraulic theories have been raised at one time or another by one writer or another, and there are probably now few geologists who will seriously contend that the force of gravity alone is adequate to accomplish widespread lateral migration of oil and gas, and the local accumulation of those materials in commercial amounts.

As Mr. Daly sees it, the chief failure of the anticlinal and hydraulic theories is that they do not take into consideration the effect upon the oil of the diastrophic forces that have affected the region. But most of the recent discussions of the origin of petroleum have laid stress upon the importance of these very forces, not, however, their importance as transporting agents, but as transforming agents, in changing the organic débris in the sedimentary beds into oil and gas. Mr. Daly tacitly assumes either that oil exists in the loose muds of the sea bottom practically from the moment of their deposition, or that it is formed soon afterward, during the period of compression due to sedimentation. In any case he appears to believe that oil exists in the sedimentary beds before they are deformed. However, until some well-authenticated

* Received Sept. 16, 1916.

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examples of the formation of oil in unconsolidated sediments is brought forward, it would appear that the assumption that oil is formed in this manner is a less reasonable one to make than that oil is formed in part at least by the forces that caused deformation and perhaps local metamorphism. Of course it is possible that these forces not only have aided in the formation of oil, but also in its movement from one place to another; but it seems very unwise not to consider the first possibility at all.

Mr. Daly's discussion of the effect produced upon a prism of solid materials by pressure applied at the ends is interesting. After describing the purely theoretical example he applies the general principles involved to a succession of sedimentary beds, but in doing so he immediately agrees to consider only the upper part of the prism, that is, the part above the "neutral plane." One is led to wonder as to the depth this "neutral plane" lies beneath the surface, and just what the conditions are below it. Are the anticlinal axes the zones of contraction and the synclinal axes the zones of extension below this plane? If so, would oil, gas, and water tend to accumulate below the neutral plane in synclines rather than in anticlines? In the southern coast ranges of California many of the folds are shallow, and the "neutral plane" would in many cases certainly not be so very many hundred feet below the surface; so the question as to the accumulation of oil in them is not quite so academic as one might imagine.

According to Mr. Daly's theories, anticlines are important, so far as the accumulation of oil is concerned, only as they mark zones where the porosity is greater than in the surrounding region. In many of the California fields the folds are of variable character. Some are sharp and closely compressed; others broad and open. Between the two extremes almost any intermediate type may be found. It would seem natural to assume that along the more sharply flexed anticlines, the beds would be more fractured, or at least more extended, as Mr. Daly would say, and that they would thus determine the position of the more porous zones. Yet it is very frequently the case that it is not along these sharper folds that the oil accumulation is greatest at present, but in the upper parts of the larger, broader, and more gentle folds. Moreover, in the California fields the beds are so variable—so tremendously variable—that the differences in porosity due to lithology must be far greater than differences in porosity due to position along the folds; certainly the large broad folds can not have so altered the porosity of the bed that lithology will not still be the controlling feature. Yet in spite of this, the California fields offer a most splendid example of the occurrence of oil along anticlinal folds or in parts of the region where those folds dominate the structure. It would seem that there must be some other reason than increased porosity that causes the oil so to seek these structures.

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If the forces that have caused the deformation of the region are the forces that have caused the migration of the oil and built up the pressure under which the oil and gas are now confined, one would expect that this rock pressure would be fairly constant over considerable areas. The field of action of these forces has been extensive, and, if we accept Mr. Daly's theory, these forces have swept the fluids from a wide area, segregated them, and collected them in out of the way corners. It seems hardly reasonable to suppose that if the rock pressure is due to such a general cause the fluid in one little corner would have a certain rock pressure, while that in a neighboring corner would be under another pressure that is radically different. Nor does it seem probable that within one little pool the pressure would vary greatly; yet in the California fields variation in pressure in a given stratum seems to be the rule rather than the exception. This variability may of course be explained in part by variation in lithology and other natural features, and also by artificial features, such as the mode of handling a well. It is not always the later wells that show the lesser pressure, so the variation can not be explained away completely as a decrease for the pool due to release by some wells. But even considering the factors that make for local variation, it seems that the differences found between wells only a few hundred feet apart are abnormally large.

Mr. Daly's discussion of the method by which an oil pool may be sealed is interesting indeed, yet it would seem that the very factors that he calls upon to seal up an oil pool and to maintain a given rock pressure might equally well be called upon to prevent migration originally. If a given force causes the oil to move through the rock pores and collect under pressure at a given place, and if the force ceases to be active, why will not the oil move back over the path it originally traveled?

According to Mr. Daly's theory the forces causing the oil and gas to migrate also caused the water to move. Indeed, he considers that the water moved carrying the hydrocarbons with it. Water, then, should be under the same pressure that the oil is under, as the force causing accumulation is the same, and the factors preventing dissipation the same for water as for oil. Yet in the California fields where water sands and oil sands are interstratified, no pressure comparable with the pressure in the oil sands is recorded for the water sands which lie between these oil sands unless a distinct flow of petroleum gas is noted with the water.

Mr. Daly notes the fact that in the Eastern fields there is an increase in rock pressure with the increase in age of the strata in which the oil or gas is found. The same can hardly be said of the California fields, for the greatest pressures noted occur in the younger beds which rest unconformably upon the shales in which the oil originated. The sands

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that are distinctly stratified with these shales contain oil under a much lower pressure.

In discussing rock pressure Mr. Daly points out that the chief explanations heretofore offered have been (1) that it is due either to hydrostatic pressure, (2) weight of superincumbent strata, (3) gradual accumulation of the inclosed gas, (4) capillary diffusion. He shows that some of these explanations are untenable, for the forces called upon are clearly not competent to have built up the high pressure now encountered. He therefore assumes that much of this pressure is due to the forces that have caused the deformation of the region.

An explanation that in many ways seems to be more reasonable is that the rock pressure is built up within the reservoir, not by the gradual concentration of gas, but by the change of some of the oil into gas. Also, perhaps by the same, perhaps by other reactions, heavy, viscous hydrocarbons are formed that so clog the rock pores that the movement through them of even the gaseous hydrocarbons is prohibited and the reservoir becomes sealed.

Rock pressure so developed would then have no direct relation to the pressure that originally caused the migration of the oil and it might be greater or less than that original pressure. Rock pressure need not be "fossil pressure," as one would be tempted to call it if one accepted Mr. Daly's interpretation.

Just what the reactions would be that would cause the formation of the gases within the reservoir can not be said, but as to those which result in the formation of the heavy viscous material there are a few more data. It is a pretty well known fact that, in the California fields, the highly mineralized water characteristic of the oil fields has a very pronounced effect upon the oil, and that where such water has entered the oil sands the oil in the vicinity is extremely heavy and tarry. This tarry material is in some places so viscous that it can not be pumped. Such material seems to be composed, in part at least, of oxygen and sulphur compounds and they seem to have been formed by the interaction of the hydrocarbons and the mineral salts in the water. This tarry material is quite competent to, and probably does, fill the rock pores and prevent the passage through them of the more fluid hydrocarbons.

MARCEL R. DALY (communication to the Secretary†).—The fundamental disagreement between Eugene Coste and the writer proceeds essentially from the contradictory views they hold on the origin of petroleum. This difference cannot be bridged, nor can inorganic and organic theories be discussed here. The writer has suggested that the proper thing in such a case would be to agree to disagree.

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† Received Feb. 21, 1917.

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Coming to the detailed objections further presented by Mr. Coste, the writer feels that, in the main, they take their source in a wrong interpretation of the theory, and the writer is sometimes too generously invested with views that are not his own.

1. The only requirement for the theory is that the oil proceeding from organic remains be distributed in a water-laden sediment, and that this sediment be submitted to the increasing compression due to the continuous accumulation of superimposed strata, which would tend to squeeze out the liquid contents (p. 748). This squeezing out would be further aided by the distortion of the strata (p. 744, *a*). Nothing here makes impossible the slow formation of the oil, if this is considered necessary, as the piling up of the strata in the syncline would require by itself a protracted length of time, and, further, the squeezing-out process is a slow one. Possibly the formation of the oil may even continue during part of the process of distortion. But the mode of formation of the oil and the time and temperature required for it are left to the chemist to decide. The diastrophic theory, which is essentially a theory of motion and accumulation dependent on the laws of mechanics, is not directly concerned with them, provided the general points heretofore outlined be agreed upon. Perhaps it would be wise not to claim too many "eons" for the formation of the hydrocarbons, if the statement made by Sir Boverton Redwood is correct that "the coral reefs of the Red Sea are charged in places with recent petroleum, formed in that torrid climate from the swarming organisms occupying the shallow pools."[1]

2. There is nothing mysterious in the tendency of the fluids, squeezed out from their original and heavily compressed layers, to congregate in some more porous or less compressed rock. This is a simple mechanical effect, from which there is no escape. A mechanical system reaches a state of equilibrium only when its potential energy reaches a minimum. It is for this same reason that water runs down hill, collects in bottoms, and does not run up hill again when so collected. Further, the squeezing out of the oil from the clayey layers becomes possible only by the deformation of the oil globules when compressed between the very fine particles of the clay; whereas in sandy layers this deformation would not take place to the same extent, on account of the greater volume of the grains and the corresponding enlargement of pore spaces. Here, the oil globules would return to their spherical form, by reason of surface tension, and would become trapped between the grains. A certain amount of the water contained in the sand layers may be replaced by the liquid containing the hydrocarbons. This portion would escape either through the overlying strata or laterally; water, on account of its lower viscosity and higher capillarity, being able to filter through where oil could not.

[1] *Petroleum* vol. 1, pp. 133–134 (1913).

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The theory has never claimed that every sand would contain oil, nor that every mud is the parent matter from which oil proceeds.

3. I think that most of the objections contained in this paragraph would have been omitted if the critic had followed more closely the description of the process as stated by the writer. For instance, when it comes to oil migration, why does Mr. Coste hold the writer responsible for a transfer of the oil through "miles and miles" to "far-away oil and gas pools" under conditions admittedly adverse to motion, when the writer has specifically declared that "the extent of the movement would be variable in the extreme according to local conditions of the strata" (p. 749). In the same way, the writer has never claimed any state of incompressibility for the strata under consideration, and, above all, any general and absolute incompressibility for any stratum. Compressibility is essentially relative, and the whole theory is based on the production of zones of unequal compression. The writer has simply said that a layer of sand may become incompressible when it still contains a great percentage of holes and may thus act as a reservoir even under considerable pressure, which is a fact (p. 741). Further, Mr. Coste seems to have lost sight in his discussion of a well-known mechanical principle, which may be expressed as follows: When a material body is distorted by the application of two external forces, different in magnitude and direction, if one of the forces enters first alone into action and then both forces act together, the strains resulting from the application of the first force will be modified by the intervention of the second. This may be termed a superposition of effects. In such a case, an area which is compressed under the action of the first force may become decompressed and even stretched when the second enters into play. An illustration of this is a vertical column, first subjected to a load which compresses it, and later to a horizontal thrust which would bend and break it. In the same way, strata may be compressed vertically at a first stage, and later submitted to a horizontal thrust which would throw them into folds, changing entirely the distribution and the nature of the strains, and even opening fissures at the summit of the arches, if the superincumbent load is not large enough. If there is any fallacy in this case, it seems to belong to the commentator and not to the author of the theory.

The last sentence of paragraph 3 in Mr. Coste's discussion raises an interesting question. As the same question is asked by Mr. Pack, both will be answered together.

The writer concedes readily the difference between the original anticlinal theory and the structural theory propounded by F. G. Clapp, in which "the anticlinal theory is only one of the factors in the accumulation of oil and gas pools," which may become even "unimportant;" and he wishes it to be understood that his objections to the "anticlinal

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or structural theory" are exclusively limited to the "anticlinal" part of it.

The interpretation of the anticlinal theory itself given by the writer is not a personal conception, but an abstract of the interpretation of this theory given by W. T. Griswold,[2] in *U. S. Geological Survey Bulletin* 318, where the movement of oil in a porous stratum, completely saturated with water, is explicitly ascribed to buoyancy.[3] This conception has been severely criticised since by Malcolm J. Munn,[4] who seems to imply that there is yet something to disagree about in this direction. Although the writer is aware that this explanation of the motion of oil is losing ground every day, it is no less a fact that its influence is still felt in many text-books and papers of recent date. Expressions like these are frequently to be encountered: "If the rocks are thoroughly saturated, there will be a general migration of the globules of oil upward through the strata."[5] Or: "Petroleum . . . persistently climbs the slope of inclined water-containing strata."[6] Or again: "It is to be expected that in saturated rocks, petroleum, a liquid lighter than water, will rise to the highest parts of its container, the oil sand."[7] Quotations of this kind may be multiplied. They evidently imply a motion of the oil due to buoyancy; so that the old interpretation seems to remain in force at least in certain quarters.

It is the writer's opinion that the buoyancy of the oil cannot be interpreted as a direct source of motion of the oil on any reduced scale more than it can be on any large one. *Buoyancy simply does not move the oil.* It does not move the oil more than the weight of a stone moves the stone when transported by a current of water. It may *deflect* the motion imparted to the oil by hydraulic action, and, in this respect, its importance is enormous; but it stops there.

This deflective action would be adequate to explain mechanically such phenomena as the segregation of the oil globules during the process

[2] W. T. Griswold and M. J. Munn: Geology of Oil and Gas Fields in Steubenville, Burgettstown and Claysville Quadrangles, Ohio, West Virginia and Pennsylvania. *U. S. Geological Survey Bulletin* 318 (1907).

[3] W. T. Griswold writes as follows: "Oil and gas entering a porous rock that is completely saturated with water, will be forced up to the top of the porous stratum by the difference of the specific gravity of the hydrocarbons and the water. Here the oil and gas will remain if the porous stratum be perfectly level; but if it has a dip sufficient to overcome the friction, the particles of oil and gas will gradually move up this slope, the gas with its lower specific gravity occupying the higher places" (p. 14).

[4] M. J. Munn: Anticlinal and Hydraulic Theories of Oil and Gas Accumulation. *Economic Geology*, vol. 4, p. 509 (1909),; and more recently *U. S. Geological Survey Bulletin* 547 (1914), Reconnaissance of the Grandfield District, Oklahoma.

[5] C. T. Lupton: Oil and Gas in the Western Part of the Olympic Peninsula, Washington. *U. S. Geological Survey Bulletin* 581-B (1914), p. 79.

[6] K. C. Heald: Oil and Gas Geology of the Foraker Quadrangle, Osage County, Oklahoma. *U. S. Geological Survey Bulletin* 641 (1916), pp. 41–42.

[7] *Geological Survey of Canada, Memoir* 88 (1916), p. 169.

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of migration, and the accumulation of the oil in places where the velocity of the current would be reduced, such as in convex arches along the dip or the strike of the sand (see latter paragraph), etc. But buoyancy without a current would not bring the result.

The writer is also of the opinion that, barring exceptional conditions, the velocity of ground waters would not be sufficient to overcome the resistances offered to the motion of the oil (p. 737). So that the motive force would have to be looked for elsewhere, and diastrophism seems to remain the only possible source to which we may apply.

Capillary action may promote the concentration of oil and gas in the sands, as explained by C. W. Washburne[8]; but the writer is not prepared to admit that it is the sole agent of such a concentration, and still less that it is the controlling factor of migration and accumulation of the hydrocarbons in pools. Its effects are related to the lithological character of the rocks and not to their structure; whereas everything points to an intimate relationship between oil or gas accumulation and structure. Further, capillary action would not account for rock pressure.

Coming back to the structural theory, the practical value of which the writer fully recognizes, we may ask: Is it a *theory* in the proper sense of the word, or is it not rather a statement of facts and conditions and a method for working out these statements in a practical way? A theory ought to be the philosophical explanation of the ascertained phenomena, thereby linking these phenomena together by some general principle. Where is the general principle to which the motion of oil and gas through the strata, their concentration and final accumulation in pools may be ascribed? It is precisely this "missing link" that the writer has endeavored to define. Whether his hypothesis, which is essentially a mechanical one, may be accepted, rejected, modified or perfected, remains an open question, and the criticism of men of the experience and knowledge of Mr. Clapp may do much to bring a final conclusion. In the mind of the writer, the merit of the diastrophic theory—if any—does not consist so much in the theory itself, but in the direction of research it opens to future investigators. The writer feels that the importance of mechanics in the interpretation of geologic phenomena cannot be overestimated. After all, everything in Nature obeys the laws of mechanics, and most geologic phenomena are nothing else than the expression of these laws.

The practical usefulness of any general theory that would definitely establish the laws that govern the motion and accumulation of the hydrocarbons is obvious. Today, using Mr. Clapp's expression, "successful oil location depends preëminently on inference." But an inference is a hypothetical induction built on the comparison of ascertained facts in every specific instance. Undoubtedly, an ascertained law would be a

[8] *Trans.*, vol. 50, p. 829 (1914).

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time and money saver in the field; when the true causes of a phenomenon are fully understood, it becomes less difficult to anticipate the conditions under which it may be repeated and to determine on the ground the places where such conditions are to be found.

Coming to the questions asked by Mr. Clapp, and numbered 1 to 5:

Questions 1, 2 *and* 3 may be answered together, as the facts stated would be derived, under the diastrophic theory, from the same cause. Without going into details, the writer wishes to say that he has been brought to foresee the following general law: "Whenever an oil-bearing region is folded by a simple dominant thrust or repeated thrusts in the same direction (Appalachians, Carpathians), the fluids inclosed in the strata, and which are put in motion, will have a tendency to collect ahead of the line of application of the thrust, and to segregate in successive deposits of increasing density, or decreasing buoyancy, with the increase of distance from this line." This implies that gas would tend to accumulate nearest to the line of thrust; oil, farther from it; and that water may reach still farther away.

The first part of this law would result from the application of a principle which has been called upon several times in this paper; *viz.*, when a mechanical system is in equilibrium, its potential energy is a minimum. So that fluids will have a tendency to migrate from the zones of higher compression—or of higher potential—toward the zones of lower compression—or of lower potential—*viz.*, away from the source of pressure.

The second part of the law would be a consequence of the same principles which regulate the deposition of bodies heavier than water in a current of decreasing velocity.

Let us consider the simple case of a horizontal current of water, in an inclosed channel, provided with a steady motion and a progressively decreasing velocity from A to B (Fig. 1); the decrease of velocity being due to a progressive increase of the width of the channel. Let us imagine that several bodies heavier than water, of varying densities but individually of the same shape and volume, are at first collected in A. The motion of the water will tend to carry them downstream, toward B. The transporting power of the water will depend on its kinetic energy ($\frac{1}{2}mv^2$), or its capacity to do work; and as same volumes and shapes are considered, and as the mass of the water per unit volume remains constant, this energy will vary only with the velocity of the current.[9] If

[9] Du Buat has shown that when a body is at rest in a moving fluid, if the motion is steady and the flow takes place in a pipe, the pressure R exerted by the water against the body may be expressed by $R = M\frac{mv^2}{2}S$, in which v is the undisturbed velocity of the stream, m the mass of the water per unit volume, S the projected area of the body on a plane normal to the direction of flow, and M a coefficient depending on the shape of the body and its relative proportions with the transverse section of the pipe (M. Bresse, *Cours de Mécanique Appliquée* 2e P.-*Hydraulique*, p. 429). According

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the velocity increases, so will the transporting power; and, conversely, a reduction of velocity will mean a decrease of the power of transportation. Now, the specific gravity of the materials has a marked effect upon the mean velocity necessary to move them. Bodies heavier than water, like stones, would move along the bottom of the channel by rolling, sliding, or more generally by a succession of leaps; *viz.*, rising from the bottom and returning to it after having described a curve in the water. It is essentially gravity which causes the resistance through which the kinetic energy imparted to the materials by the water is thus spent in friction or collisions. So the greater the gravity, the greater the requisite mean velocity of the current.[10] The result is that to each material there corresponds a minimum velocity under which transportation is no more possible, and that each material will be dropped at the point where the corresponding minimum is reached. A mechanical segregation will ensue, whereby the respective distances of transportation, from the point of origin to the point of deposition, will increase as the specific gravity of the body decreases.

FIG. 1.

Let us repeat the same process with bodies of varying shape and volume, as well as of varying gravity. For each series of bodies of same gravity, the increase of volume will cause a corresponding increase of resistance to motion;[11] and the mean velocity required for the motion will

to Lord Rayleigh's theory (*Philosophical Magazine*, December, 1876), if a plate with parallel edges be held in a stream normally to the direction of flow, the mean unbalanced pressure on the body is $= 0.88 \frac{mv^2}{2} S$, where the letters have the same signification as above. Both formulæ show that the power of transportation of a current of water is, at the same time, a function of the kinetic energy of the water ($\frac{1}{2}mv^2$) and of the shape and volume of the body transported (S). If these last two factors are supposed to be the same for the different bodies considered, as m is a constant, the intensity of the force of motion will simply vary, in each case, with the square of the velocity of the current.

[10] Chailly gives the following formula for finding the velocity required to move rounded stones or shingle: $v = 5.67\sqrt{a \cdot g}$ where v is the velocity of water in feet per second; a the average diameter in feet of the body to be moved; and g its specific gravity. The velocities required to move such bodies of the same shape and volume, but of varying densities, would thus vary as the root of their specific gravities.

[11] Du Buat's formula shows that the pressure exerted by the moving water against a body at rest and immersed in the current, is proportional to the projected area of the body on a plane normal to the direction of flow. For a spherical body, the projected area would vary as the square of the radius of the sphere and the weight as its third power. Weight, hence resistance to motion, in the case of bodies heavier than water, would increase more rapidly than the projected area, hence than the motive force. As solid bodies transported by a current are more or less rounded after a short time, the same relation is approximately true for them, if we consider their mean radius.

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increase with the volume. The bodies will thus be deposited at distances increasing as their volume decreases. There will be, not a *point* of deposition for the bodies of like gravity, but a more or less extended *zone;* and the zones of deposition of the series of different gravities may encroach on each other.

Experience agrees with this interpretation. For instance, the principle is used in ordinary gold washing, or for the assortment of sands of like size but varying specific gravity, etc. Worded as it is, this interpretation applies only to bodies heavier than water; but it may be shown that, with a simple modification of terms, it may be extended to bodies lighter than water; or, in other words, that the same law regulates the order of deposition of materials of any gravity in a steady current of decreasing velocity flowing through an inclosed channel.

We may remark that the action of gravity which creates a resistance against motion is not really represented by the specific gravity of the material (ratio of its weight to that of an equal volume of water), but by the difference between this specific gravity and the specific gravity of the water; for a body immersed in a fluid suffers a loss of weight which is equal to the weight of fluid it has displaced. If δ be the specific gravity of the body, and if the specific gravity of water is taken as a unit, the resistance due to gravity is a function of $(\delta - 1)$ and not of δ. In the C.G.S. system $(\delta - 1)$ would express a density. For lack of a better expression we may term it the "density in water" of the body. Now, this particular density may have a positive or a negative value, depending upon whether δ is greater or smaller than unity. If positive, the action of gravity will be directed down; this is the case of bodies heavier than water. If negative, the direction will be up, and the "density in water" becomes the buoyancy of the body per unit volume. In either case, the resistance to motion may be explained along the same lines. But the bodies will either drop to the bottom of the channel or be raised against its roof, depending upon whether δ is greater or smaller than unity. It may be remarked further that the absolute value of $(\delta - 1)$, when $\delta < 1$, decreases when δ increases; or, in other terms, that the numerical value of buoyancy decreases when the density of the body, that is lighter than water, increases; and the buoyancy of a body having the same density as water would be equal to zero. A general law may then be expressed, which would be applicable to all materials, whatever their gravity: "When a flow of water takes place in an inclosed horizontal channel, with a steady motion and a decreasing velocity from one end to the other, material bodies of varying specific gravity, shape and volume, will have a tendency to segregate and to be deposited at distances from the point of origin which will increase as their respective 'densities in water' $(\delta - 1)$ and their respective volumes decrease."

Applying this law to oil and gas transported in a current of water

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under the conditions stated above, we find that: (*a*) oil would have a tendency to segregate from the gas; (*b*) it would have a tendency to be transported and deposited farther than the gas from the point of origin.

But there are several factors that have not been taken into account in the preceding simplified theoretical instance. The chief ones may be enumerated as follows: (1) The motion will not take place in an inclosed channel, but through many irregular and capillary channels, and the resistances to motion will vary, not only according to density, shape and volume, but also according to the individual nature of the bodies transported (viscosity, surface tension, etc.) and the local texture and porosity of the "sand." (2) The strata through which the flow takes place will not be horizontal, but flexured. (3) The materials would not proceed, as a whole, from the same point of origin, but would be first in a state of irregular dispersion over a more or less wide area, from which they would have to be collected by the current. (4) Some chemical or physical changes may occur: gas may evolve from the oil during the process of migration through the sand, as well as in the final area of deposition (reservoir), whether it be dissolved gas released by the reduction of pressure, or gas produced by chemical action (?).

It is not possible to discuss here in detail the effects that such a set of conditions would entail; and the lack of experimental data as well as the complexity of the problem does not allow a definitive conclusion. But a careful investigation of the nature and probable influence of the intervening factors has led the writer to believe that the tendency toward segregation of the materials (oil and gas), as well as to their relative order of deposition, would still obtain. Only, for each material the zone of accumulation instead of being unique, may be repeated several times in succession; for the velocity of the transporting water would pass through a series of maxima and minima, and the minimum velocity required for the deposition of this material may be encountered more than once. Further, these zones of accumulation, instead of being limited to reduced areas, would become zones of more or less great extent, and the oil zones and gas zones would sometimes encroach on each other. The water zone—the water being in large excess—would extend all through, with or without local interruptions.

In Pennsylvania, at least for oil and gas, the rule holds good (question 1). As Mr. Clapp remarks, "natural gas is found east of Pittsburgh in abundance, while oil is generally absent." The thrust would have proceeded here from the southeast, and the first pools encountered, when we start from the line of application of the thrust and follow in the direction of the movement of deformation toward the northwest, are gas pools. The oil pools are mostly farther west and northwest; Pittsburgh happening to be nearly on the dividing line. This distribution is exactly what should be expected from the law. It is further interesting to note

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that the gas and oil regions (zones of deposition) encroach sometimes on each other, as may be seen by the inspection of the map, and this too would agree with the conclusions of the writer.[12]

In the fields of central Europe (Galicia, etc.) (question 2) where at least two consecutive thrusts in the same direction have taken place, the results are more obscure. But, here again, it seems as if oil would be ahead of gas. Transylvania occupies the central portion of the sector of which the Carpathians constitute the arc, and gas is found in this section at many points of the basin drained by the Maros River and its tributaries east to the Hargitta range, and by the Szamos river northward, while oil is mostly found farther north, east or southeast, in the outlying region. The general disposition seems to follow here closely the disposition of the fields of Pennsylvania, and probably for the same reasons.

As far as salt water is concerned (question 3) the writer has shown that the water should reach farther away than the transported materials (oil and gas). This is evident from the fact that these materials would be dropped from the current when the minimum velocity which would allow their motion, was reached. But water itself would continue to move until its own speed was reduced to nil. So that it becomes possible to imagine the formation of "pools of water," ahead of the region of oil and gas, and farther from the line of application of the thrust. Originally, the pressure of the water in such pools would have been due to diastrophic pressure, less the loss of head encountered. But the "keeping up" of such a pressure on water alone would be very questionable on mechanical grounds. By reason of its reduced amount of compressibility,[13] water is a bad accumulator of pressure, unless it is held in an absolutely tight container, like the cylinder of a hydraulic press. But terranes are not so tight, and such a pressure would soon disappear. If then, pressure is ever found to exist in a "water pool," ahead of the oil and gas regions, and if such a pressure can not be traced to static pressure due to artesian action, it must indicate the presence of some oil or gas reservoir with which the water would be in communication, and that would act as an accumulator for the pressure. It would not be necessary, in such a case, for the accumulator to be located in the immediate neighborhood of the water pool, as pressure may be transmitted by water itself through underground channels, the same as it would be through a pipe from an ordinary compressor.

Questions 4 *and* 5.—In order to answer these, it will be useful to enter into some more details of the mechanical process of deformation and of

[12] M. L. Fuller: The Gaines Oil Field of Northern Pennsylvania, 22nd *Annual Report of the U. S. Geological Survey* (1902), Plate XXXVI, Map of oil and gas fields of Pennsylvania, opposite p. 579.

[13] Regnault, Grasset and Cailletet give about 50 millionths for the compressibility of water at 0° C., for one atmosphere of pressure.

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the migration of the fluids as interpreted by the writer. This part has only been sketched in the preceding paper.

Let us imagine a prism composed of a series of horizontal strata, submitted to a lateral horizontal thrust, acting in the direction of the arrow (Fig. 2). For the sake of simplicity, we will suppose, at first, that the strata are continuous, individually homogeneous and of equal thickness all through, and that the superincumbent load is distributed in a uniform way; further, we will consider only this part of the prism that is situated above its theoretical neutral plane. We have seen (p. 743) that, under such conditions, the prism will be deformed, if the thrust is of sufficient magnitude; that the deformation will begin first along the line *AA* of application of the force, and, by and by, will reach farther away from it, giving rise to successive waves or longitudinal bends (anticlines and synclines) normal to the force, more or less parallel to *AA*, and de-

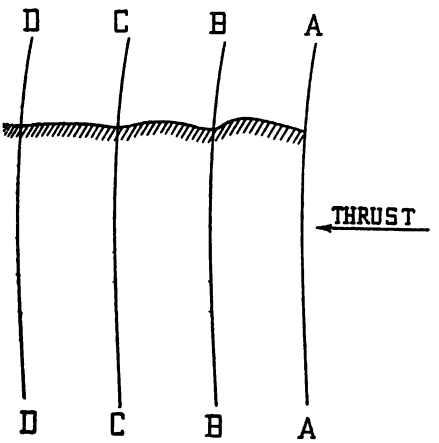

FIG. 2.

creasing in importance the farther we reach in the direction of the arrow; and, finally, that the rate of the compression to which the different points of the prism are submitted will follow a similar law, decreasing in a general way as their distance from *AA* increases, but presenting a wavy succession of maxima and minima, the maxima corresponding to the synclines and the minima to the anticlines. The whole prism may be divided into zones, individually elongated in a transversal direction to the prism and parallel to *AA*, in which the amount of compression and of deformation obtained are greater the nearer the zone stands to the line *AA*.

The first consequence is that the fluids inclosed in the strata would be put in motion *as soon* as the deformation itself begins, and that they would move from the areas of higher pressure toward the areas of lower pressure, *i.e.*, away from the thrust. But a time may arrive at which the

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elasticity of the compressed material of the strata in the first zone *AABB* will be overcome; the material, unable to satisfy any longer the accumulated stresses by simple bending, will give way, and ruptures or crushings will occur. These disruptions will not take place at the same time at every point, as the active forces and the resistances would never be equally distributed everywhere; but the material will give way in succession at points irregularly distributed along this zone, and these points will become as many *centers of impact*, transmitting a sudden pressure to the inclosed liquids. This will be renewed again and again, until compression has reached its maximum effect, for the time being, in the zone *AABB*, *viz.*, until the accumulated stresses have been everywhere satisfied by deformation in this zone, and a new temporary equilibrium obtained. From this moment, the principal effect of the deformation will be transferred to the next zone *BBCC*, where the same succession of phenomena may take place; then to *CCDD*, etc. *So that while AA remains, throughout, the line of application of the thrust, the zone of active deformation, or the zone of the centers of pressure, will progressively move away from it in the direction of the thrust*, and this displacement may continue as long and as far as the thrust itself continues to operate. It may thus reach considerable distances, with an ever decreasing effect, until it dies out.

The same process will be repeated many times. Stresses will constantly accumulate in the zone *AABB* and new disruptions will follow, that will spread out, from place to place, or from zone to zone, away from the thrust, until they reach the limit of the deformed area. The region will thus be gone over repeatedly by *waves of deformation* that will follow each other in succession and gradually decrease with the distance, as ripples do in a still water where stones are dropped at intervals from the same point.[14]

It may be stated here immediately that this process would not imply, as a necessary consequence, a migration of equal importance for the fluids transported (oil and gas). On the contrary, we will see that migration may sometimes be very limited in extent. But the compressed region as a whole would be, under the simple conditions admitted, progressively combed of its transportable material, the combing process following the direction of the movement.

Let us now return to the moment of our description where the compressed rock material is giving way along the zone *AABB*. We have seen that disruption would take place on different points of the zone in irregu-

[14] The resulting motion imparted to the liquids would thus become essentially discontinuous and the transportation of the material from one point to another would be composed of a succession of independent stages. But during each of these stages, the motion of the current may be considered as steady or as constituted of a succession of steady motions, with a sufficient degree of approximation.

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lar succession, creating successive centers of impact. Let us consider one of these centers, O, which may be situated on any line NN parallel to the line AA of application of the thrust (Fig. 3). The fluids inclosed in a given stratum, compressed at the point O, will be pushed forward in the direction of the arrow. But this movement will not take place simply in a straight line, normal to NN; it will be fan-shaped, and the surface covered by the liquids in motion will take the form of a sector, whose center will be in O. The cross-section of the stream will thus increase progressively in the direction of flow, and if the resistance to motion is uniform, as in our simplified case, the velocity of the liquid will progressively decrease away from the thrust. The points of equal mean velocity $v_1, v_2, v_3, \ldots$ written in a decreasing order, will be distributed along concentric arcs of circles, of successively increasing radii. If now the same process is followed for every point of the line NN, and the points

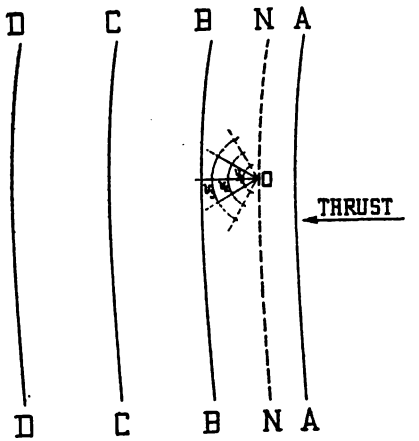

FIG. 3.

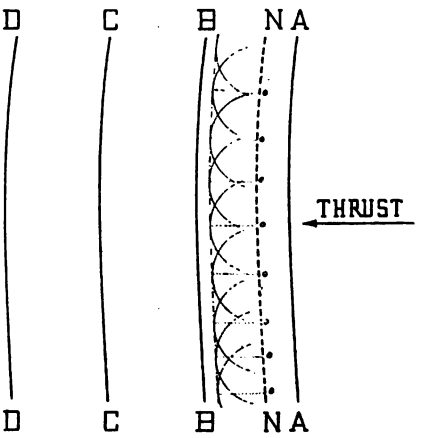

FIG. 4.

of equal mean velocity $v_1, v_2, v_3, \ldots$ are marked on the normals to this line, these points will be found to be located on the envelopes of the corresponding circles, *i.e.*, on lines parallel to AA (Fig. 4). And the mean velocity of the sheet of liquid displaced, when considered in its entirety, will be found to decrease progressively along the direction of flow, *although the motion may not take place everywhere at the same time.* This will be true whatever the position of the line NN in the zone. So that every particle of the liquid contained in the zone and which is put in motion, will successively reach the velocities $v_1, v_2, v_3, \ldots$ along more or less narrow strips parallel to AA. If then, among these velocities should be found the minimum velocity beyond which the transportation of a given material ceases to be possible, this material would be deposited along the corresponding strip. In other words, the general trend of the deposits would be parallel to the line of application of the thrust.

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Precisely there is a factor which determines such a reduction of velocity and fixes the location of the strips of deposition; this is the factor of structural deformation. If, instead of considering the single zone *AABB*, we would consider a series of such zones in succession, they would present in section the appearance of Fig. 5. The synclines being compressed more than the anticlines, each syncline individually becomes a center of pressure, whose importance decreases away from the thrust; and these centers will act as relays in the movement of the liquid. Between two relays, the velocity of the liquid would decrease when the stream passes over the arch; for the reason that the raising of the liquid from a lower level to a higher one and the sudden increase of the section or of the number of the channels of flow, when entering from a less open to a more open portion of the stratum, would cause a loss of head and an increase of the total section of flow, hence a reduction of speed. When the arch had been passed, the velocity would again tend to increase when the liquid enters the following syncline; but this new rate of velocity

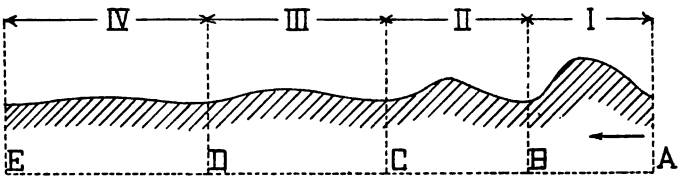

FIG. 5.

would be inferior to the one obtained in the preceding syncline, by reason of the loss of head encountered and the progressive reduction of the compression away from the thrust. So that the velocity of the current would diminish away from the thrust, but would at the same time present a succession of ever-decreasing maxima and minima, the maxima corresponding to the synclines and the minima to the anticlines or arches.

Now, with the progressive increase of compression and consequent increase of deformation, an anticline would progressively increase in height, and the difference of porosity between the summit of this anticline and the bottom of the preceding syncline would increase also.[15] So that the loss of head itself would tend to increase and the velocity of flow to decrease. Thus, the velocity of the flow passing over the arches would become lowered in the course of time, during the process of deformation, and it may reach the velocity-limit corresponding to deposition before this limit is reached on any other point of the line. The summits of the arches become in this way the natural loci of deposition.

[15] The anticlines considered here are the anticlines of the Appalachian type. Forms exaggerated by compression would give a different result. But it must be remembered that the case under treatment is the simplest possible one.

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As the arches themselves are parallel to the line AA of application of the thrust, so will be the final deposits or pools.

The same result would obtain with monoclines and the rims of terraces. These forms mostly represent a stage of incipient flexure farther away from the thrust, especially when the strata are slightly bent downward, as in a basin of deposition. And again, the same process would explain why changes in the direction of the strata along their strike may cause local concentration of the material. We have seen that the flow issuing from a local center of pressure would be diverging in direction (fan-shaped). When a line of strike is encountered, any velocity which is not normal to it may be decomposed into two components: one normal to the strike, and the other tangent to it. The first would move the liquid along the dip, in a vertical plane; the second would move it along the strike, in a horizontal one (Fig. 6). And the horizontal incurvation of

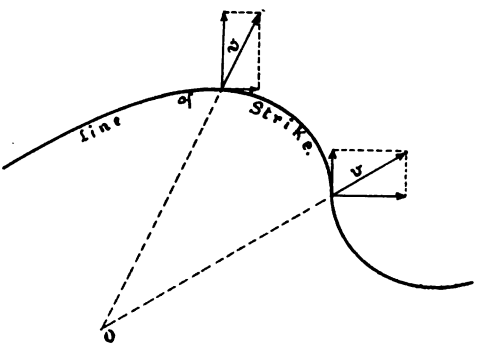

FIG. 6.

the strike may bring about local reductions of velocity (conflicting currents, etc.), and consequent areas of deposition.

It is evident that the extent of the migration of any transported material (oil or gas) under the conditions described, would depend primarily on the distances at which two successive zones, presenting the velocity-limit necessary for the deposition of this particular material, may be encountered. Between two such zones, the territory would be combed of the material—if any; but such material need not go farther than the second zone. *Transportation and concentration, or migration, becomes, under this conception, an essentially local phenomenon* in the affected area, which may take place independently on different points of the field, either at the same time or at another. This, in contradistinction with compression and deformation, which are phenomena of a general order, that would follow the laws of continuity throughout the field.

In developing the preceding theory, the writer has had in view the simplest possible conditions, and it may be objected that such conditions are nowhere to be encountered at the same time on any extended

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area. This is readily conceded. The number, direction, relative importance, and age of the successive deformative movements to which a region has been submitted, may vary widely, inducing consequent irregularities in the distribution, extent, nature and physical conditions of the layers. Lateral variation will be the rule in any stratum, and unconformities may occur between the series. Simple flexion may be complicated by the sliding of the beds; structural forms exaggerated by compression; faults and other tectonic accidents or even igneous intrusions may occur. Finally, changes may take place subsequent to deformation and accumulation, such as a change in the local level of the waters, etc. But whatever the conditions may be, the whole trend of the observed phenomena will have to be underlain by the same general principles of which the writer has tried to make an application; for these principles are not relative, but partake of the independent reality of the laws of mechanics. Only, the result will be that of a superposition of effects and may be complicated accordingly. Thus, when the geologist would come to locate an oil region, or simply a pool, under the diastrophic theory, he would not be exempted from considering every particle of evidence any more than he is today under Mr. Clapp's structural theory; but he would at least be provided with a few comprehensive laws, which would allow him to anticipate the general trend of things, to seriate the intervening factors according to their probable rank of influence, and finally to reach a conclusion based not only on inference, but on a logical process of reasoning, where deduction would come to the help of induction.

It now becomes possible to answer the further questions proposed by Mr. Clapp.

(*Question* 4). If the writer interprets Mr. Clapp[16] correctly there are many cases in which oil is found on both sides of a fault crossing a productive field, and the question is: how is oil to be found *on both sides* of the fault, as, for instance, in Oklahoma, where such faults are assumed to be thrust faults.

To this, the following answer may be made. According to the views of the writer, heretofore presented, the movement towards accumulation keeps pace with deformation and accumulation begins as soon as in the future reservoir the velocity of the transporting waters reaches the minimum under which transportation is no more possible. In a simple flexure (anticline or terrace) accumulation may begin and may even be completed long before the limit of deformation has been attained. On the other hand, a fault, especially a thrust fault, is generally one of the last expressions of deformation. When a stratum is compressed tangentially its tendency is to bend before breaking. Thus, compression may first accumulate the hydrocarbons in a reservoir and only later break through this same reservoir with a fault, after the accumulation has been com-

[16] From correspondence with Mr. Clapp.

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pleted. According to the relative position of the fault and of the reservoir, the hydrocarbons would be found either in the front, in the rear, or on both sides of the fault. The last case, where the fault cuts through the reservoir, would naturally be the most frequent, as the location of both the reservoir and the area of maximum local stresses would be nearly coincident. This would appear the simplest and most probable explanation of the facts. In the case under consideration, the oil deposit would antecede the faulting, in the same way as the coal seam antecedes the fault which has displaced it.

(*Question* 5). We have seen that a distinction must be made between the line of application of the thrust and the zone of active deformation or the zone of the centers of pressure. The former's relative position in the deformed area remains constant during the process of deformation, whereas the second would move away from it progressively, in the direction of the force. Concentration and accumulation may thus take place as far as deformation remains effective, provided the other conditions (existence of oil and gas in a disseminated state, presence of water, porous layers, etc.) are favorable; and the distance attained is limited only by the extent of the area of deformation itself. On the farthest borders of such an area, where the effects of compression would be reduced, the velocity of the transporting waters would be reduced in proportion, and the velocity-limit necessary for deposition may be reached with dips smaller than elsewhere. But it must be remembered that the remoteness of the "mountain belt" or the "major axis" from the point of accumulation, would have no bearing whatever upon the distance covered by the migration of the hydrocarbons. As previously stated, migration is essentially a local phenomenon.

Answering Mr. Pack, it is not the intention of the writer to assume that the oil existed in the original strata where the parent matter was at first deposited (in lagoons, marshes, deltas or at the bottom of the sea) from the moment of the deposition of such parent matter or even soon afterward; and the writer readily admits that the transformation may have occurred not only during the time of accumulation of the sediments in the syncline, but even during part of the movement of deformation itself. But, as already stated, this point is immaterial for the theory, which is interested in the genesis of the oil only in so far as the mode of formation concedes a first state of dissemination of the hydrocarbons in a water-laden sediment, submitted to increasing vertical pressure and subsequent lateral thrust or thrusts.

It frequently happens that, from the start, it is not possible to attack a problem in its more complex form; and this is especially true when we try to submit natural phenomena to mathematical treatment. The number of the factors that would affect the results, their intricacy, and, let us say, our ignorance of their real nature and true mode of action,

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is such as to put any solution out of reach, unless we consent to reduce the problem to its simpler lines and to consider, at first, some well-defined and better-known specific instance. The writer is aware of the inconveniences and of the danger inherent in such a procedure, but, unfortunately, it is the only one at our disposition. If the results are not always entirely satisfactory, it is no less true that the solution of a specific case may open our eyes to the general direction of things, exactly as the knowledge of a curve between narrow limits may give us a glimpse of the class to which it belongs.

Among the known oil and gas fields, the Appalachian belt seems to the writer to present one of ideal qualities. Here, the structure is relatively simple and has been well worked out; the principal folding has taken place at one defined period and under the influence of a single dominant thrust; and later disturbances have not obscured the results. In this region, the thickness of the strata involved justifies us in considering the oil sands as located above the theoretical neutral plane, which simplifies the treatment, and the gentle anticlines offer a simple feature of distortion. These conditions render an easy analysis of facts and application of mechanical principles. Mechanical laws are absolute, and if they are true in one field, they are equally true in any other. They simply need to be applied with proper data and proper judgment. We concede that sometimes conditions may be confusing, as in the South Coast Range of California, where the folds are often shallow, as Mr. Pack remarks, and where, further, two different sets of thrusts, acting from two different directions, may have to be considered. But liquids will always tend to move from zones of higher to zones of lower compression; for every material transported in a current of water, there will be always a velocity-limit under which transportation is no more possible and deposition ensues; distorted strata will always obey the general laws of distortion; and the mechanical reasons which have brought the writer to consider diastrophism as the principal motive power and water under hydraulic action as the transporting agent, will remain the same. As in an equation, you may have to change the coefficients; the fundamental value of the equation remains.

Let us state here, incidentally, that forms of distortion, like an anticline, may present zones of porosity at one time and not at another. An anticline may be said to begin with the smallest incurvation of a stratum and it may end in a flattened isoclinal fold, overturned or not, faulted or thrusted or not. Between the extremes, all kinds of intermediate forms exist, in which the internal structure of the material would not be the same, and the conditions of porosity would vary accordingly. These would even vary for any given anticline during the folding process. What the writer has said about anticlines must be understood to apply to the anticlinal ridges of the Appalachian type, which he had in view, but not to forms exaggerated by compression.

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It is true that anticlines of the aforesaid type are important, in so far as the accumulation of oil is concerned, as zones of greater porosity; a container is always needed to hold the contents. But this is not the sole function of an anticline, nor is the anticline the sole possible container. This has been pointed out by the writer.[17] Convex edges or arches along the dip or the strike of the strata may be good places for accumulation in general, as these would result in a reduction of the velocity; and buoyancy, whose real function seems to appear here, would act as gravity does when a slackening current of water drops its load: only the action would be reversed. Buoyancy would deflect the direction of motion upward and the hydrocarbons would become trapped under the concave or vault-like portion of the strata.

The lithological character of the strata may have much to do in the way of easing or impeding the circulation of the liquids; but an extensive layer with the same lithological character, the same thickness and the same dip throughout, probably would not be a favorable place for accumulation, as it would offer poor chances for an abrupt reduction of motion, which seems to be an essential factor of deposition or accumulation. It is precisely where "folds dominate the structure" that the best conditions of deposition ought to be found.

There is no mechanical reason why rock pressure, as the writer understands it, should be fairly constant over considerable areas in an oil or gas pool; but there are reasons why it should *not* be so. According to the writer's views, rock pressure in oil or gas pools is essentially due to the original pressure of deformation, less the loss of head encountered on the way, plus a certain possible amount of added pressure due to progressive gas increase.[18] Now, an oil pool or a gas pool cannot be compared to a gas holder or to a reservoir for water under constant pressure. In a gas holder, the gas is at the same pressure everywhere, and in a reservoir under constant pressure, water is at the same pressure on the same horizontal level. For the reason that, in the first case, gas molecules can move with an equal freedom in any direction; and, in the second case, water molecules have an equal freedom of movement in all directions of the same horizontal plane. But a gas pool or an oil pool is formed by the juxtaposition of an indefinite number of cells, intercommunicating in the most irregular way, and equal freedom of movement is not permitted in any direction. Further, the resistance to motion between cells is variable, and the resistances accumulate with the distance between points. So that, even barring the possible additional pressure due to irregular gas increase, the original pressure due to deformation would not have to be the same at all points, as the loss of head would be at variance; and no balance could be reestablished between points, in the

[17] See pp. 739, 749.

[18] *Idem.*

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course of time, as the resistance to motion between cells would remain the same. The conclusion is that rock pressure, under the diastrophic theory, must vary, as a rule, from one point to another in an oil or gas field, and the theory agrees once more with the observed facts.

Mr. Pack, in common with Mr. Coste, remarks that the very factors that the writer calls upon to seal up an oil pool and to maintain a given rock pressure, might equally well be called upon to prevent migration originally. To this it may be answered that conditions in both cases are entirely different. In an oil and gas pool, the rock pressure is essentially static; whereas, during the process of migration, the motive pressure would be dynamic, the transporting agent being water acting under a ram effect. As an illustration in the case, you may consider the following test: try to drive a nail in a board simply by the weight of the hammer bearing on the head of the nail, and then drive the nail by a blow. The difference is apparent at once. The first case is a case of static pressure; the second, that of dynamic action. There are other differences, all in favor of the process of migration: Rock pressure is only a remainder of the pressure to which migration is due; again, the "tubes" of the capping rock which holds the gas would be of a subcapillary size, instead of being capillary as the channels of the sand strata through which the movement toward accumulation takes place, and the resistance offered by the Jamin tubes is in the inverse ratio of their diameter; then again, original oil would be in a state of disseminated particles in a matrix of water, instead of being in the state of a more or less continuous mass, as in an oil pool, etc.

Oil which has moved through the rock pores and has collected under pressure at a given place, would not move back over the path it originally traveled any more than the nail which has been driven in the wood would be thrust out of the board. Static pressure would be unable to undo here what repeated dynamic action had caused to be.

The fact that "in the California fields, where water sands and oil sands are interstratified, no pressure comparable with the pressure in the oil sands is recorded for the water sands which lie between the oil sands, unless a distinct flow of petroleum gas is noted with the water," is easily explained, in accordance with physical and mechanical laws, as a consequence of a common original diastrophic pressure. Let us note first that Mr. Pack's remark implies that whenever there is a difference in the pressure between an oil sand and a water sand, there is an intermedial layer between both sands which is impenetrable to this pressure, or else the balance of pressure would be soon reëstablished. The conclusion is that the pressure in both sands is independent. Now, let us suppose that, at a certain moment, the same amount of pressure should be imparted to both sands, as would be the case if these layers were submitted together to a common diastrophic pressure; and, for the sake of simplicity, let us consider the temperature as constant. Liquids are

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compressible only in a very small degree; which means that a very small reduction in volume would correspond to an enormous increase of pressure, and, conversely, that a very small expansion of volume, at constant temperature, would allow a very great reduction of pressure. Whereas gases are essentially compressible, and a great increase of pressure, at constant temperature, would only be reached by a corresponding large decrease of volume; or, conversely, the volume would have to be largely increased to decrease the pressure to any serious extent. Gases are thus good accumulators of pressure, where liquids are not. As oil would always be in relation with some gas, an oil sand would become a good accumulator of pressure also; but a water sand would not, unless accidentally put in direct relation with the gas. On the other hand, strata, especially Tertiary sediments, are never absolutely tight, and a very small expansion of the water body would rapidly reduce the pressure to the normal; whereas oil strata, containing gas, would be in a very much more favorable condition to retain a large part of it.

The fact pointed out by the writer in reference to the increase of rock pressure with the depth—or age—of the strata in the Eastern fields (Appalachian belt), is not contradicted by this other fact that the situation seems to be reversed in some Californian fields, where greater pressures are noted in younger beds resting unconformably on older rocks; as the tectonic conditions would not be the same in both instances. In the first case, the whole pile of sediments has been involved in the same deformative movement, due to a single dominant thrust, and, therefore, the result is simple and follows the law of continuity; whereas, in California, deformation has been due to several thrusts, acting at different periods, in different directions, and it has affected unequally the unconformable series. The mechanical law of the superposition of effects, to which the writer has previously referred, may have largely interfered in this case, and its influence would have to be defined.

It is the writer's view that "fossil pressures," to use the picturesque expression of Mr. Pack, are to be considered as the rule in Nature and not the exception. The earth's body is in a constant state of readjustment, as well shown by seismic and kindred phenomena; and these phenomena draw their source from the past much more than from the present, as they point to the relief of slowly accumulated strains. When the lower sediments of a syncline are compressed by superincumbent strata, the first compression dates from the time of the first deposition of these strata; the "heaving" in mines, the deformation of rocks in quarries during operations, the production of fissures on the summit of arches denuded by erosion, etc., show as many traces of fossil strains. Would it be wise to deny the possibility of a fossil pressure where conditions for its formation and preservation seem to have been the best? If there has ever been such a thing as deformation through diastrophism, strains

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have been the result; and the gases inclosed in the strata, as well as the liquids in relation with them, having been submitted to these strains, must have acted as accumulators where favorable conditions obtained. They must show traces of these strains today, unless these traces have been dissipated since. But if they had been dissipated, would not the pressure due to gas formation have been dissipated in the same way? Further, the building up of rock pressure within the reservoir by the change of some oil into gas, seems to meet with a serious objection. Washburne has pointed out that crude oil is exothermic, and therefore has no tendency to split or decompose under its temperature of dissociation[19] ("Cracking point," from 250° to 400° C. and above); and the possibility of such a temperature could not be considered, as a rule. Thus, if oil migration is due primarily to rock deformation, it becomes difficult to escape the conclusion that rock pressure in an oil or gas pool is, at least partially, due to deformation itself.

The writer thinks that he has answered the different objections raised by his critics and shown that the items criticized agree with the diastrophic theory instead of being in opposition to it. But he does not intend to say that the theory cannot be amended or perfected. If the principles on which the theory is founded should be recognized as true, some definite and more complete form may be reached as a result of "team work," in which critics and criticisms may play their part.

[19] *Trans.*, vol. 51, p. 607 (1915).

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Principles of Natural-Gas Leasehold Valuation

BY SAMUEL S. WYER,* COLUMBUS, OHIO

(Arizona Meeting, September, 1916)

Magnitude and Economic Importance

THE magnitude and economic importance of the problem of correctly valuing natural-gas leaseholds become evident when we consider that:

(*a*) Natural gas is handled in 55 per cent. of the gas distributing plants in the United States.

(*b*) Present known natural-gas acreage forms 47 per cent. of the total known mineral-land acreage in the United States.

(*c*) Five acres of land are now required to protect and maintain continuous service to each of the 2,000,000 domestic natural-gas consumers in the United States.

(*d*) The cost of acquiring and maintaining this acreage of an expendible resource represents a substantial part of the cost of the natural-gas service to the consumer.

(*e*) "The right of a citizen by means of his ownership of or his mining leases on land to draw gas from beneath its surface is property and sometimes valuable property."[1]

A large number of other court decisions have established the basic legal principle that the right to drill for gas within a given area constitutes an interest in the land itself, and is necessarily an exclusive property right.

Definition of Vested Interests or Rights

"Vested interests are economic interests which are legally recognized to be such that they cannot be impaired by public action, directly or indirectly, without indemnification. Vested interests are largely property interests. The recognition of an interest as a vested interest gives it some of the attributes of property, and by American courts it would be comprised in their very inclusive concept of property. Otherwise than through property, vested interests generally arise through contract."[2]

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* Consulting Engineer.

[1] U. S. Circuit Court, Haskell vs. Cowham, 187 Fed. Rep., p. 403.

[2] Richard T. Ely: *Property and Contract in Their Relations to the Distribution of Wealth.*

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Definition of Lease

A natural-gas lease is a contract for a consideration establishing a vested right to enter upon a definitely described parcel of land, for a determined period, to prospect for, reduce to possession, remove and market natural gas. The vested right is the crux of the whole matter, and it is immaterial whether the instrument creating it is called a "lease," "contract," "grant," or "deed of conveyance."

"Oil and gas leases, generally speaking, are not strictly leases as defined in the law of landlord and tenant. They are in the nature of written licenses, with a conditional grant conveying the grantor's interest in the gas or oil well, providing that gas and oil is found in paying quantities. It is well settled that the title of the lessee is inchoate until such discovery, at which point of time he acquires a vested estate in the mineral itself."[3]

"In a given tract of land it is always a matter of doubt to what extent, if any, mineral may exist in paying quantities, until very considerable development work has been performed, which requires in most instances large expenditure of capital. For this and other reasons, a custom long ago arose for the owner of supposed mineral land to grant to a mine operator the right to enter upon the land and search for and extract mineral, and the form which the contracting parties pretty generally adopted to express their agreement was a 'lease,' which purported to entitle the 'lessee' to occupy such part of the premises as was necessary to carry on his mining operations, and to use, mine and extract the minerals therefrom."[4]

License and Lease Distinguished

"A license is an authority to go upon the land of the licensor to do an act or series of acts there, but passes no estate or interest in the land. It is technically an authority to do something on the land of another without passing an estate in the land. The distinction between lease and license is that the former is a distinct conveyance of an actual interest or estate in the lands, while the latter confers a mere incorporal right to be exercised in the lands of others."[5]

Definition of Property

Property in the legal and economic sense means an absolute, entire and exclusive right to do anything, and embraces material things, labor or services, and rights. It is important to bear in mind that the term means not only the thing owned but also every right which accompanies such ownership and is its incident. Thus, the right to drill at some future time a gas well on a certain parcel of land is property in the same sense that the land itself is property. A long line of court decisions[6] has definitely established what the term property includes.

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[3] Lindley on Mines, 3d ed., p. 2144.

[4] *Idem*, p. 2134.

[5] *Idem*, pp. 2129 to 2130.

[6] A list of cases is given in Ely's *Property and Contract in Their Relations to the Distribution of Wealth*, p. 855.

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Definition of Science of Valuation

The science of valuation is an interdependent mixture of law, economics, and engineering: Law, to establish the governing ethical principles; economics, to establish the criteria of value or monetary standards; engineering, to apply the general criteria of value to any given specific case in accordance with ethical principles.

Definition of Value

Value is the relation of two services, and implies two things—utility and scarcity. The idea of value entered into the world for the first time when a man said to his brother, "Do this for me, and I will do that for you." They had come to an agreement. Then we could say the two services were worth each other. The word "value" means the exchange power which one commodity or service has in relation to another. Value may also be defined as the price of a given unity of wealth multiplied by the quantity or number of units, and hence is an expression of the relative desirability of different classes of wealth or property in terms of a common standard.

Market Value

This is an adjustment of two services as between a willing seller and a willing buyer under open conditions of competition, without any coercion from either party. The sale value of property is indicated by recent prices for similar property in the same locality. Sale values are not uniform for the same kind and quality of goods, even in the same locality, unless there is a complete general knowledge of current prices and the desire to buy is approximately equal to the pressure for sale.

Price

A price, when expressed in terms of a common standard of value, is the amount of money accepted in exchange for a unit of goods. Prices resulting from exchanges are agreements between two persons as to the value of a unit of property. Prices are, therefore, of human and mental origin, and are established by economic causes which influence the opinions of the majority of purchasers and sellers.

Wealth and Property

"Wealth is defined as material objects owned by human beings. Property is the right represented by ownership, and is a material object itself. The benefits derived from wealth make its ownership desirable, and the right to enjoy these benefits is indicated by the term 'property.'"[7]

[7] H. H. Chapman: *Forest Valuation.*

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Gas is a Mineral

Although natural gas is a mineral, it cannot be measured in terms of acres, like coal, ore, or solid minerals. On account of its fugitive and wandering nature it develops characteristics which differentiate natural gas valuation problems from all other mineral valuation questions. It is important to note that it is not a continuous process, but a fixed storage reservoir that must be considered, which storage will be depleted, without regeneration, as the supply is removed, and that the transportation from the mining district to the ultimate consumer can take place only through pipe lines.

Transient Nature of Mineral Values

"In the administration of natural resources, we must bear in mind some important differences. Mineral rights and surface rights bear quite different relations to society. Surface rights have a permanent, underground rights but a transient value. The more intensely a farmer cultivates his land, the more valuable does it become to him and to the community. If development takes the shape of permanent and useful structures on the land, the increase in value to the individual and to the state may reach vast proportions. Indeed, it is not possible to anticipate the maximum usefulness which surface rights may acquire; but we know that when these rights are being used to the best advantage the country is also reaping the greatest benefit, and it is then that the life of the right appears longest and most useful.

"A mineral right is of a different nature. The more it is worked, the smaller becomes its intrinsic value. It is of value to the community only at the time, and in proportion to the extent of its productiveness; but the greater this temporary usefulness, the shorter its duration.

"The agricultural claimant locates a 'homestead,' while the prospector locates a 'claim.' The former indicates permanency of tenure, while the latter suggests a temporary possession."[8]

Judicial Recognition of Hazard

"We take judicial notice of the fact that mining for oil and gas is a very hazardous and dangerous business, involving great risk, and requiring large expenditures of money."[9]

Intrinsic Value of Gas

Natural gas in the ground, or even in the field, has little intrinsic value in or of itself. The surface owner cannot ordinarily take the gas to market. The gas becomes of value primarily only as service is performed on it in delivering the gas under satisfactory working conditions to the ultimate consumer.

The right vested in the surface owner to remove the gas is always a property right and in many cases a valuable one. However, the gas

[8] Frederick F. Sharpless: *Trans.*, vol. 48, p. 388 (1914).

[9] Supreme Court of Appeals, W. Va., Garrett vs. Oil Co., 66 W. Va., p. 591.

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itself does not become property until reduced to possession, as explained later in this paper, under *Property Rights in Natural Gas.*

The value of the gas cannot be considered by itself, but must be considered with the leaseholds, wells, gathering lines, compressors, main lines, and distributing systems, all of which must be properly coördinated to give the gas in the field any value whatsoever. Furthermore, in considering the value it would not be policy to consider a single individual case, but the property as a whole must be taken together.

Quality

The quality considerations that affect the value of a natural-gas leasehold are as follows:

(*a*) Heating value.

(*b*) Purity, with regard to sulphur, moisture, and oil.

In this connection it is important to bear in mind that the quality is fixed by nature, and that it is economically feasible to alter this. On Apr. 13, 1915, the District Court of Shawnee County, Kansas, in the unreported case of Ely vs. Public Utilities Commission, No. 29,229, set aside the order of the Commission fixing a minimum heating value for natural gas. The decision has not been appealed.

Limits of Underground Reservoirs

There is absolutely nothing fixed from the surface, and while surface conditions may be indicative, the question of underground location can be established by the drill alone. Even the presence of gas sand is not necessarily an indication of the presence of gas.

"We judicially know, as a matter of common knowledge, that gas or oil does not exist in paying quantities under all lands within the recognized district, and there is no other generally acknowledged way than putting down a well to determine whether or not it does exist."[10]

Rock Pressure

The rock pressure of individual wells is not necessarily indicative of the amount of gas that may be obtained from such wells.

Criterion of Actual Flow

The open flow capacity of a well may be far from indicating the value of the well under routine operating conditions, since the actual line flow will always be less, and in many cases very much less than the open flow. If a well is located in a part of the field where it is not feasible to maintain

[10] Supreme Court of Indiana, Consumer's Gas Trust Co. vs. Littler, 162 Ind., p. 326.

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low line pressure into which the well may discharge, obviously the flow will be less than if the well were located where low line pressure might be continuously maintained.

Migratory and Fugitive Nature of Natural Gas

"Plaintiff assumes that there is a certain fixed amount of oil and gas under his farm, in which he has an absolute property. True, they belong to him while they are a part of his land; but when they migrate to the land of his neighbor, or become under his control, they belong to the neighbor."[11]

"Oil and gas . . . have no fixed situs under a particular portion of the earth's surface within the area where they obtain. They have the power, as it were, of self-transmission. No one owner of the surface of the earth, within the area beneath which oil and gas move can exercise his right to extract from the common reservoir, in which the supply is held, without, to an extent, diminishing the source of supply as to which the other owners of the surface must exercise their right."[12]

"Oil and gas . . . unlike coal, iron and other minerals, they do not have a fixed situs under a particular portion of the surface, but are capable of flowing from place to place, and of being drawn off by wells penetrating their natural reservoir at any point. They are a part of the land, and belong to the owner so long as they are in it, or are subject to his control; but when they flow elsewhere, and are brought within the control of another by being drawn off by wells drilled in other land, the title of the former owner is gone. So, also, when one owner of the surface overlying the common reservoir exercised his right to extract them, the supply as to which other owners of the surface must exercise their rights, if at all, is proportionately diminished."[13]

Possession of Natural Gas

"The owner of the surface has no property right in the gas or oil until he has actually reduced it to possession, or, if he has any property right therein, it is a right in common with the co-equal right of other land owners to take from the common source of supply, and therefore subject to the legislative power to prevent a destruction of the common property of one of the common owners."[14]

"The property of the owner of lands in oil and gas is not absolute until it is actually in his grasp and brought to the surface."

"If possession of the land is not necessarily possession of the oil and gas, is there any reason why an oil and gas operator should not be permitted to adopt any and all appliances known to the trade to make the production of his wells as large as possible."[15]

"It is no longer an open question in this state that natural gas, when reduced to possession, becomes private property, and is a commercial commodity, which the owner may dispose of in whatever manner he may consider most advantageous."[16]

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[11] Supreme Court of Pa., Jones vs. Forest Oil Co., 194 Pa., p. 379.

[12] U. S. Supreme Court, Ohio Oil Co. vs. Indiana, 177 U. S., p. 190.

[13] U. S. Circuit Court of Appeals, Brewster vs. Lanyon Zinc Co., 140 Fed. Rep., p. 801.

[14] U. S. Supreme Court, Ohio Oil Co. vs. Indiana, 177 U. S., p. 190.

[15] U. S. Supreme Court, Ohio Oil Co. vs. Indiana, 177 U. S., p. 190.

[16] Appellate Court of Indiana, Richmond Nat. Gas Co. vs. Enterprise Nat. Gas Co., 66 N. E. Rep., p. 782.

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Property Rights in Natural Gas

"The rule first announced in the case of Hale vs. Reed, 15 B. Mon. (Ky.) 479,[17] and followed by the courts of last resort in all the great oil- and gas-producing States save one, Indiana . . . is stated by Mr. Justice Shiras in Brown vs. Spilman, 155 U. S., 665, as follows:

"'Petroleum gas and oil belong to the owners of the land, and are a part of it so long as they are on it or in it, or subject to his control; but when they escape and go into other land, or come under another's control, the title of the former owner is gone. If an adjoining owner drills his own land and taps a deposit of oil or gas extending under his neighbor's field, so that it comes into his well, it becomes his property.'

"The rule adopted by the courts of Indiana is this: The owner of the fee of oil- or gas-bearing lands does not have an absolute ownership in the oil or gas in place in the land, but a qualified ownership only, capable, however, of being made absolute by reduction to possession."[18]

"It must be held he who by lawful right reduces to his possession mineral, gas or oil, has the same absolute right of property therein, with the same power of barter, sale or other disposition, including, of necessity, the right of transportation and delivery under such reasonable rules and safeguards as the exigencies of the case may demand and the State employ, as the farmer has of his corn, his wheat, or his stock, or the merchant of his ware, and such absolute right therein as the State cannot deny him without just compensation, and any attempt to do so would be in violation of the fourteenth amendment to the federal constitution."[19]

"Gas and oil were likened to, not made identical with animals *ferœ naturæ*, and, like such animals, were subject to appropriation by the owners of the soil, but also like them did not become property until reduced to actual possession.

"But an important distinction was pointed out. In things *ferœ naturæ*, it was observed, all were endowed with the power of reducing them to possession and exclusive possession. In the case of natural gas, only the surface proprietors had such power, and the distinction, it was said, marked the difference in the extent of the State's control. In the one, as the public are the owners, everyone may be absolutely prevented from seeking to reduce to possession. No devesting of private property, under such a condition, can be conceived, because the public are the owners, and the enactment by the state of a law as to the public ownership is but the discharge of the governmental trust resting in the state as to property of that character. . . . On the other hand, as to gas and oil, the surface proprietors within the gas field all have the right to reduce to possession the gas and oil beneath. They could not be deprived of this right which belongs to them without a taking of private property."[20]

"Natural gas after severance is a commodity which might be dealt in like other products of the earth, as coal and other minerals, and is a legitimate subject of interstate commerce; and that no State, by such laws as were involved in the case, can prohibit its transportation in interstate commerce beyond the lines of that State. The court held, after considering and construing the provisions of the act of 1907, that it was, upon its face, a law undertaking to prohibit the transmission or transportation in interstate commerce of natural gas to points beyond the State; that it was

[17] Decided in 1854.

[18] U. S. Circuit Court, Kansas Natural Gas Co. vs. Haskell et al., 172 Fed. Rep., 545.

[19] *Idem.*

[20] U. S. Supreme Court, West vs. Kansas Natural Gas Co., 221 U. S., 229.

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an unconstitutional interference with the rights of the complainants, who were legitimately engaged in that commerce, and that therefore the act was null and void."[21]

Natural Gas Should be Considered Only as a Utility Service

The only criterion of value that ought to be applied to gas leasehold valuation problems, is to consider the natural gas as an integral part of a public utility service.

Manufacturing Use of Natural Gas Should Not be Considered

Natural gas, although an ideal fuel for domestic service and manufacturing work, when used for industrial purposes must be sold at an absurdly low price in order to compete with coal and producer gas for industrial service. Although over 65 per cent. of the natural gas used at the present time is used for industrial service, this industrial use has had the inevitable effect of curtailing very materially the supply for domestic consumption.

Domestic Consumers Should be Given First Preference

On account of the exceptional value of natural gas for domestic service, every effort ought to be made to conserve the supply, so as to guarantee an adequate future, as well as an adequate present continuous service. It is doubtful whether anything does more for the prosperity, convenience, comfort, and in many cases the health of a community, than the introduction and continued use of natural gas.

Effect of Governmental Opposition to Unified Control

The present governmental tendency to prevent monopoly in the gas field, causes a decrease in the field of the leaseholds, and increases the cost of the gas to the public. Gas-field operating conditions should be regarded as a natural monopoly, so that in the development of the field one company could space the wells properly, and drain the field only to its safe working capacity, thereby greatly increasing and strengthening the life of the field.

Results of Competition Always Economic Waste

Competition in a gas field always results in a duplication of lines, unnecessary wells, enhanced operating cost, lack of proper coördination, failure to remove all the gas, and shortened life of the field, with the inevitable resulting injury to the domestic consumer.

[21] U. S. Supreme Court, Haskell vs. Kansas Natural Gas Co., 224 U. S., 217.

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Ease in Drilling Stimulates Competition

The easier it is to drill a well in any given territory, the more wells will be drilled by small and inexperienced operators, and the greater will be the inefficient operation of the field. Furthermore, the indiscriminate drilling by inexperienced local operators always tends to increase the use of gas for manufacturing purposes, and takes the gas out at the fastest possible rate, thereby decreasing the effective life of the pool.

Drainage of Field

Under competitive conditions, even where the underground gas reservoir is made up of many local pools, various operators will drill into the same local pool, and thus drain out the gas from under each other's leaseholds.

Effect of Competition on Value

As the competitive conditions decrease the life of the field, and in other ways affect its stability, competitive conditions obviously decrease the value of the leasehold.

Effect of Competition on Cost of Gas

While competition decreases the value of the leaseholds, it increases the property value necessary in marketing the gas, by virtue of the large amount of development and inefficient operation, so that in the end the ultimate consumer pays more for his gas on a competitive basis than he would under conditions of regulated unified control.

Ethical Concept of Property

"The question of property is central and pivotal in modern distribution. Private property is the nourisher of mankind, the incentive to industry, and is the cement of society; it binds men together and is society's surest and firmest bond."[22]

"The right to property is found in nature, sustained by organized society, and protected by the sanctions of the divine law. This right has its origin in a prior fact that each human being is a distinct individuality."[23]

"The right of acquiring and possessing property and having it protected, is one of the natural, inherent and inalienable rights of man. Men have a sense of property, property is necessary to their subsistence, and correspondent to their natural wants and desires. Its security was one of the objects that induced them to unite in society. No man would become a member of a community in which he could not enjoy the fruits of his own labor and industry and the preservation of property is the primary object of the social compact."[24]

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[22] Richard T. Ely: *Property and Contract in Their Relations to the Distribution of Wealth.*

[23] J. P. Newman: *Supremacy of Law.*

[24] U. S. Supreme Court, Van Horne vs. Dorrance, 2d Dallas, p. 304 (1795).

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Ownership

In the United States we have been trained to regard property as a very holy thing. Here, property in the modern sense represents the basis upon which our social order was established. One of the most cherished clauses in our Constitution declares that private property shall not be taken for public use without fair compensation.

"The power to demand rent on land does not depend upon the manner of obtaining it, but upon the possession alone. Modern economic society does not ask a property owner how he became possessed of his property. The fact of possession is sufficient to yield him an income. Our civilization has been erected upon the theory of the validity of effort. Reward provides the stimulus of effort. It is reward, or the hope of reward that inspires. Deny the reward of effort, and the well-spring of effort is dried up."[25]

State is Basis of All Ownership

"There is no such thing as natural property. Property is entirely the work of law. The idea of property consists in an established expectation; in the persuasion of being able to draw such and such an advantage from the thing possessed, according to the nature of the case. Property and law are born together, and die together. Before laws were made there was no property. Take away law and property ceases."[26]

"Property implies the assent of the State, and in this we revert to the distinction between property and merely possession. If you have possession, when you lease the field another comes in and takes possession; if you have property, then the third person, the State, keeps out others although you be absent yourself. We cannot have property without law, for through law possession ripens into property."[27]

Natural Resources Starting Point of All Value

The fact that natural gas was made by nature has been responsible for many erroneous ideas regarding its worth. Few people appreciate that all raw material is natural, and is the gift of God to man, and that "all production is carried forward upon the resources of nature by labor with the aid of capital. Every product of industry owes its origin to natural resources, the fields, the mountains, the water, some natural agent was the starting point for each material good on its way through the intricacies of the industrial system. Food, clothing, wealth in all its forms, is derived originally from nature.

"The forces of nature, working through the ages, have created things which mankind needs. Human effort expended on these products of nature, converts them into forms that are usable. All usable wealth, no matter what its form, owes its value in the beginning to nature's gifts, and after that the process of production."[28]

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[25] Scott Nearing: *Income.*

[26] Jeremy Bentham: *Theory of Legislation.*

[27] Richard T. Ely: *Property and Contract in Their Relations to the Distribution of Wealth.*

[28] Scott Nearing: *Income.*

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Present Fair Value is True Basis

The same legal doctrines that are used in valuing other utility properties ought to be applied to natural-gas leaseholds. A long line of court decisions, from the United States Supreme Court down, establishes beyond question the doctrine that "present fair value" is the true basis. The crux of the whole matter is very aptly stated in the following typical water rate case:

"The value to be considered is the present fair value at the time the rate is fixed. The original cost is not at all conclusive, if it can be shown that it now has a different value, although the original cost is, as in all cases, an element which may be considered. The present fair value should be determined by the best evidence of which the nature of the case is susceptible. It should be measured by the fair market value of a similar right in the locality, or a similar locality, if such can be established by satisfactory evidence."[29]

Real Effect of Leasehold Operation

"The removal of mineral substances from the land is an act which constitutes a permanent destruction of the substance of the real estate. It is a use of an estate which, unlike the use of a house or farm, consumes the things used. It no longer exists. It is obvious, therefore, that when one grants to another the right to thus exhaust the substance of a mineral estate, the exercise of the right, so far as it goes, works upon the estate, the same result, irrespective of the form of the instrument conferring such right. Where such instrument is in the form of an absolute conveyance of the mineral estate, little need be said. It becomes a simple case of a grant of real property by deed, requiring the usual formalities incident to such grant, and the instrument itself will be construed in the same manner as ordinary conveyances of real property.

"Where the instrument is not in the form of an absolute conveyance, but where the language employed is sufficient to pass the entire estate in mineral to the grantee, it often operates as a sale of real estate, although it purports upon its face to have a different scope and purpose."[30]

How Leases are to be Valued

When gas leases are valued they are to be valued as property within the meaning defined herein.

Cost and "Value" Distinguished

To grasp clearly the distinction between value and cost is one of the first fundamental principles of valuation work. Very little reflection is needed to convince one that a thing may be worth much more than it

[29] Idaho Supreme Court, Murray vs. Public Utilities Commission, 150 Pac. Rep., pp. 50 to 51.

[30] Lindley on Mines, 3d ed., p. 2129.

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cost, or that it may be worth much less. It may even be worth or valued much more at the present time than it cost, or it may be worth or valued much less.

Time an Element in Values

"Prices and values center upon the satisfaction of human needs in the present moment. The tendency is to avoid delay, and reduce as far as possible the period which elapses between effort and satisfaction.

"The present moment is the basis for computing and comparing values. To every one the enjoyment of wealth in the present is valued more highly than at any future period. No one will undertake long-time investments unless he expects an increase in the value of his estate in a measure corresponding to the paralleling and accumulation of cost and interest."[31]

Sale Value as a Basis of Value

"In case a business changes hands by sale, the sale value so determined is accepted as the value of the business. In this transaction the price paid for the assets must be entered as their value on the books. Such sales thus establish a new recorded or book value for these assets. The fact of the sale is the most convincing evidence of value, although even this is not final proof, for one or the other of the parties may have been deceived or handicapped in the transaction. Nor will a past sale be accepted as absolutely determining a future sale even for the same property; in fact it is usually the reason for desiring a different value. Sales of property or business have a profound effect upon proprietary accounts in that the former owner receives at once all his income and capital, and can balance his books and determine his net profit, while the purchase is saddled with a cost or investment which forms the opening entry in a similar account, and no future acts will serve to reduce this initial cost."[32]

Appraisal of Market Value

"It is unnecessary to quote authorities to show that, in estimating the market value of land, everything which gives it intrinsic value is a proper element for consideration; not only its present use but its capabilities are to be considered."[33]

"The owner is entitled to the value of the property taken; that is, what it fairly may be believed a purchaser in fair market conditions would have given for it and not what a tribunal at a later date may think a purchaser would have been wise to give."[34]

Reflection of Hazard in Value

"The sale of an undeveloped resource is predicated upon an advance estimate of its prospective value for purposes of development, and whatever the theory of division of its appraised value between the land owner and the operator, there will exist a certain element of risk which necessarily involves the possibility of disproportionate profit or loss to either party and which naturally will be discounted by both. We must recognize that the risk thus created becomes a legitimate if not absolutely necessary basis for an additional item of cost in operation, and therefore will tend to increase both cost and selling price; so that if at this point there is introduced the third party to all such transactions, namely, the ultimate consumer, we discover who pays the carrying charge of this risk. This added item of cost may appear in the preliminary

[31] H. H. Chapman: *Forest Valuation.*

[32] *Idem.*

[33] United States Supreme Court, Wetmore vs. Rymer, 169 U. S., p. 128 (1897).

[34] United States Supreme Court, City of New York vs. Sage, 239 U. S., p. 57 (1915).

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financing in the form of a larger interest rate offered to the bondholder, or of a larger discount given to the underwriter, or of a larger dividend promised to the stockholder, or in all of these combined."[35]

Public is Served Best when Natural Gas Mining is Profitable

Natural gas has a heating value of from 50 per cent. to 100 per cent. greater than manufactured gas, and is also non-poisonous as distinguished from the poisons in all manufactured gas, and is therefore very much safer, especially for domestic use. Natural gas can do everything that manufactured gas need do, and many things that manufactured gas cannot do. In brief, man with all his skill has never been able to make a gas equal to that supplied by nature. Natural gas has usually been sold at prices far below the prices prevailing for manufactured gas, even without considering the increased worth of natural gas, due to its purity and much greater heating value, as against all manufactured gases. Therefore, every foot of gas that is found and served to the public represents a distinct economic gain to the community.

Natural gas can be found only by diligent prospecting. After it is found the service can be maintained continuously only by further continued development and persistent hunting for new supplies. In this development the prospector must figure on an average chance of getting one dry hole in every four wells drilled. Since the hazards are greater than in any other mining enterprise, the profits ought to be correspondingly greater. This element of profit is the only incentive which impels men to engage in so speculative an enterprise. If, in the aggregate, this amount of profit does not measure up to the hazards in the business the men will cease their work of prospecting and put their capital in safer enterprises. Wherever a close connection exists between effort and profit a strong resulting incentive is furnished for a further and continuous expenditure of effort. Therefore, a high rate of profit which will induce men to prospect continuously for natural gas brings about the condition that more people can use gas, and represents a distinct saving to the community.

Valuation Not an Exact Science

Some of the problems of valuation are unsolvable on any definite mathematic basis. To solve the problem as to what is the value of property requires the most logical reasoning, involving legal, economic and technical science, and in addition to all, that very rare endowment known as common sense.

"The ascertainment of value is not controlled by artificial rules. It is not a matter of formulas, but there must be a reasonable judgment having its basis in a proper consideration of all relevant facts."[36]

[35] George Otis Smith: *Trans.*, vol. 48, p. 431 (1914).

[36] United States Supreme Court, Minnesota Rate Case, 230 U. S. 352.

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Reproduction Method not Applicable

On account of the fugitive and migratory nature of natural gas, and since there is no regeneration, it is not possible to attempt to apply the reproduction method of valuation because it cannot be reasonably applied.

"The cost of reproduction method is of service in ascertaining the present value of the plant when it is reasonably applied and when the cost of reproducing the property may be ascertained with a proper degree of certainty, but it does not justify the acceptance of results which depend upon mere conjecture."[37]

Classification

The well-known classification of leaseholds into producing, protective, reserve, and prospective groups, will answer all practical valuation problems. Producing leases are those on which producing wells have been drilled and are maintained. Protective leases are those which are contiguous to producing leases and which are held merely to protect the wells on the producing leases. Reserve leases are those which are held—usually after a certain amount of testing—as a reserve acreage to replace the depleting supply in the present producing wells. Prospective leases —sometimes called "wild-cat"—are those which have not been tested or proven, but are held because the indications are that they may contain gas.

Unit Prices

In fixing the value of natural gas leaseholds it is not possible to establish a market price, as is the case in ordinary commercial commodities which are constantly changing ownership in the open market. The unit price per acre to be applied to the different classes of leases must be fixed by expert testimony—that is, by the judgment of men skilled and experienced in the natural-gas business—in accordance with the principles herein defined, as so well stated in the following water right case:

"If no market value can be established, then the opinion of competent witnesses as to the actual value may be considered. In this respect the case does not present any exceptional features. The same rule is applied in the case of any property, real or personal. The fair market value is the usual standard; but if it be shown that the property has no market value, then witnesses may testify to actual value, which is, of course, largely a matter of opinion. Because it is difficult to determine the exact value of a certain kind of property, it does not follow that the owner shall be refused the protection of the law. The fair present value of the right is the ultimate fact to be found and considered. Exactly what probative or evidentary facts shall be considered, or what standard of measurement shall be adopted in finding that ultimate fact, will depend largely upon the facts of each case as it arises."[38]

[37] United States Supreme Court, Minnesota Rate Case, 230 U. S., 352.

[38] Idaho Supreme Court, Murray vs. Public Utilities Commission, 150 Pac. Rep., pp. 50 to 51.

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Discussion

F. G. Clapp, New York, N. Y. (communication to the Secretary*).—I assume that where this valuable paper states, near its end, that "it is not possible to establish a market price," the author means that no particular price which will fit all conditions and fields can be given. I am not in agreement with Mr. Wyer, however, that the actual value of natural gas leaseholds is "largely a matter of opinion." I would even be willing to express myself very strongly as believing that if we, as experts, cannot give anything closer than a mere opinion, our testimony or reports are not of great value to our clients. I believe, moreover, that by detailed geological examinations and with our latest knowledge of geological structure, principles of natural gas occurrence, etc., we can approach just as near a certainty in natural gas valuations as we can in oil valuations, and much more so than in the valuation of many metal prospects, in which only the upper few feet of the vein has been opened. In other words, we are getting nearer and nearer every year to bringing geology as applied to oil and gas development into the class of an exact science.

Mr. Wyer's paper gives an excellent outline for methods of leasehold valuation. Being presumably intended as an outline only, it can be expanded indefinitely by a detailed discussion of the methods of valuation for the four different classes of leaseholds which he mentions: (*A*) "Producing;" (*B*) "Protective;" (*C*) "Reserve;" and (*D*) "Prospective." This classification is probably as good as can be made in practice.

In the producing leaseholds in particular it is easy to establish a market price at which these leaseholds should be bought or sold. The question would arise in any case whether or not the leases are connected with a pipe line or the distance from the pipe line. Assuming that a pipe line already taps the property and that gas is already being sold for a certain figure, this price must be multiplied by the number of thousand cubic feet still "in sight" in the ground, from which is to be deducted (*a*) bonuses, (*b*) rentals, (*c*) royalties, (*d*) cost of drilling, (*e*) taxes and insurance, (*f*) overhead charges, etc., taking into account, of course, the number of years during which the property has produced and will probably produce, and paying particular attention to the decline curve for the particular wells and the property as a whole. It is also quite necessary to consider, in producing properties, whether these have been thoroughly drilled, or whether there is room for additional wells. The distance apart of the wells and the question of whether or not they have tapped the deepest known sands is important; also the pressure of the gas, its rate of decline, and whether or not it must be pumped. All these and many more questions must be considered in valuing a lease;

* Received Sept. 18, 1916.

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and when these facts are known, it is not a difficult matter for a natural gas geological engineer to place a definite figure on the property.

The real "protective leases" might be grouped with the "reserve leases," since if they contain natural gas they constitute part of the reserve supply; but if they do not contain gas, they are not necessary as a protection, and are a liability rather than an asset. Consequently, for practical purposes of valuation, they would probably be considered on nearly equal terms with the reserve leases. The value of these two groups of leases is to be figured on the basis of the number of successful wells which can be assumed for them; determined, *first*, from the positions of the reserve leases relative to the producing property, and *secondly*, on the basis of the continuity of the geological structure which has caused the accumulation of the gas.

Another factor which must be taken into account in valuing the protective and reserve leases is the degree of competition in drilling, which, if keen, will cause a serious decline of the pressure and volume of the gas. This is particularly true in such fields as the Hogshooter field in Oklahoma, the Mexia field in Texas, and certain other Oklahoma fields where a number of different gas companies were in the field, all of which picked up leases, many of them of small size, drilling them rapidly and sometimes inefficiently, exhausting the gas as fast as possible, causing a decline in pressure, with resulting influx of salt water into the sand along the lower level of the gas, and finally flooding out the entire field. These fields could have been operated for years, and their value would have been great, if possible to do so without competition; but conditions were such that an engineer on the spot could say definitely that none of the properties were of any great value, but that, on the other hand, they constituted a losing proposition. Such a field, however, if considered as a whole, would have had great value, since all natural gas engineers and geologists will unite in the conviction that monopoly is the proper thing in the natural gas business, and only by monopoly can large properties or fields be operated with financial success and economically for the community.

We therefore see, in *producing*, *protective* and *reserve* leases, that it is possible for an engineer to calculate very definitely the proper value which should be placed upon them in order to render their purchase or sale profitable. In the case of the leases which Mr. Wyer classes as *prospective* leaseholds, the difficulty is a little greater, perhaps, since these properties are frequently situated many miles from known gas production; and to a person unfamiliar with natural gas geology, there might be no basis on which to judge their prospective success and consequent value. This is where the geological engineer comes in, and no doubt exists that our science has now reached the standpoint where we can predict the chances in such territory with a reasonable degree of success. We cannot, of course, say in any particular field, as yet untested,

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whether it will *certainly* contain gas; because sands may be absent, even though the correct geological structure be present; but we can say, having a group of fields or leases, about what proportion of these will be successful and what value should be placed upon them; this being fully as definite and safe a proposition as to place a value on a particular security of an industrial corporation before the latter has started its machinery. The values in such cases are based on the actual net profits "in sight," with the addition of a certain percentage for prospective future business. This additional percentage is an instance parallel to the prospective outlying territory as yet undeveloped by a natural gas company.

We see, therefore, that natural gas valuation, if not already an exact science, is approaching this limit, and that there is no difficulty, at least in the case of a particular property or company, in fixing a valuation, if the properties be studied in the necessary detail. The sellers or purchasers may not always agree on this valuation, any more than they will in a gold mine, war stock or any other commercial proposition; but, nevertheless, it is possible for each side to fix the figure which the property is worth for the purpose desired.

One of the most important questions in valuing a natural gas company, and one to which very little attention has been paid, is that of salt water in the sands. This is sometimes a serious matter, especially in fields where drilling has been close and where the sands are rapidly depleted owing to the heavy draught of gas. A field where the sand is saturated with water below the natural gas level is quite a different proposition commercially, and one not nearly so attractive, as is a field where the sands are dry. In this respect, the natural gas business differs from the oil business.

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Problems Connected with the Recovery of Petroleum from Unconsolidated Sands

BY WILLIAM H. KOBBÉ*, ST. LOUIS, MO.

(New York Meeting, February, 1917)

I. INTRODUCTION

THE word recovery as used in this paper is applied in its broader sense and not limited to wells producing from horizons of unconsolidated sands. Certain problems connected with the winning of petroleum from such horizons present themselves while drilling is in progress and should be considered before drilling is commenced wherever unconsolidated sands are expected. Maximum recovery depends not alone upon efficient pumping methods but also upon the selection of a proper drilling system and the completion of a well especially adapted to the extraction of these oil-bearing sands.

II. DRILLING

DETERMINING FACTORS IN SELECTION OF METHOD

The selection of a drilling method for the development of a region in which the oil reservoir consists of unconsolidated sands is based on the following considerations: (1) Character of the formations overlying the reservoir; (2) thickness of the oil-bearing stratum or strata; (3) thickness and character of water or gas sands; (4) gas pressure to be encountered or expected; (5) total depth of well.

1. *Character of the Formations Overlying the Reservoir*

The strata overlying the reservoir may consist of beds of shales, clays, sands and other soft materials, or they may be slates, limestones and sandstones consolidated to all degrees of hardness. Very often the overlying strata are predominantly of soft materials but interbedded with harder formations. Likewise, soft strata are encountered at different horizons in an overburden consisting almost entirely of the harder sedimentaries. The physical character of this overburden is frequently

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complicated by the occurrence of quicksands, gypsum, rock salt or iron pyrites.

In deciding upon the surface equipment best adapted for the penetration of these various formations composing the overburden, the governing factor is based upon evidence or expectations that the strata are: (*a*) predominantly soft, (*b*) predominantly hard, or (*c*) about equally divided.

If it is known that the overburden consists of predominantly soft and cavey material such as sands, clays, soft shales, quicksands, beds of gypsum, etc., a rotary drilling rig of a size and type commensurate with the depth would be the most efficient method of overcoming such conditions.

On the other hand, if it is known that the overburden consists of predominantly hard strata such as limestones, sandstones, hard shales and slates, cable-tool equipment would be employed and a standard rig, with or without calf wheels, erected.

In many parts of the world it is impossible to classify the overburden either as predominantly soft or as predominantly hard. It is made up of many strata of each class. Such conditions are typical of the deep and difficult drilling in certain parts of California, for example at Fullerton and in portions of the Midway district. The only efficient method of drilling wells through 3,000 or 4,000 ft. of these mixed strata is the combination system employing both cable and rotary on the same rig. A heavy combination equipment, with a 24 by 112-ft. derrick, iron crown block, latest type of draw works and large rotary, frequently costs as much as a completed well located in the comparatively shallow portions of the Mid-continent or Eastern fields.

2. *Thickness of the Oil-bearing Stratum or Strata*

The thickness of the unconsolidated pay sand that must be penetrated is an important factor in the selection of the drilling system. One hundred feet of loose oil sand carrying even a moderate gas pressure often necessitates the use of rotary equipment notwithstanding the fact that a decision based solely upon the physical character of the overburden would select cable tools for the work. For example, in the extreme northern portion of the Midway field in the San Joaquin Valley, California, there is 700 ft. of predominantly hard cable-tool overburden which must be penetrated in order to reach an unconsolidated oil sand 280 to 300 ft. thick. Experience has shown that the rotary drilling method completes a well in that district in half the time and at less cost than cable tools. This for the reason that the former system averages more hole per day from top to bottom than the latter although more time is required by the rotary to reach the sand. The cable tools have no difficulty "making hole" through the 700 ft. of predominantly hard

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overburden but encounter the greatest difficulty in making headway in the oil sand. In fact, the heaving nature of these sands causes innumerable fishing jobs and frozen strings of casing when an attempt is made to overcome them with cable equipment. After fighting them for weeks, if the standard driller has 200 ft. of pay to his credit he is doing well, whereas the same driller on a rotary rig can wash through the 300 ft. of sand in one tour of 12 hr.

These conditions indicate the important bearing the thickness of the unconsolidated pay sand has upon the problem of selecting a drilling system.

A few miles south of the area just described the oil sand is only 10 ft. thick and underlying similar formations of hard strata. Here the cable tools are far superior to the rotary because they can readily overcome the thin bed of pay sand and are better adapted to drilling through the overburden.

Therefore, it is seen that the mere thickness of unconsolidated oil sand may be the controlling factor in classifying the physical nature of the territory to be drilled.

3. *Thickness and Character of Water or Gas Sands*

Cable tools are superior to the rotary for prospecting new territory and for determining the exact depth of water- or gas-bearing horizons. It sometimes happens that an area or pool has been inefficiently developed and exploited; that available well records are unreliable, or that the lenticular nature of the sands causes great uncertainty regarding the exact depth of water strata. Under such conditions it is advisable to employ cable tools until all necessary data have been obtained, when the drilling method may be advantageously changed to the rotary. Conditions such as these at one time existed in portions of the Burkburnett field in northern Texas and although it was known that the territory was particularly adapted to rotary drilling it was necessary to "feel out" from proven areas with cable tools on account of the "spotted" nature of the oil and water sands.

4. *Gas Pressure to be Encountered or Expected*

Gas pressure is a factor of the utmost importance in the selection of a drilling system. Other things being equal, the greater the pressure the greater its importance; not only from the mechanical standpoint but to serve the ends of conservation. This pressure may occur above the oil reservoir, within the reservoir, or both. When it is known to occur above the oil sands it may be cased off with cable tools and conserved between strings of casing by the use of a packing spider, a special

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device which has met with great success in the deep and high-pressured territory of southern California.

Where high gas pressures exist in formations difficult for a rotary to overcome it frequently becomes necessary to utilize special methods in conjunction with the cable system to properly cope with the situation. For example, the mud-laden fluid method and circulator systems may be called to the aid of the cable tools in passing through strata under high gas pressure.

The rotary system, however, is the ideal method of overcoming gas sands and should be employed wherever it is possible to drill by this method. In portions of the north Midway field the pressure within the unconsolidated and very loose oil pay is sufficient to heave the sands 200 to 300 ft. up in the casing of cable-tool wells unless special safeguards are introduced, whereas the rotary with its column of mud slip penetrates these sands with the greatest ease and dispatch.

5. *Total Depth of Well*

This is naturally an important factor in the selection of a drilling system, whether the oil sands to be developed are unconsolidated and loose or hard and compact. No attempt will be made in this paper to describe the many different types of rigs and methods of drilling. As has already been pointed out, the character and thickness of the oil sand often modifies the selection of any particular drilling method but the same sand at 1,000 ft. requires lighter equipment and possibly a different system than if it occurred at 4,000 ft. The former may be easily reached with cable tools and a 20 by 84-ft. standard rig while the latter may require the heaviest rotary or combination equipment with a 24 by 112-ft. or even 120-ft. rig, 7½-in. Ideal rig irons, calf wheel with sprocket drive, etc.

Wildcat Territory

In the foregoing paragraphs it was assumed that developments of unconsolidated oil sands was to be undertaken in proven territory–that is, in a region already known and where accurate well logs were available for study and correlation. This is a very frequently not the case. Oftentimes little or nothing is known concerning underlying strata and unless geological evidence enables the construction of a fairly accurate columnar section of the region the selection of a drilling system is practically limited to cable tools. As has already been stated, this is the only method of procedure in unknown or wildcat districts because the cable system is peculiarly adapted to prospecting and the accurate determination of oil-, water- and gas-bearing strata.

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Johnson and Huntley[1] make a good comparison of the advantages and disadvantages of the cable and rotary systems which indicates this superiority of the former for work in wildcat districts.

Comparison of Drilling Systems

Cable System.—Advantages.—

1. Less first cost of tools and rig.
2. Lower labor cost per day.
3. Less water necessary.
4. Can drill in the hardest rock.
5. More drillers available in some fields, although this is becoming less true.
6. Gives more information as to the formations passed through, and is thus better for prospecting.
7. Less cost per foot for relatively shallow wells.

Disadvantages.—

1. Longer drilling time.
2. Much slower when under-reaming is necessary.
3. Danger of delays and fishing troubles in soft strata.
4. When many water sands, hard to carry large hole to deep pay.
5. Greater cost per foot for moderately deep wells.
6. More casing necessary to handle caves and water sands.
7. Liability of getting crooked hole in soft formations.
8. Harder to control heavy pressures and more likelihood of "blow-outs."

Rotary System.—Advantages.—

1. Faster drilling in soft strata.
2. Less trouble from caving and water sands.
3. Less casing used in soft formations with water and gas sands.
4. Straighter hole in deep drilling in soft formations.
5. Can handle alternate hard and soft formations, with less danger of accidents than with cable tools. This is made possible by the new bits and heavier rotary machines.
6. Can carry a large hole deeper.
7. When "drilling in," easier to control high gas pressure and prevent blowouts.

Disadvantages.—

1. Very slow in hard strata.
2. Greater daily labor cost.

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[1] R. H. Johnson and L. G. Huntley: *Principles of Oil and Gas Production*, pp. 118–119. New York, Wiley (1916).

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3. Limited trained labor supply in some fields.
4. Greater cost per foot for shallow wells.
5. Does not show up smaller oil and gas pays, and important reservoirs may be passed through in prospecting.
6. More water necessary, a drawback in arid regions.

Cable System Generally Used in Wildcatting

Good practice demands that cable tools be used on any test well in an unknown or wildcat district. If the approximate depth of the stratum which is thought to be oil-bearing is known, or if the depth is decided by contract with the landowners, a standard or calf-wheel rig is erected of a size and type commensurate with that depth and drilling commenced with cable tools. If the territory proves to be suitable for cable-tool work and stands up, the well progresses satisfactorily and rapidly as a rule but if caving and unconsolidated sands are encountered, complications surely follow. These unforeseen contingencies will be treated briefly.

A test well passes through 1,500 ft. of hard formations and enters a stratum of unconsolidated oil-bearing sand under pressure. The 10-in. casing is landed at 1,450 ft. as a water string to shut off several water-bearing strata encountered at higher levels. The 8¼-in. casing put in after landing the 10-in. becomes "logy" and finally "freezes" when 40 ft. of the sand has been drilled.

The foregoing case is typical and one taken from actual practice. It is essential to keep the casing "free" when carrying hole through unconsolidated sands, whether pay sands or not. Sometimes 10 ft. of sand will freeze casing and in other cases with careful management in experienced hands 150 to 250 ft. of heaving oil sand may be overcome with cable tools.

In the example just mentioned, the 8¼-in. casing is frozen. The first thing to be done is to attempt freeing it. Alternate pulling and driving may accomplish this. The power of a calf wheel applied through seven lines to the heavy casing block is tremendous and an expert driller knows just the amount of stretch the casing will withstand before parting. If it comes an inch or two the drive clamps are adjusted on the square of the stem and the casing driven back to its former position, when it is again pulled—possibly yielding an additional inch. The principal thing is to cause it to move even slightly, as it is sure ultimately to become free by this method. It is well to apply the casing tongs after each pull or two and set up the entire string a trifle. This tightens any threads that may have become loose.

It frequently happens that the alternate pulling and driving method proves futile. The casing refuses to move an inch. In this event other methods are at the command of the engineer. One that is often success-

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ful and is not commonly known is to relieve the static pressure within the casing, it being presupposed that the hole is full or nearly full of fluid, and allow the sand to heave within the drilling string. It is seldom necessary to bail more than 300 or 400 ft. of fluid from the well in order to so disturb the balance of pressures that a sudden upheaval of the oil sand takes place. Following this upheaval the driller tries the casing and if it is free, which is nearly always the case, the hole is cleaned out to bottom and drilling resumed.

It sometimes happens on account of insufficient pressure in the sand, or from other causes, that the sand fails to heave when the static pressure is reduced. In that event a casing spear, preferably of the trip type, is run to bottom on the tools and engaged near the shoe, with long stroke jars and sinker bar or stem giving necessary impact to the jars. A strong pull is then taken with hydraulic jacks applied to the casing by means of a spider, while jarring is commenced on the tools. This method will free exceedingly tight casing. A nail may often be driven out when it cannot be pulled. The jarring of casing is exactly similar.

If this method fails to free the casing, a spear may be run on a string of the next smaller size casing and a hold secured near the bottom of the frozen pipe. A heavy pull is then taken on both strings of casing while jarring is commenced with the tools which carry long stroke jars and a casing spear as described in the previous method. In fact, the two methods differ only in that a much stronger pull can be taken on the two strings of casing than on the one; in other words, the pull is doubled.

In rare cases it happens that the unconsolidated sand holds the casing in such a grip that even this tremendous pulling force and the heavy jarring, which may be likened to the blows of a steam hammer, fail to free it. As a last resort the casing may be split in several places to allow the binding sand to run into the well. To accomplish this a casing ripper is run in on the tools and several long gashes cut in the casing at a point where it is thought the maximum "friction" exists. If this results in the entry of sand it will nearly always free the string.

In the event that all these attempts fail and it is impossible to free the casing, two methods of overcoming the difficulty remain. A four-wheel casing cutter may be run in on 3-in tubing and the casing cut at a point just above where it is frozen. The upper portion of the string is then removed from the well, a new shoe adjusted and the joints imbedded in the sand "side-tracked" or drilled past. This is slow and difficult work and not always successful. Some operators shoot the casing instead of cutting it. A small charge of dynamite is exploded at the point where the casing is frozen, but this results in jagged ends at the place of rupture, making the casing more difficult to side-track. It is a quick and inexpensive method but cannot be considered good practice in many cases.

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If it is not desired to cut or shoot the frozen string, the only thing to be done is to abandon it and case with the next smaller size pipe. This is permissible providing the frozen string is of such a size, say 8¼ or 6⅝ in., that the well may be completed to the required depth before "pointing out."

Loose oil sand under pressure often heaves in a cable-tool drilling well when the bit drills through the cover rock, and unless the driller is warned in time the tools are buried for many feet by the sand. In certain of the California fields the oil sand has often buried a string of tools 300 ft. or more and frequently led to a bad fishing job. Whenever such sands are anticipated the experienced driller "carries" the hole full of water or thin mud slip in order to counterbalance the rock pressure. The jars and sinker bar are also used on the tools so that in case of a sand heave they may be jarred free. It is sometimes necessary to jar the tools through 100 ft. or more of oil sand, which requires possibly 12 to 24 hr. If the wire drilling line parts a bad fishing job is apt to result and it may be necessary to pull the casing (if it is free) in order to recover the tools. Or if it is frozen a "fishing string" is run in, after cleaning out to the top of the lost tools, and an attempt made to recover them with a socket or other special fishing tool. This is simply an example of one of the many fishing jobs of endless variety that may occur from the heaving of unconsolidated oil sand. These same remarks apply with equal force to conditions arising from the penetration of any unconsolidated sand or other material of a cavey nature which may freeze casing or bury cable tools.

The foregoing problems connected with the drilling of wells with cable tools in unconsolidated sands are, of course, not met with when the rotary system is employed, but it must be remembered that wildcat territory, which demands the use of cable tools, is being considered. The difficulties encountered were unforeseen and unexpected. After the cable tools have proved the district, future exploitation may be carried on by the rotary system.

Rules Governing the Drilling of Deep Test Wells

A few general rules and cautions governing the drilling of deep test wells in wildcat territory where unconsolidated sands are expected, follow:

1. Allow a wide margin above the calculated cost of the well.
2. Commence drilling with a sufficiently large hole—20 to 22 in. for a deep test.
3. Do not expect a light rig for shallow territory to drill a deep test.
4. In remote districts have all the casing on the ground and an adequate supply of small tools, pipe and fittings.
5. Have a rope grab, combination socket and other frequently needed fishing tools available for instant use.

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6. After selecting the site for the rig, have a cellar dug about 8 by 10 ft. by at least 15 or 20 ft. deep. The depth is important.

7. Provide an adequate supply of the best obtainable water for steaming purposes.

8. Anticipate heavy oil or gas pressure and have a control casing head on hand if occasion arises for its use to cap the well.

9. Locate a deposit of fine clay for use in mixing mud slip to shut off pressures or to use while drilling in unconsolidated sands that refuse to bail readily.

10. If the well is to be drilled by a contractor at so much per foot, do not expect sands tested, water shut off, and logs kept as carefully and efficiently as would otherwise be done. Many important features of the work are slighted and the time required for their proper accomplishment sacrificed in the effort to make the maximum amount of hole in the minimum time.

11. Carry every string of casing to the maximum depth possible and never abandon the effort until every means at the command of the operator has been exhausted in the attempt.

12. The management of the work, selection of methods, etc., should not be left to the drillers but placed in the hands of a competent superintendent.

13. Keep a most careful and accurate log of the well, preferably in several copies in order that if one is lost the record may be preserved.

Taking up some of these points in more detail: In allowing for the cost of a test well in wildcat territory a safe rule to follow is to figure the actual cost as accurately as possible and multiply this figure by two. How many wells are reported as "abandoned for lack of funds?" It is safe to say that unconsolidated sands or other cavey materials are responsible for a large percentage of the failures and for most of the difficulties, discouragements and delays in the drilling of test wells. Many wells that "ought not to cost $15,000" actually require the expenditure of $25,000 or $30,000 for their completion. Much larger amounts are not at all unusual and it is well to keep in mind that figuring the cost of a test well in a wildcat district is a good deal like provisioning an exploring party to penetrate unknown wilds—emergencies must be anticipated in both cases.

The importance of commencing the well or "spudding in" with a sufficiently large hole cannot be over-estimated. In order to reach the desired depth, whether 2,500 or 4,000 ft., it is imperative to make the size of the hole at the surface commensurate with the depth. In the soft Tertiary formations of California and in the Baku fields of Russia it is found advantageous to use large-diameter stovepipe casing for the first few hundred feet, especially where beds of boulders are encountered. This type of casing is made by riveting sheet iron or steel of 14 to 10 U. S.

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gage and may be purchased in lengths of 2 ft. or multiples of 2 up to 20 ft., the shorter lengths being used when progress with the string becomes slow. In the Russian fields, wells are commenced with holes of extremely large diameter, not on account of any extraordinary depth but in order to complete a well in the pay sand of sufficient diameter to permit the use of large-sized bailers, the method used in that country to bring the oil to the surface, because the excessive amount of unconsolidated sand precludes the use of pumps. The usual sizes of the California stovepipe casing are 16 in., 18 in. and 20 in. and if 500 to 700 ft. of the 18-in. size can be put in a well the operator has a good beginning for a deep test in unconsolidated formations. A great advantage possessed by this type of casing is the absence of collars with a consequent reduction in friction, making it especially suited for driving through strata of unconsolidated sands. Its use will probably extend to Oklahoma and the Mid-continent fields as development extends westward into deeper territory possessing all the problems arising from the presence of unconsolidated sands or other strata of loose materials. Many test wells have been and are being drilled in the Permian Red Beds of western Oklahoma and the occurrence of deep deposits of unconsolidated materials in this region is gradually bringing about improved drilling methods and the introduction of more efficient mechanical devices. For the same reason, operators accustomed to the hard strata of the Eastern fields where casing problems are of small importance are realizing the necessity of large-diameter holes and the use of heavy-weight pipe with improved methods of handling it. Commencing a test well with too small a diameter means that it "points out" before it "tests" anything. In Pottawatomie County, Oklahoma, a typical wildcat section, are many abandoned "test" wells that pointed out before a depth of 2,500 ft. was reached, although it was known that oil, if it occurs, would be found at 3,000 ft. or more. These failures were all due to the fact that the holes were commenced with diameters too small and that the presence of unconsolidated sands was not taken into account. These sands necessitated the use of a greater number of strings of casing which in turn caused the well to point out sooner than would have been the case had it been commenced with a hole of larger diameter. "It may be said in conclusion that in drilling in wildcat territory where the number of water and gas sands is unknown and the depth of the oil sand uncertain, and therefore the ultimate depth of the hole cannot be known, one must start with a larger size of casing than that which will probably be used for later wells when the field is developed. Pioneer wells have been drilled in some pools, which failed to discover oil because the hole was so small it could not be carried deep enough to penetrate the sand which was later discovered to be the main oil pay, perhaps only a few feet beyond where the early well stopped drilling. Wells in the Calgary dis-

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trict in Alberta are started with 18-in. casing. In California even larger casing has been used."[2]

Probably the best type of rig for handling heavy strings of casing in unconsolidated sands is the California or calf-wheel with sprocket drive and extra heavy rig irons. A light standard rig such as is commonly used in the Eastern fields and parts of Oklahoma is sufficiently strong to handle light-weight strings of casing through consolidated formations but is totally inadequate for the heavy pulls required to free long strings of California pipe in contact with several hundred feet of unconsolidated sands. For the heaviest work, iron or oak-calf and bull-wheel shafts are required, and concrete foundations for the derrick. The usual practice of placing a few pieces of waste lumber under each derrick leg as foundations should not be countenanced, especially where the casing may have to be carried through cavey material with the consequent heavy strains which inevitably result in pulling the derrick out of plumb.

A feature connected with cable-tool drilling in unconsolidated sands which many operators, accustomed to hard formations, overlook, is the importance and advantage of an adequate cellar. When a string of casing is being carried through sands or other cavey material it has to be "worked" almost continually and sometimes freezes while one "screw" is being run. It is difficult or impossible to make any hole ahead of the shoe and frequently the casing is allowed to "follow;" that is, it is released from the spider slips and follows the drilling bit of its own weight. If it refuses to follow it is worked up and down with the casing block and calf wheel and sometimes spudded with a jerk-line or driven with the drive clamps and tools. In all of these operations a deep cellar is of the greatest advantage because it allows the driller to work the casing below the derrick floor until there is sufficient clearance for another joint to be screwed on. Casing averages about 20 ft. to the joint, hence a cellar of approximately that depth is most desirable, although 15 or 18 ft. is a great improvement on the inadequate and shallow pits commonly placed under derricks in regions where the problem of unconsolidated sands is unknown or under-estimated.

Finishing a Well in Unconsolidated Sands

The process of finishing a well in an unconsolidated pay sand is quite different from the "bringing in" of a well tapping a hard sand. It sometimes requires weeks to penetrate a thick stratum of loose pay sand with cable tools, whereas the "drilling in" and "shooting" of a hard sand is a much simpler process. Where the sand, whether loose or hard, is under great pressure and the well "comes in" as a gusher or flowing well

[2] R. H. Johnson and L. G. Huntley: *Principles of Oil and Gas Production*, p. 125. New York, Wiley (1916).

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as soon as the sand is tapped, the results are exactly similar except that the well in the unconsolidated pay is apt to throw out great quantities of sand.

The ideal method of finishing a well through a thick stratum of loose oil sand is the rotary system. As mentioned in the first part of this paper, it is usual for a rotary to penetrate 300 ft. of unconsolidated oil pay in 12 hr. or less, and by thickening the circulating mud the pressures may be balanced to such a nicety that upon reaching the stratum underlying the sand the entire string of drill pipe may be withdrawn without the slightest danger of caving. In other words, a column of mud-laden fluid extends through the sand and is substituted in the physical balance of forces for the loose sand which previously occupied the same space. Although all the purposes of an open hole have been served, nature's balance is left undisturbed. This mud may be allowed to stand for several hours but the best practice is to prepare the "liner" or perforated casing in advance and have everything ready for "casing" immediately upon withdrawal of the drill pipe.

The liner may consist either of "shop-perforated" casing or one of the several types of screen pipe which have come into extensive use because of their greater efficiency in handling sand with a consequent increase in the amount of oil recovered.

It will be assumed that screen pipe is to be set and that the water string consists of 8¼-in casing landed in a hard stratum a few feet above the oil sand. Three hundred feet of 6⅝-in. screen pipe is carefully tallied and two blank joints of the same size made ready. A collar is screwed on the bottom of the last joint of screen pipe and a back-pressure valve and wooden wash plug placed therein. The 300 ft. of screen pipe with the two blank joints on top is then run into the hole to bottom on 3-in. tubing, this tubing resting on the wooden wash plug and being attached to the 6⅝-in. casing with a right- and left-thread nipple. The principal thing is to have the tubing extend through the screen pipe in order that the water may pass out of the bottom of the liner and not through the meshes of the screen. When the liner has reached bottom it is raised an inch or two and the well thoroughly washed by pumping clear water through the tubing or wash pipe for 24 hr., or until the "returns" cease to show any trace of mud. The wash pipe is then backed off by turning to the right and withdrawn. This leaves the liner clean, with no mud between the meshes and the oil sand, and the two joints of blank 6⅝ in. extending up into the 8¼ in. forming a 40-ft. lap. An adapter from 6⅝ in. to 8¼ in. is then lowered on the bailer or dropped through the water in the casing until it rests on top of the liner, making a smooth connection between it and the 8¼ in. The well is then tubed and when the water pressure is relieved begins to produce oil.

The proper setting of screen pipe with a rotary requires experience

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and knowledge, especially as quick work is desirable and any accident may prove costly. Instead of using tubing, a heavy liner, whether of screen pipe or perforated casing, may be set with the rotary drill pipe, as the same method is used with both.

The finishing of a well with cable tools in a very thick stratum of unconsolidated oil pay is much more of a problem than that just described where a rotary is used. The ordinary procedure, especially in districts where screen pipe has never been tried or is unknown, is to carry the casing, of the size the hole is to be finished with, through the oil sand and then perforate it with a perforating machine. It frequently happens that the string freezes after penetrating the sand for 100 ft. or less, in which event it is usual to place an iron or wooden heaving plug in the bottom of the casing and then perforate. If the operator is fortunate enough to carry the casing through the pay it can be landed in the stratum underlying the sand, which obviates the use of a heaving plug, the only function of which is to prevent the sand heaving inside the casing and overwhelming the pump. Irrespective of the depth of sand attained, the use of the perforator is necessary with this method and is its greatest drawback, mainly on account of the uncertainty of the results therefrom. For mechanical reasons, variation in the thickness or toughness of the casing, and other factors beyond the control of the operator, the use of a perforating machine is frequently disappointing. If the well comes in as a small producer there is always the suspicion that the perforator failed to punch a sufficient number of holes, whereas if the casing "sands up" and the well requires constant "pulling" the reverse is true. Arnold and Garfias[3] mention these points in describing methods of oil recovery in California:

"The indiscriminate use of perforating machines has been the source of much trouble. In the Coalinga field one well about 1,300 ft. deep was perforated three times, and when the casing was removed it was found that, except for a few holes, the machine had only indented the casing. In other wells, owing to local brittleness of the casing, the perforator has removed large pieces of it; and in some deep wells, owing to the great weight of the column of tubing to which the perforator is attached, the perforator has cut into or strained the collars so as to cause collapse of the casing."

In the light of these facts and of past experience in drilling many wells in unconsolidated oil sands, it may be said *that every effort should be made to avoid bringing in the well by perforating the casing after it has been placed in the hole.* As a substitute for such perforating the operator may use screen pipe or shop-perforated casing. In finishing a cable-tool well with either of these two devices the problems involved are largely or entirely dependent upon the thickness of the unconsolidated sand. If this sand is a thin bed 50 ft. or less in thickness there is usually no difficulty in

[3] R. Arnold and V. R. Garfias: Methods of Oil Recovery in California, *U. S. Bureau of Mines, Technical Paper* 70, pp. 9–10 (1914).

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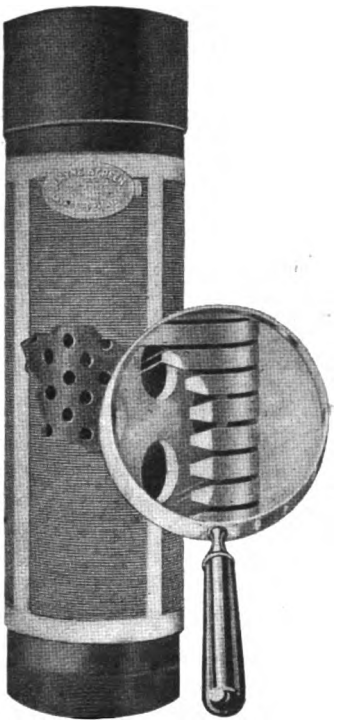
setting any type of screen or of shop-perforated casing, but where the sand is 250 to 300 ft. thick many obstacles may be encountered.

The ideal method of finishing a cable-tool well in a very thick stratum of loose pay sand is to carry, say, 8¼-in. casing *through* the pay and then set a 6⅝-in. liner, either screen or shop-perforated, preferably the former, inside this 8¼ in. and then remove the 8¼-in. casing, leaving the liner in contact with the oil sands. This is very difficult to accomplish, however, because it necessitates keeping the 8¼ in. free at all



After Layne & Bowler Corp.
FIG. 1.—SECTION OF WIRE-WOUND SCREEN CASING.

After McEvoy Wireless Well Strainer Co
FIG. 2.—SECTION OF WIRELESS SCREEN CASING.

times, and this is seldom possible. In fact, it may freeze beyond all power of freeing while the liner is being set.

It well may be asked, why not carry 8¼-in. screen or shop-perforated pipe through the sand in the first place and thus save the extra labor of setting a liner and gain the advantage of completing a well of larger diameter. The answer is that it is much more difficult to carry perforated than blank casing through unconsolidated sands and that all types of screen pipe possess the same objection. In addition, certain screens are not made to withstand such work and are easily damaged, necessitating the use of a conductor casing when setting.

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Screen casing may be divided into two general classes: (*a*) the screening device on the outside of the casing, see Fig. 1; and (*b*) the screen forming a part of the casing wall, see Fig. 2. In the first type the screen generally consists of wire wound around perforated casing, while in the second type the screen is in the form of numerous buttons or slotted brass plugs carried in the walls of the casing and flush with the outside surface, or in slits cut directly in the casing wall itself. This latter type, sometimes known as "wireless" screen, has the advantage of withstanding hard usage, and many operators have carried it for considerable distances through unconsolidated sands. But in very thick strata of loose pay it is as difficult to carry as shop-perforated casing and entirely impossible where blank pipe encounters difficulties. The wire-wound screen casings are entirely unadapted for carrying through thick strata of unconsolidated sands and the attempts to finish a well in that way should never be made. The wire screen, however, possesses many advantages over the wireless, as will appear in later paragraphs, but in finishing a well with this type of screen it is necessary to set it either inside a conductor string or by means of a rotary, as otherwise the wires may become detached from the casing and cause endless complications and trouble.

In concluding the subject of the drilling and finishing of wells in unconsolidated sands, it is desired to emphasize the fact that most of the problems connected therewith arise from two general causes: (*a*) the development of unknown districts; and (*b*) the existence of such a combination of physical conditions in the strata that it is impossible to employ one system capable of overcoming them. For example, if a thick body of unconsolidated oil sand has an overburden of 1,200 ft. of hard compact formations the cable system demanded by the latter is confronted, upon entering the oil sand, by many of the problems already described. These conditions would not justify the installation of an expensive combination system and could best be overcome only by the exercise of good judgment and foresight backed with skill and experience. Even though possessing these qualifications in high degree, the seeker for oil must recognize the element of "luck" in many of his operations and be duly appreciative when it is in his favor and stoically patient when it is otherwise.

III. EXTRACTING THE OIL AND SAND

The process of obtaining oil from the earth when that oil is contained in a loose unconsolidated sand is very different from the method employed to extract the recoverable portion of the oil content of a hard stratum. *With an unconsolidated sand the extracting of its oil content may take place above ground whereas with a hard oil-bearing stratum this is impossible.* This distinction has an important bearing upon all questions relating to

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oil production. It is the factor deserving the greatest consideration in determining the efficiency of methods employed for the maximum recovery of oil from unconsolidated sands and is the basic principle of the statement that: *Other things being equal, the maximum recovery of oil from an unconsolidated sand is directly dependent upon the maximum recovery of the sand itself.*

The truth of this law is based upon two important considerations: (1) The removal of sand by a well causes a larger volume of sand, and therefore of oil, to move toward that well than would be the case if oil only were removed. (2) The efficient separation of sand and oil, and therefore a nearer approach to obtaining the recoverable oil content, is made when this separation takes place above ground.

The Movement of Sand and Oil Toward a Well

The higher the viscosity and the lower the gas pressure within the oil reservoir the greater becomes the importance of creating and maintaining a movement of sand toward a producing well. This has been proved beyond all question in extensive operations in the north Midway district of California where the oil is of 14.5° Bé. gravity with a very low gas pressure.

"In the Balakhany oil district, where the oil is heavy, and the gas has nearly all escaped from innumerable wells, the concentration of the petroleum toward old producing areas is very marked, and whilst old bailing properties, which have for many years yielded a moderate payable production show little or no fall-off, new lands interspersed between these are almost valueless and give practically no output when new wells are sunk. This is additional testimony of the movement of oil towards spots where immense quantities of sand have been removed by bailing, creating areas of low density into which the petroleum, no longer assisted by a gas pressure, percolates by gravitation. In one case, a new well bored in the Balakhany district was a distinct failure, for stratum after stratum failed to yield even a mean production, but an old well which had been bailed for many years before abandonment, gave, when cleaned out, the extraordinary production of 2,500 poods a day for more than a month, and then only fell off by degrees to 400 poods daily."[4]

The fact that large producers come in without "making sand" evidences the existence of high gas pressure and insufficient means of entry for the sand to pass into the casing, but it is not proof that the large production is due to the absence of sand or that the output would not be increased if sand were expelled with the oil.

With proper mechanical arrangements a well tapping a thick stratum of unconsolidated pay produces a tremendous amount of sand. That it continues to do this for long periods is proof that a constant replenishment takes place near the casing, necessitating a movement of sand and

[4] A. B. Thompson: *The Oil Fields of Russia*, p. 51. London, Crosby Lockwood and Son (1908).

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oil toward the well. This movement is frequently of such volume, or occurs so suddenly, that the casing penetrating the reservoir is damaged or completely severed from the upper and more rigid portion of the string. Several instances of such damage may be mentioned: In one case 250 ft. of 36-lb. 8¼-in. casing was twisted somewhat in the shape of the letter S and was removed from the well only with the greatest difficulty. In another instance several hundred feet of 6¼-in., 20-lb. casing was broken just below a collar and displaced to such an extent that no part of it was touched by the tools while repairing the damage. Several cases may be cited where casing has been so deflected or bent by sand movements that it was impossible to tube the well beyond the point of flexure. No type of casing is proof against this force of moving sand, and both screen and perforated pipe suffer, although it is believed the former less so than the latter. Likewise it appears to make but little difference whether the casing passes entirely through the sand and is anchored in an underlying stratum or penetrates for only half the distance.

Methods of Extraction

In order to bring about a movement of sand with its oil content toward the well, it is necessary to provide means for its efficient removal with special casing devised to facilitate this process. If the sand cannot enter the casing, not only is the movement of oil retarded but the sand surrounding the casing is partially drained of its oil content, resulting in a constantly increasing body of "dead" sand opposed to the richer portions of the reservoir. Instances are known where a well, because of faulty perforations or inadequate screen openings, produced small quantities of clean oil free from sand for several years and when an attempt was made to substitute screen with proper-size mesh the sand had become so "dead" in the vicinity of the well that it lacked the "life" to enter the casing and continued to obstruct the path of better "pay." The same conditions apparently explain similar failures when a well was reperforated, but in such a case there is always the element of doubt that arises whenever a perforating machine is used. These cases occurred with a very loose oil sand containing an asphaltic crude of 14.5° Bé. gravity and were undoubtedly influenced by nearby wells having established strong drainage channels. In any event, the fact remains that they produced no sand and very little oil, and that these conditions could not be improved. Furthermore, there was no local variation in the reservoir or other controlling factors to account for this phenomenon.

On the other hand, when a well is properly equipped and produces great quantities of sand with the oil a large area of lower density is gradually established around the casing and, unless encroaching water is present in quantity, this removal of sand must inevitably lead to the for-

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mation of a cavity. This cavity probably assumes the form of an inverted cone as sand continues to be removed while new sand with its oil flows by gravity down the slopes of this subterranean funnel and supplies the well with fresh material. The shape of the cavity is undoubtedly influenced by the dip of the strata, when marked, and by the disturbing effect of nearby wells. That a cavity forms, however, cannot be disputed. Sand slips, damaged casing, and collapse of the cover rock evidence such a cavity in addition to the impossibility of otherwise explaining the effect of removing the tremendous volume of sand that wells have been known to produce.

It would be interesting to know when the slopes of these cavities reach an angle of repose and experiments with saturated oil sand on the surface should prove most instructive. The fact that wells producing from unconsolidated pay sands gradually diminish in their output of sand may indicate the approaching stability of these underground slopes, and evidence presented in later paragraphs strengthens such belief.

Having attempted to show the necessity, not only of removing sand in order to recover the maximum quantity of oil, but the favorable results of such removal in cavity formation with its accompanying low pressures and "live slopes," the mechanical handling of this sand will now be described.

Function of Screen Casing

Screen casing of proper mesh serves its best usefulness in permitting the *regulated entry of the maximum amount of sand and oil* while excluding gravel and small stones which would "sand up" the pump by sticking the valves, or otherwise prevent its proper action. It probably serves another use in that it results in a more even distribution of the oil and sand channels radiating from the well than is the case with ordinary perforations. This even drainage of sand and oil equalizes the pressure in all directions around the casing, whereas the use of a perforating machine may result in the sand being drained from one side more than the other, thus creating a pressure against the casing. This unequal drainage due to faulty perforation is an argument for the use of screen pipe and a possible explanation of deflection and damage to oil strings.

A section of wire-wound screen casing is shown in Fig. 1. This type of screen utilizes "keystone wires" whose function is to prevent clogging of the mesh—the space between the wires increasing radially and thus permitting any substance that enters to pass through unhindered. An exceptionally large screening surface is exposed to the sand with this type in that the wires between the perforations are utilized as fully as those directly over the openings.

Fig. 2 shows a section of wireless or button screen which is especially

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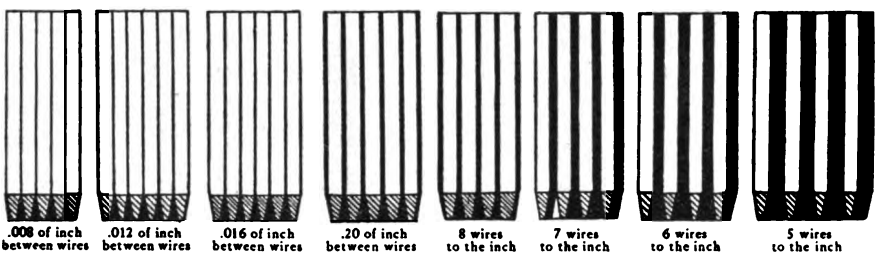
adapted for work with cable tools, as it will withstand hard usage and the driving and pulling often necessary with that system in drilling through unconsolidated sands.

The selection of the proper-sized mesh depends upon the physical character of the sand, viscosity of the oil and the gas pressure. The mesh should be of sufficient size to allow the maximum entry of sand and oil while excluding pebbles and small stones. This is best determined by practical experiment above ground. Other things being equal, a high-viscosity oil demands a coarser mesh than one of low viscosity. A screen made up of five wires to the inch (see Fig. 3) proved most efficient with a moderately fine sand carrying oil of 14° to 15° Bé. gravity. The erroneous belief that the function of screen casing is to prevent the entry of sand in oil wells, with the consequent avoidance of pumping troubles,



After Layne & Bowler Corp.

FIG. 3.—SHOWING DIFFERENT MESHES OF A WIRE-WOUND SCREEN.

leads to the selection of too fine a mesh in many instances. *The mesh should be too coarse rather than too fine.* Screen pipe came into use and was devised in connection with water wells and it was natural that the same rules were thought applicable when it was introduced in the oil fields.

PUMPING THE SAND AND OIL

Assuming that a well has been properly drilled, thoroughly washed, screen casing set, and that sand and oil are passing freely through the mesh, the most efficient device to bring the sand and oil to the surface is the plunger pump (Fig. 4). Although many wells flow naturally for a time or may be agitated by periodically moving an "agitator string," and others containing much water may be pumped with an air lift, the standard equipment for handling sand is the plunger pump. This device consists essentially of an outside cylinder or "working barrel" and a hollow plunger with upper and lower valves (Fig. 5). Although simple in principle, these pumps are very carefully made of the best material and ground to an exact fit, and their proper installation and operation require experience and knowledge in order to obtain the best results in

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FIG. 4.

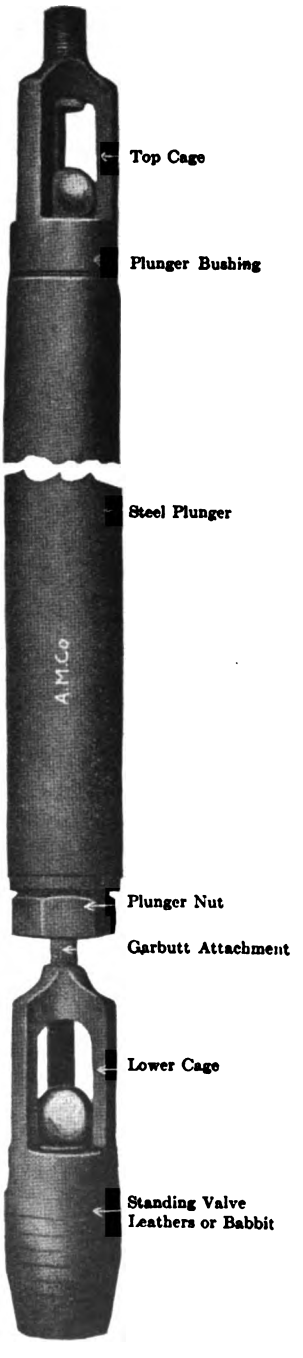

FIG. 5.

FIG. 6.

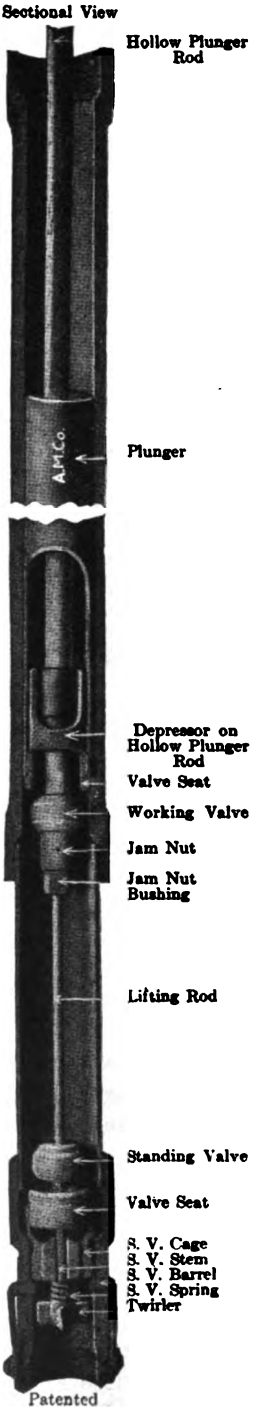

FIG. 7.

For description, see foot of next page.

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the removal of sand. Fig. 4 shows a pump completely assembled and ready to be attached to the tubing. The working barrels are made in lengths from 42 to 60 in. and from 2 to 4 in. diameter. A common size is 3 by 60 in. for the "renewable" or liner pump with extension and top collars at both ends (Fig. 6). This is one of the best types of pumps for handling sand, as the upper portion of the plunger does not leave the barrel and therefore is not in contact with sand on its downward stroke. The standing valve barrel is carried on a 3 by 24-in. nipple which allows the plunger to pass below the lower end of the barrel on each stroke, thus retarding the entry of sand between plunger and barrel.

The Parker pump shown in detail in Fig. 7 is said to be particularly efficient in handling sand on account of the large area of the valve openings and the positive action of the valves.

Pump Operation

A properly installed and operated plunger pump will handle a surprising quantity of sand; in fact, it may be said that it is capable of pumping sand containing oil rather than oil containing sand. It is not at all unusual for one of these pumps to handle a mixture containing 50 per cent. of sand by volume. A careful record should be kept of the performance of the pump on each well: Depth at which it is pumping, date when new valve seats or balls were substituted for the old, when the barrel was renewed and on what date the tubing was lowered. The lasting qualities of a pump are directly dependent upon the quantity of sand produced and as this varies with each well the importance of keeping a record of pump performance is apparent. A barrel may require renewal every 6 weeks on one well while on another it shows but slight wear after 3 months' use.

The common practice of allowing a well to pump until the barrel becomes so worn that the production dwindles to a small stream or until some accident or breakage necessitates pulling should be condemned. Each pumping well should be studied and every effort made to maintain the production not only of oil but of sand.

The well should be tubed to such a depth that the maximum quantity of sand which the pump is capable of handling reaches the barrel at all times. This point can be determined only by experiment and gradual lowering of the tubing as sand production diminishes. Continued pumping of sand may save expense and labor of cleaning out "dead" sand with tools and bailer.

FIGS. 4 TO 7.—PLUNGER PUMP AND PARTS FOR RAISING SAND AND OIL.
FIG. 4.—PLUNGER AND VALVES IN WORKING BARREL.
FIG. 5.—PLUNGER WITH UPPER AND LOWER VALVES.
FIG. 6.—WORKING BARREL WITH LINERS AND EXTENSION NIPPLE TO STANDING VALVE BARREL.
FIG. 7.—PARKER PUMP.

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In order to determine the exact condition of the barrel and plunger it is essential that both be removed from the well, washed with distillate and then tested for fit and the absence of lateral play between the plunger and the barrel.

The common practice of removing only the rods and plunger for examination is to be condemned.

The pumping of sand is troublesome and means much work around the wells. For this reason there is a tendency to avoid it.

A long stroke is advisable for pumping wells producing sand and the wristpin should be carried in the "second hole."

If a valve becomes clogged with small gravel or sand it may often be freed by "shaking up the well" or running the pump at high speed for a few strokes. Twenty-five strokes to the minute is the usual pumping rate when the rods drop freely and prevents the sand from settling.

Hot Oiling

Methods of Overcoming a Decline in Production

After a well has produced sand for a time varying from months to years it gradually diminishes in output even though the well is tubed to bottom. In such cases several methods are used to "liven up" the well and increase production. Steam has been used in such wells and "swabbing" is commonly practiced, but the best method is probably the introduction of hot crude oil. Where this method fails it is undoubtedly caused by faulty casing, either screen or perforated, or the hot oil is improperly applied.

The best practice is to heat about 100 bbl. of crude almost to the boiling point and allow it to flow by gravity or pump it down the well casing. It is usually treated in a nearby tank with steam coils and although some operators introduce it through the tubing this practice has the disadvantage that the necessary stoppage of the pump may cause it to sand up.

The effect of the hot oil is to wash the screen or perforations and to lower the viscosity of the underground supply, causing both the oil and sand to flow more readily toward the well.

The hot oil need not remain in the well for more than ½ to ¾ hr. and is then pumped out together with large quantities of sand and "live" oil, oftentimes accompanied by considerable gas. It frequently happens that so much sand enters the well following this treatment that the pump is unable to handle it, in which case two or three joints of tubing are removed, the barrel afterward being gradually lowered as the sand becomes exhausted. After a few weeks the pump again reaches bottom, when another hot oiling is given.

There is no fixed rule as to the frequency of this treatment. This

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can be determined only by experiment and is largely dependent upon the age of the wells. In some cases it may be applied once a week with beneficial results while in others once a month is sufficient.

IV. QUANTITY OF SAND PRODUCED

This is a most interesting subject but, unfortunately, exact figures on the actual quantity of sand produced by flowing or pumping wells are unavailable.

In speaking of the sand produced by wells in Russia, Mr. Thompson[5] says: "The oil from fountains is commonly accompanied by an equal bulk of sand, large numbers of stones, and the liberation of millions of cubic feet of gas which becomes disengaged from the oil on its exit from the tube. A recent Bibi-Eibat spouter on plat No. 29 gave as much as 10,000 tons of oil and 10,000 tons of sand in a day, and in a few weeks yielded, in addition to several million poods of oil, no less than 1,700,000 cu. ft. (85,000 tons) of sand; an amount which will be better appreciated when it is realized that this quantity of material would raise the natural level of the ground on 1 dessiatine nearly 14 ft., or cover an acre of land to a depth of 40 ft."

The famous gushers of California and other loose-sand fields have expelled tremendous quantities of sand which in some cases have completely buried the engine house, belt house, and the lower panels of the derrick.

"In the old Sunset field wells that have a strong gas pressure produce more sand than any others in the State, it being estimated that sometimes as much as two-thirds of the gross yield of the wells is sand. One well alone produced over 110,000 cu. ft. of sand in about 4 years, and another has yielded almost as much in 2 years. The yield of sand gradually decreases with the age of the well, but at no time entirely ceases. These peculiar conditions—soft sand in great quantities accompanying the flow, heavy oil, and strong gas pressure—make the problem of well operation in this field difficult."[6]

Some pumping wells in the north Midway field produce sand at the rate of over 200,000 cu. ft. a year and careful measurements would probably show that this amount is actually produced in a year or even exceeded.

V. OIL SEPARATION AND SAND DISPOSAL

The separation of oil and sand, although more fully accomplished above ground, is one of the difficult problems connected with production from unconsolidated sands. With low-gravity, viscous oils and great

[5] A. B. Thompson: *The Oil Fields of Russia*, pp. 52–53. London, Crosby Lockwood and Son (1908).

[6] R. Arnold and V. R. Garfias: Methods of Oil Recovery in California, *U. S. Bureau of Mines, Technical Paper* 70, p. 22 (1914).

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volumes of sand, nothing has been found to take the place of the open-air "sump," which is simply an excavation made near the well with teams and scrapers. The sand collects in great piles in these sumps while the oil separates by gravity and is pumped from the lowest point in the depression. With heavy oil the loss through evaporation and percolation is slight, as it has been determined that it does not penetrate the ground for more than a few inches. Light oils, however, should not be allowed to flow into sumps, as the loss is undoubtedly great; but when accompanied by much sand other disposition is difficult. As the piles of sand are gradually freed of their oil content they become fairly dry and a crust is formed on their surface which will bear the weight of a man.

Many tanks, settling boxes and other devices have been designed for the separation of oil and sand but with little success. Most of these devices are entirely lacking in scientific principle and are carelessly constructed affairs designed by the workers in the fields and their failure is in large means due to these facts. An efficient means of separating light oils and sand can undoubtedly be devised and would prove of great benefit to the producer. Heavy oils being much more difficult to separate, of less market value, and of high viscosity, may be most economically handled in properly constructed sumps.

The great disadvantage of such excavations is the space they occupy, their unsightly appearance, and the huge deposits of sand which gradually surround the well. The removal of this sand finally becomes imperative when all available space for new sumps has been utilized, but its disposal is a problem.

The only use to which it could be put in the north Midway was road surfacing and although this is possibly a very good use it is a more or less expensive one. The method used is simple.

When a sump had been drained of its oil and the sand had become sufficiently dry, it was loaded upon wagons and distributed on the many dirt roads traversing the property. The roads were sometimes dragged to prepare them for the sand but this was frequently dispensed with. A layer of sand 2 in. thick was sufficient to surface the ordinary road, which was immediately opened for traffic in order that the sand might become packed. The roads in that section of the country were in very poor condition, being formed from use and never constructed or built. They were deep with dust and ruts and the beneficial results attained through the use of oil sand were truly remarkable. This sand as it comes from the sumps contains sufficient oil to stain the hands and serves as a binding material for the road surface. It soon packs into a durable layer of asphaltum-like hardness. It withstands the traffic of heavy automobile trucks and is easily maintained by occasional dressings of fresh sand in the summer when the heat softens the mixture.

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Discussion

Arthur Knapp, Ardmore, Pa. (communication to the Secretary*).—On p. 821 I find a quotation from Mr. Thompson regarding the fountains of the Baku field. I have seen a large number of these gushers or fountains. In every case where unusual amounts of sand were produced the wells caved in and shut off in a few days and were an absolute loss Very often the formation had caved so far up as to allow the water to enter the oil strata, and not only was the well lost but the ground for many acres around the well was useless for drilling purposes because the disturbed ground had allowed the gas and oil to dissipate and the water to enter.

On p. 814, Mr. Thompson is again quoted regarding the Balakhany field. If the text is consulted further, it will be found that the oil at Balakhany is heavy only in contradistinction to the Bibi Eibat field. The gravity of the oil at Balakhany is from 28 to 32. Also, it must be noted that the illustration is taken from the conditions in the older part of the field. This field was first drilled 40 years ago, and was a worn-out field 15 years ago. The wells which produce sand are wells in which the casings are practically gone and the formations are full of water. The sand must be bailed from these wells because the clays from the upper stratum have caved into the wells and are obstructing the passage of oil.

On p. 808 is the statement that the Russian wells cannot be pumped on account of the large amounts of free sand produced. They have never tried pumping in the Baku field to any extent. Wherever tried, it has proven a great success.

If we take the author's theories as correct and wish to put them into practice, it would first be necessary to consider a few figures.

Let us assume that we produce oil and sand together in the proportion in which they exist in the productive strata.

It is not uncommon to have wells in the California field which are not gushers, but which, by pumping, produce 1,000 bbl. per day the first 100 days.

Taking the porosity as one-third, there will be drained during the 100 days, 1,680,000 cu. ft. of sand. If the sand were removed we would have to dispose of:

62,400 cu. yd. = 168,000,000 lb. = 84,000 tons = 1,000 car loads.

To haul this sand away in wagons would require 312 wagon loads per 24 hr.

Further, assuming that the oil was dead and not assisted by any gas, it would require five or six times more power to raise the sand and oil together than the oil alone.

The author, after expounding the theory that the best way to extract

* Received Jan. 15, 1917.

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oil efficiently is by producing a large quantity of sand with the oil and separating the oil at the surface, then says that the best way to extract the sand is by pumping it along with the oil. Of the total volume produced by pumping, not more than 4 or 5 per cent. can possibly be sand. The oil must have a velocity great enough to carry the sand along with it through the pump and up the tubing. Each particle must be separate or they will soon gather together in such quantities as to overcome the velocity of the oil and will settle and stop the pump. Wells producing 500 to 600 bbl. of oil daily and only a few cubic feet of sand will "sand up" the pump unless great care is taken.

I. N. KNAPP, Ardmore, Pa. (communication to the Secretary*).—The author of this paper makes a most remarkable statement concerning a basic principle of oil recovery, as follows:

"Other things being equal, the maximum recovery of oil from an unconsolidated sand is directly dependent upon the maximum recovery of the sand itself."

Unfortunately, the author fails to say what the other things are that must be equal, and gives no intimation of what percentage of the total oil may be considered a maximum recovery when the work is done according to his basic principles. He proposes to extract unconsolidated oil sands from depths of 3,000 to 4,000 ft. and rely for the effectiveness of such operation on the uncontrolled caving of the unconsolidated oil sands at these great depths.

This proposition is contrary to the fundamental precepts of good mining, and can easily be shown to be impracticable.

The paper mentions oil sands of 280 to 300 ft. in thickness; 100 per cent. extraction of these sands would cover the property about 280 to 300 ft. deep. It is self-evident that an extraction of 30 per cent. would present a costly problem in caring for the sand.

The author says, regarding the disposition of sand brought to the surface: "It was loaded upon wagons and distributed on the many dirt roads traversing the property." This indicates an extraction of only a fraction of 1 per cent. of the total sand. Also, after the sand and oil is brought to the surface for maximum oil recovery the author says, "This sand as it comes from the sumps contains sufficient oil to stain the hands and serves as a binding material for the road surface. It soon packs into a durable layer of asphaltum-like hardness." It is, therefore, evident that a maximum recovery of oil from that portion of sand actually brought to the surface is far from 100 per cent.

The problems connected with the recovery of petroleum from any kind of sand cannot, in my opinion, be properly discussed without taking into consideration the porosity of oil-bearing sands, and this the paper under discussion fails to do.

* Received Jan. 30, 1917.

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The porosity of sands, sandstones, etc., has been very elaborately investigated by Professors Slichter and King, and the results are given in the form of professional papers appearing in the 19*th Annual Report of the U. S. Geological Survey*, Part II, 1899. Also *Water Supply and Irrigation Papers* Nos. 67 and 140.

The maximum pore space or reservoir capacity of a theoretical sand was found to be 47.67 per cent. and the minimum 25.95 per cent. with a mean of about 36 per cent.

Experiment proved that the sands that occur in nature come well within these limits, that fine sands contain more pore space than coarse ones; also, that the fine sands yield their liquid contents at a slower rate than the coarser ones.

From the data given, it is fair to assume that the porosity or reservoir capacity of the so-called unconsolidated oil sands is one-third. Therefore, every foot in depth of saturated oil sand of the kind mentioned would yield theoretically 4 in. in depth of oil. This figure is about 2,500 bbl. per acre. If a 10-in. hole a foot deep is drilled in such a sand, 125 sq. in. of pore opening would be uncovered to feed 78½ sq. in. of well opening, and the walls of 10 ft. of well would open a pore space 16 times the area of the 10-in. well.

These figures show conclusively that the extraction of oil alone will soon form in the aggregate a large space, and that it is not necessary to remove any sand with the oil to get a space of low density around the well casing and screen, to which oil may flow. Space produced in this way will in no wise affect the stability of the sand or well, and is in accord with good mining practice. Also, the aggregate of the vast number of small pores feeding through the sand to a well are of sufficient capacity to supply gas and oil for the flow of the greatest gushers and this without any movement in the sand itself if the well is properly screened. For setting well screens and cementing in well casing using the rotary method of drilling, I would refer to my papers[1] treating of these matters in detail.

In a field where the depths of formation are known, a well can be drilled by any suitable method to near the sand. It is then entirely feasible to drill in with the rotary using the mud control and have a full-sized well to the very bottom.

A screen of the full size of the hole may then be set to cover the unconsolidated oil sand, then the oil string of casing may be set above the screen and cemented in. By attaching suitable appliances, the well is ready to be bailed and brought in under absolute control as to the outflow of oil and gas.

If a proper mesh of screen has been employed, the extraction of the oil when properly controlled cannot induce caving. The proper screening

[1] Cementing Oil and Gas Wells, *Trans.*, vol. 48, p. 651 (1914). The Use of Mud-Laden Water in Drilling Wells, *Trans.*, vol. 51, p. 571 (1915).

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of a well reduces pump troubles to a minimum, always assuming that the well may become a pumper.

This entire prevention of caving is in accordance with the fundamentals of good mining practice.

Indiscriminate caving around the well casing must endanger if not ultimately destroy the well, and this destruction may happen before any but a fractional percentage of the sand and oil is produced and it must inevitably happen long before any but a small percentage of the total sand is extracted, and not only destroy the well itself but put a considerable area surrounding out of commission so far as profitable drilling is concerned.

Regarding the caving of sand, the author says: "This movement is frequently of such volume, or occurs so suddenly, that the casing penetrating the reservoir is damaged or completely severed," and again "No type of casing is proof against this force of moving sand."

The necessity of following the fundamental precepts of good mining practice in extracting oil from oil sands below the surface is evident from what the author himself has said, which evidence discredits the statement of the basic principle and law set forth in the paper.

That a cavity or opening of any considerable size can be formed in unconsolidated material by extracting sand with the oil from a well at depth of 3,000 to 4,000 ft., as assumed by the author, is, in my opinion, open to question. The weight of the superincumbent strata must be sufficient to cause much of such material to flow toward any point where the equilibrium has been destroyed by the removal of solids and thus tend to prevent the formation of a cavity. It is well known that many soft clays, slates and shales do flow and soon close up any opening made in them at comparatively shallow depths. It is in some cases impossible to hold even a small tunnel open with any kind of timbering without continual cutting back of the flowing ground until two, three or more times the volume of the tunnel is removed, after which the flow will gradually cease. I have worked in ground of this character and seen it squeeze and flow.

I once had the experience of drilling a well with the rotary to about 1,800 ft. in depth in unconsolidated formations and into a horizon where gas was known to exist in great volume. After setting 40 ft. of fine ($\frac{1}{100}$ in.) wire-wound screen and completing the well, and allowing time for the cement to set, the mud was bailed down, the well cleaned itself and came in like a good gasser, giving dry clean gas with a little dust of fine sand. After blowing a short time, say 20 to 30 min., thin bits of clay came out, that were evidently squeezed through the screen. The screen gradually closed up until the flow of gas was nearly gone. The inside of the screen pipe was cleared of clay and a 20-lb. shot of 40 per cent. dynamite exploded in it. The explosion was followed by a flow of

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gas, sand shale and clay. Possibly as much as 5 cu. yd. of solid material came out in 10 or 15 min. The well cleared itself and measured around 2,000,000 cu. ft. open flow of clean gas, and showed when closed over 600 lb. gas pressure. The gas from this well was used to start another hole, but it soon played out and clay was found to have squeezed up the casing for about 40 ft. above the break.

This column of clay in the casing could be reduced to within about 10 ft. of the break, when the clay would begin to squeeze in faster than it could be extracted. The well was then filled with mud and cleaned out past the shot and, on again bailing, it partly cleaned itself and then went to salt water and shut itself off. This shows that it is practically impossible to form cavities in unconsolidated materials at great depths in the manner assumed in the paper.

Unconsolidated sands are known to occupy about 10 per cent. less space than in the original bed when they are broken down into loose sand. This can mean but one thing, that the sand grains have moved closer together and consequently the reservoir and seepage capacity is lessened. From this it is fair to assume that an unconsolidated oil sand held in place in the well by proper screening can be as well or better drained of its liquid contents than caved or loose sands left in the well, or even when brought to the surface.

THE CHAIRMAN (M. L. REQUA, San Francisco, Cal.).—We have had in California a great deal of trouble from the breaking off and collapsing of well casings from shifting sand, and it is quite true with us as indicated in the paper, that no casing has been found sufficiently strong to withstand that.

I know of wells in which casing that has not been in the hole over 2 or 3 days has collapsed and when that casing has been pulled out it has been just as flat as your hand. I am also familiar with swelling bed rock in the gold drift mining ground of California. It does swell and keeps on swelling and never seems to stop. Just as long as tunnels are kept open in that kind of material there has to be a gang going through the tunnel constantly cutting out and easing up. They have put in timbers 24 in. in diameter in an effort to hold that kind of ground and it has always filled. The only way to succeed is to keep a gang of men busy all the time cutting out and easing down as it squeezes in.

C. NARAMORE, Washington, D. C.—My experience has been very similar to yours in the same field in the matter of breaking off of casing. I had charge of one well, in which I put in reënforced casing, anchored it in place and got 1 day's production, 150 bbl., out of it. The following morning my lease foreman telephoned that it had broken off. I thought at that time we would have more evidence, so I drove out to the well without ordering a new shoe joint. It had been customary to merely

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start from town with a new shoe joint, pull the pipe, take off the crooked bottom joint, start the casing back and sometimes never touch any iron (lost casing) when redrilling the well. Where the flattened joints went to, is more than I care to say, but in this case I thought we would hit that old joint. Many operators in the Coalinga field have had similar experience in redrilling wells in which the oil string had collapsed. At the Republic No. 1 well, there are four strings of casings side by side through the oil sands. They broke off as fast as we put them in. One of the lost strings is inserted pipe.

CHAIRMAN REQUA.—It is a very curious thing what becomes of those pipes that are broken off. They seem to have been pushed over sidewise. I have seen evidences myself of a very pronounced movement where the pipe has been bent as though there had been a slide or pushing over. Some people have ascribed it in part to gas and some are inclined to think it is the direct result of some movement in the sands themselves.

C. NARAMORE.—Many of these wells, where we have buried so much pipe, have made a great deal of sand. The casings there have been replaced and moved again and again, so that the total quantity of solids removed has something to do with the cavity below.

On another property I had a foreman who used to say "We've had another earthquake," as he expressed it, when two or three wells would go off at the same time. He firmly believed that the oil sands actually shifted over an area covering several well locations. This view is hardly acceptable as evidence for a discussion of this kind, but he was correct to the extent that three or four wells would be broken off during one 24-hr. period.

DORSEY HAGER, Tulsa, Okla.—I have measured roughly some of the sand heaps in California, and found as much as 100,000 to 200,000 cu. ft. of oil sand that had been ejected from the well. That is just as a matter of information as to quantity.

WILLIAM H. KOBBÉ (communication to the Secretary*).—I have read with much interest the discussion of my paper by Arthur Knapp and I. N. Knapp.

These discussions apparently assume that my paper advances a number of ideas that are not intended:

1. That the total sandbody or pay is to be removed.

2. That upon bringing this sand and oil to the surface 100 per cent., or perfect separation, can take place.

3. That this tremendous quantity of sand is then hauled away in wagons and distributed as road material.

The paper under discussion is not a theoretical treatment of the sub-

* Received Apr. 29, 1917.

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ject but is the result of actual experience in oil production under the conditions mentioned. That wells that produce sand produce more oil, and the best operating methods to bring this about are fully treated. It is, of course, impossible to remove all the oil sand underlying a property, but the greater the amount removed the greater becomes the production of oil.

As stated in my paper, the surface disposal of this large volume of sand is a problem and the only use to which it can be put is road surfacing, but I did not intend to convey the idea that all this sand was thus used. Only a very small quantity was required on the company's roads, and two wagons only were employed for this purpose. The remaining sand accumulated in the numerous "washes" and sumps.

Although this sand contained sufficient oil to stain the hands, it was surprisingly dry and could be easily shoveled. As stated in the paper, "the piles of sand are gradually freed of their oil content and a crust forms on their surface which will bear the weight of a man." I consider this a high degree of separation. A small amount of oil necessarily remains in the sand and is held there by capillarity. Otherwise it would be useless as a road binder, and, furthermore, to recover such oil special processes of treating are required.

Arthur Knapp says, "of the total volume produced by pumping, not more than 4 or 5 per cent. can possibly be sand." I cannot agree with this statement as I have managed many properties where the wells were so operated that the amount of sand produced was five times this figure. The pumps used for this purpose were Axelson extension barrels with steel plungers and steel-packed standing valves. Sand cannot be handled with the usual Eastern pumping equipment and experience under local conditions is necessary to operate sandy wells, maintain production and prevent the pumps from "sanding up."

I. N. Knapp takes exception to the statement that "other things being equal, the maximum recovery of oil from an unconsolidated sand is directly dependent upon the maximum recovery of the sand itself" and asks what other things must be equal. The meaning of this statement is simply that the more sand a well produces, the greater is the ultimate amount of oil recovered. In order to compare two or more wells, the "other things that must be equal" are naturally gas pressure, viscosity of the oil, depth and physical aspect of the sand, and the age of the wells—in other words, it would be useless to arrive at conclusions by comparing the production of wells operating under radically different physical conditions. It is maintained that if two wells of about the same age are producing 15 Beaumé oil from the same horizon of unconsolidated sand under like conditions of gas pressure, depth, etc., the one which produces sand has the greater output of oil.

I agree with I. N. Knapp that from a theoretical standpoint the pre-

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vention of sand entry and caving appears to be the correct procedure, but my experience is that wherever this has been followed the wells suffered a marked decline in production. I believe in the "regulated entry" of sand and the use of a screen mesh suited to this purpose. Fine sand has not troubled my pumps as much as small gravel and stones.

I. N. Knapp further says that "if a proper mesh of screen has been employed, the extraction of the oil when properly controlled cannot induce caving. The proper screening of a well reduces pump troubles to a minimum, always assuming that the well may become a pumper." I agree fully with this statement, but hold that under such conditions the well will not produce the maximum amount of oil possible.

The occasional injury to casing from shifting sand is not fatal to the well. I have redrilled many such wells, and it is not unusual for a well to continue producing great quantities of sand and oil after the initial flexure of the casing occurs. It requires a serious injury to so deflect the casing that it becomes impossible to tube it.

I realize that redrilling is expensive; that sand production is very troublesome; that underground caving frequently follows sand removal and that all of these conditions may be prevented, but I maintain that such prevention is invariably at the expense of oil production. I know of a great many wells that have produced large volumes of sand but which continue to pump with no indications of casing damage.

A. B. Thompson, in his recent book on oil-field development, describes the use of screen casing at some length and concludes that although such casing may be utilized to prevent sand entry, in practice this results in decreased oil production. He advocates sand removal.

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Reservoir Gas and Oil in the Vicinity of Cleveland, Ohio

BY FRANK R. VAN HORN, *CLEVELAND, OHIO

(New York Meeting, February, 1917)

IT is customary to ascribe two general modes of occurrence to natural gas, namely, shale gas which, as the name indicates, is found in shale, and reservoir gas, which occurs in sandstone, conglomerate and dolomitic limestone. Both of these types are found in the Cleveland district. The shale gas has been produced on a small scale for a long period of years, probably dating back as far as 1883. Shale gas is a low-pressure gas, small in volume with variable pressure, and is limited to no definite horizon. It is generally independent of oil, may last for a long time, and is not dependent to any great extent on the structure of the rocks. There are many such wells in the district which range from 400 to 1,840 ft. in depth, although most have been completed at about 800 ft. This indicates that the wells pass through the Cleveland and Chagrin shales into the Huron or Portage shale of the Upper Devonian.

During the last 2 years great interest has developed in a deeper-seated reservoir gas.

What is commonly called reservoir gas is generally a high-pressure gas occurring in large volume at a definite horizon. Oil may, or may not, be associated with the gas, and in most regions in which reservoir gas is found, the geological structure is of great importance. This type of gas occurs in all large fields such as those of West Virginia which have been supplying Cleveland until the recent discovery of local gas at great depths in commercial amounts.

HISTORICAL OUTLINE

The oldest well producing reservoir gas in the district was drilled in 1886 in Newburg for what was then known as the Cleveland Rolling Mill Co. This well was 3,000 ft. deep, and was thought to have reached the "Clinton" formation. Two "sands" showing small amounts of oil were recorded in the Niagara limestone at depths of 2,658 and 2,686 ft. This is about the horizon of the so-called Newburg sand which has proved to be of some importance as a source of gas and oil by recent drilling operations. The rock pressure was reported to be 400 to 500 lb. per square inch, but since the volume was only 14,000 to 16,000 cu. ft. daily, the well was not only considered unsuccessful but was thought to prove the absence of high-pressure gas in commercial quantities in the Cleveland district.

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The discovery of gas and oil in Clinton rocks in central Ohio between 1900 and 1907 proved an incentive to the continuation of prospecting northward, and as a result, several thousand acres west of Kamm's Corners and Berea were leased for gas and oil rights between 1905 and 1907 by the East Ohio Gas Co., and the Logan Gas and Fuel Co. The former company did most of the drilling, and was still at work in 1908. It also purchased the site of the Newburg Salt Co. in Mill Creek Valley, and in 1907 and 1908 deepened the old salt wells to the Clinton horizon. In most places the wells were unsuccessful, and most of them were not even capped. One of these wells was fairly successful, however, and furnished the second example of deep-seated reservoir gas in the district. The well was drilled by the East Ohio Gas Co. at North Ridgeville, Lorain County, and was finished in June, 1908. It was sold to A. L. Mills, who furnished the driller's record which has been interpreted as shown in Table 1.

TABLE 1.—*Partial Record of Well Drilled in June, 1908, at North Ridgeville, Ohio*

	Thickness, Feet	Depth, Feet
Drift	18	18
Bedford and Ohio shale	1,032	1,050
Devonian and Cayugan limestone (water in limestone at 1,300 ft.)	563	1,595
Salina	80	1,675
Niagara limestone (show of oil at 2,200 ft.; water in limestone at 2,250 ft.)	825	2,500
Cataract sand "Clinton"	8	2,508
Cataract and Queenston	44	2,552
Richmond	47	2,599

At the time of casing, the rock pressure was 840 lb. per square inch, and the open flow of gas was reported to be 250,000 cu. ft. a day. The supply from this well was used to light and heat several houses. The "show of oil" at 2,200 ft. is undoubtedly the horizon of the so-called Newburg sand of present-day operations.

The next development in deep drilling for gas in this district, as far as the writer knows, was on Oct. 17, 1911, when the Newburg Brick and Clay Co., near Warner and Canal Roads, South Newburg, "brought in" a gas and oil well at a depth of 2,520 ft. It was found in a "sand" between limestones of Niagara age, and this horizon has since been called the Newburg sand. The well was still furnishing both gas and oil in December, 1915.

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Present Developments

The oldest wells of the present boom were finished to the "Clinton" sand at a depth of 2,740 ft., in February, 1912, by the National Carbon Co. and the Winton Motor Carriage Co., near Highland Avenue, Berea Road and the New York Central Railroad. Both wells furnished about 1,000,000 cu. ft. of gas daily, and the pressure was about 1,100 lb. per square inch. There was but little drilling during 1912, or until well along in 1913 when several good wells were drilled in Lakewood. Early in 1914, several other successful wells were drilled in Lakewood and West Park, but no actual excitement or boom started until Jan. 30, 1914. On this date, at

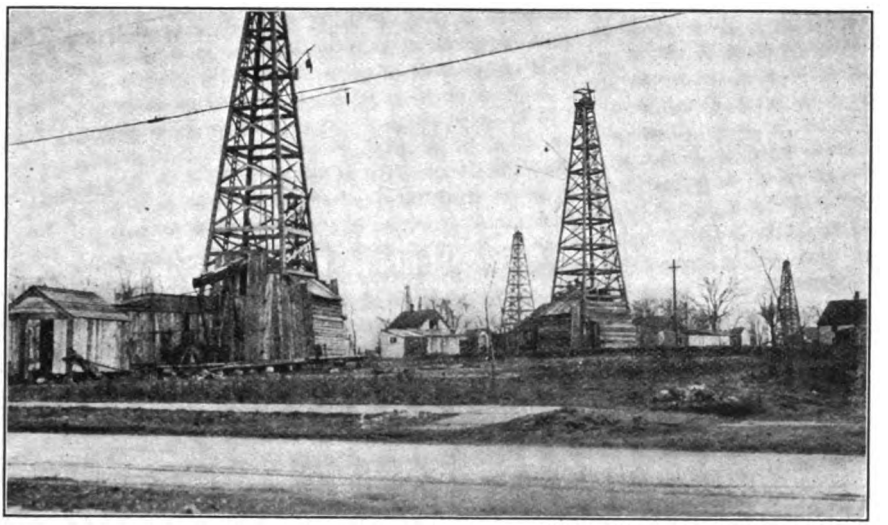

FIG. 1.—VIEW ON LORAIN STREET, CLEVELAND, SHOWING SIX DERRICKS. THIS GIVES SOME IDEA OF THE CONGESTED DRILLING DURING THE GAS EXCITEMENT OF 1914–1915.

the plant of the J. L. and H. Stadler Rendering & Fertilizer Co., South Brooklyn, gas was found at about 2,400 ft. in the Newburg sand. The initial flow of the well was about 12,000,000 cu. ft., and the rock pressure was 950 lb. per square inch. The production dropped to about 3,000,000 cu. ft. within 6 months, and when the service from this well was discontinued in August, 1915, the pressure was only about 100 lb. per square inch. In April, 1914, there were already 55 producing wells, 10 of which were in the Newburg, and the remainder in the Clinton sand. Drillers came from all parts of the country, and many people inside the western limits of the City of Cleveland, as well as in Lakewood and West Park, insisted on having private wells in their own back yards. As a result of this situation, drill holes were placed too near each other, and the production of the older wells rapidly decreased (see Figs. 1 and 2). There did not seem to be very much decrease in the initial pressure and flow of the

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newer wells when compared with the earlier ones, but there was a notable decrease in the life of the newer wells. In June, 1916, over 1,000 wells had been drilled, about 900 of which were drilled between January, 1914, and January, 1916, so that only a little over 100 wells were drilled during the first 6 months of 1916. This indicates a very decided curtailment of drilling operations, which is better understood when the statement is made that nearly 50 per cent. of all wells drilled between January and June, 1916, were unsuccessful. The average cost of a well in the district is about $5,500, so that the total amount invested in drilling alone has

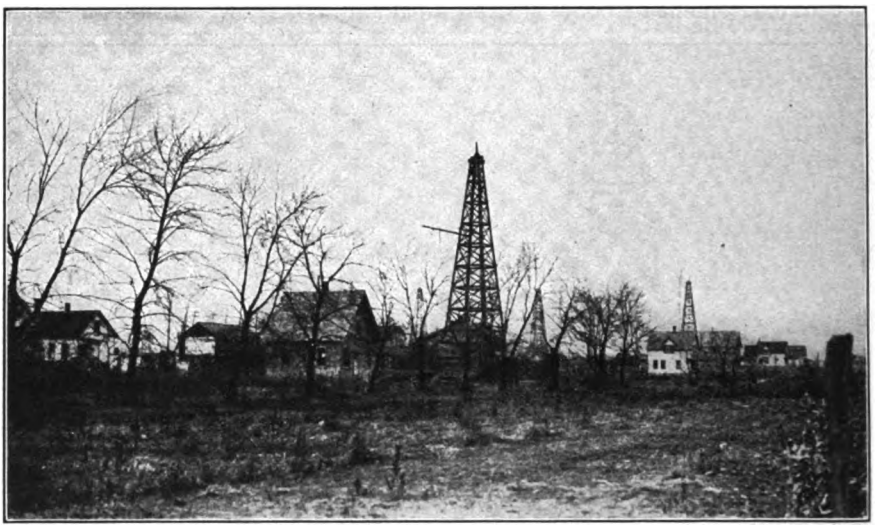

FIG. 2.—VIEW ON LORAIN STREET, CLEVELAND, SHOWING FOUR DERRICKS, CLOSELY SPACED.

been over $5,500,000, to say nothing of the cost of land, leases, or other expenses. It is fairly certain that the value of the gas produced has returned but a small percentage of the original capital invested, much less any interest on it. Only the drillers and a few others have profited by the discovery. Of the total number of wells drilled, about 230 holes were dry or yielded so little that they were immediately abandoned. Of these, 34 were in the Newburg, and the remainder in the Clinton sand. About 150 of the successful wells had been abandoned before May, 1915, at which time many others were producing very little. The average life of the wells of the district is said by an official of the East Ohio Gas Co. to be about 8 months.

The record well of the Cleveland area is said to be that finished by M. F. Bramley and others, on Feb. 22, 1915. It is located on the Aulenbach property, on Harrington Road near Puritas Springs Road. The Clinton sand was reached at a depth of 2,712 ft., and the initial flow was said to be

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nearly 14,000,000 cu. ft. daily. Two other wells produced 13,000,000 each, and the Stadler well had an initial flow of 12,000,000 cu. ft. a day. Six other wells are said to have started with a flow of 10,000,000 cu. ft. each. Over 50 wells are reported to have had an initial flow greater than 3,000,000 to 4,000,000 cu. ft. daily, but on the other hand, many wells have not produced 1,000,000 cu. ft. a day, and the number of dry holes has averaged more than one out of every five producing wells. No accurate figures could be obtained about the total production of the Cleveland field, but it was reported that late in 1914 the district furnished close to 100,000,000 cu. ft. daily, which is about the amount required by the City of Cleveland and its suburbs. The increase in production in Ohio during 1914, over the previous year, was 17,657,974,000 cu. ft. This averages 48,275,271 cu. ft. daily and the increase was due largely, if not wholly, to the production from the Cleveland field. It is very probable that the amount produced by December, 1915, was hardly one-quarter of the maximum production. Practically all the gas is purchased by the East Ohio Gas Co., although some manufacturing plants have supplied their own gas. Since January, 1915, the center of drilling operations has gradually moved from West Park and Kamm's Corners to the southeast, toward Brook Park, Berea, and Middleburg township where each well is protected by considerable acreage. This should result in a longer life for each well than was experienced in the northern part of the district where the overdrilling was so pronounced.

Producing Sands

The chief sands of economic importance have been called the Newburg and Clinton. Some people have applied the name "Stadler Sand" to the Newburg, but the latter term has priority as well as more common usage. There is also one unimportant horizon called the "Stray" sand, and the Trenton limestone has been prospected to a slight extent.

Stray Sand

This sand has been found in at least three places in Lakewood, at depths ranging from 1,355 to 1,400 ft., and its position must, therefore, be in the lower Devonian or upper Silurian limestones. The sand is about 3 ft. thick and at two places produced oil, while at the other locality an initial flow of 4,000,000 cu. ft. of gas was obtained. Water caused more or less trouble, and the wells were abandoned after a time, or were deepened to the Clinton.

Newburg Sand

The horizon of the Newburg sand is found at depths ranging from 2,300 to 2,600 ft. The Newburg is said to attain a thickness of 30 ft. in places, but in other localities it is apparently absent. In the Lakewood

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region, its thickness is rarely greater than 15 ft., but it becomes thicker to the south and east toward the Denison-Harvard district, where the maximum thickness is found. In that vicinity its depth below the surface also increases, owing to the southeast dip. According to reports made by the East Ohio Gas Co., the Newburg varies from 3 to 17 ft. in thickness. At the pioneer well of the Newburg Brick & Clay Co., the sand was penetrated to a depth of 15 ft., reaching a total depth of 2,520 ft. The gas sand from this well was of grayish color, soft, brittle, and showed abundant cleavage planes. In polarized light even the smallest fragments showed high interference colors which characterize minerals of very high double refraction. This sand dissolved almost wholly in cold dilute hydrochloric acid, and the solution showed few, if any, traces of iron. The properties given previously are those of a fairly pure limestone, composed of calcite or dolomite. Another specimen of Newburg sand was of grayish-red color, and contained particles which were grayish, reddish-brown, and grayish-black, so that it might have been a mixture of three rocks. This sand was easily crushed but some of the particles were hard enough to scratch glass. The grayish particles showed high double refraction, and dissolved readily in cold dilute hydrochloric acid. The reddish-brown particles dissolved less readily and the other particles were insoluble. The solution was colored yellow with iron chloride. This sand was calcareous, but certainly originated from a more impure limestone than that obtained from the Newburg Brick & Clay Co.'s well. The Newburg sand is, therefore, a calcareous or dolomitic limestone, more or less pure, and not a quartzose sandstone like the Clinton horizon. It probably consists of a porous limestone similar to the Trenton limestone of the Findlay-Fostoria region. The Newburg sand occurs in what is called the Big Lime, and belongs to the Niagara epoch of the Silurian. The horizon probably corresponds to that of the Lockport dolomite. About 124 wells have been drilled to this sand, and only 34 have failed to find gas. Many of the latter were continued down to the Clinton. Chemically, the gas from the Newburg sand is said to be little, if any, different from the Clinton gas, an analysis of which is given in Table 3. The Newburg has been most successfully prospected in the southern and southwestern parts of Cleveland, known as Newburg and Brooklyn, especially in what is called the Denison-Harvard district, where the sand is thickest. The three largest wells came in with 13,000,000, 12,000,000 and 6,000,000 cu. ft. of gas daily. One of these was the Stadler well, located at Denison Avenue and the Belt Line, which really started the gas boom in the Cleveland territory. Although this well came in with a large volume of gas, it lasted only from Jan. 30, 1914, to about August, 1915. Because of the general interest in this well, its log, as furnished by the drillers, with interpretation, is given in Table 2.

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TABLE 2.—*Log of Stadler Well, Denison Avenue and Belt Line, Cleveland. In Newburg Sand. Completed Jan.* 30, 1914. *Volume* 12,000,000 *Cu. Ft. Rock Pressure* 950 *Lb. per Square Inch*

	Feet		Thickness, Feet	Depth, Feet
Drive pipe	70	Drift	70	70
Shale	70–1,106	Devonian shale	1,036	1,106
Top of lime	1,106			
Lime very hard	1,360			
Water (filled to top of hole)	1,395	Devonian and Cayugan limestone	689	1,725
Gas	1,415			
Through lime	1,555			
Broken lime	1,555–1,725			
Salt	1,725–1,915			
Lime	1,915–2,010			
Salt	2,010–2,025	Salina	390	2,115
Slate	2,025–2,070			
Lime shell	2,070–2,080			
Slate	2,080–2,115			
Lime	2,115–2,250			
Lime very hard	2,250–2,260	Niagara above Newburg	240	2,355
Lime, broken	2,260–2,320			
Lime, broken	2,320–2,355			
Gas	2,365			
Total depth	2,375	Newburg sand	20	2,375

Clinton Sand

Although through long usage the sand at this horizon has been known as the Clinton sand, it is very certain that it does not belong to the Clinton, but to the Cataract formation of the upper Medina, and, like the Newburg, is also a Silurian horizon. On account of the common use of the term Clinton, it will probably be best retained. The sand is found at depths ranging from 2,700 to 2,900 ft. In the northern part of the Cleveland district it is reached quite constantly at about 2,750, but toward the south and east the depth increases, owing to the dip of the rocks in those directions. According to reports, the thickness ranges from 8 to 50 ft. The East Ohio Gas Co. gives results varying from 5 to 35 ft. At the plant of the Cuyahoga Brick & Shale Co. in Parma township, between Ridge and Pearl Roads, the top and bottom of the Clinton were reported at 2,823 and 2,867 ft. respectively, giving a thickness of 44 ft. The Twin City Oil and Gas Co. at West 100th Street near Bertha Avenue, drilled from 2,737 to 2,765 in the Clinton, a distance of 28 ft., but reported that "The Clinton here is 50 ft. deep, which is about double the normal thickness." The thickness most commonly given is about 20 ft. J. C. Gillette, of the National Carbon Co., says that one of the reasons for the

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varying reports is that some drillers include in the Clinton certain rocks which do not belong there. In his experience he finds, on approaching the Clinton, first a layer of reddish, clayey sand, ranging from 0 to 12 ft. in thickness, which contains some gas. This is followed by 3 ft. of shale, and then comes about 20 ft. of gray, faint pink, or whitish sand, which is the chief gas reservoir. If these three strata were all included as Clinton, it would give a thickness approaching 35 ft.

Two specimens of pink, and gray or white Clinton sands have been investigated. They consist of quartz sand stained more or less with iron oxide, which even in the white sand dissolves in cold dilute hydrochloric acid, and stains the solution yellow with ferric chloride. Neither sample showed any traces of lime, and the white sand originated from a grayish, porous, friable, quartzose sandstone, particles of which, hard enough to scratch glass, could be seen in the sand. Two other specimens of grayish color were also examined, but these contained limestone fragments along with the sandstone and, consequently, effervesced easily with acid. The limestone particles undoubtedly came from horizons above. Although the Clinton sandstone is generally porous, it is sometimes reported as very dense and hard, so as to require shooting in order to open up the flow of gas. Two analyses were furnished by the East Ohio Gas Co. Under 1, is given an analysis of Clinton gas from Cleveland, while under 2, is another of West Virginia gas, used in Cleveland, for comparison.

TABLE 3.—*Analyses of Natural Gas Furnished by the East Ohio Gas Co.*

	1 Cleveland	2 West Virginia
Carbon dioxide	0.0	0.20
Illuminant (heavy hydrocarbons)	0.2	0.30
Oxygen	0.1	0.15
Carbon monoxide	0.6	0.50
Methane	95.5	93.60
Hydrogen	1.6	1.65
Nitrogen	2.0	3.60
Total	100.0	100.00
Average B.t.u.'s	1,105	1,095

Approximately 925 wells have been drilled to the Clinton sand, 200 of which have been dry or so small that their production was not utilized. At least eight wells have yielded an initial flow of 10,000,000 or more cu. ft. each, and two of these started, according to reports, with about 14,000,000 and 13,000,000 cu. ft. daily. Like most wells in the Newburg sand, many Clinton wells have declined rapidly in production. It is a matter of interest that the original well of the National Carbon Co. which "came in" on Feb. 14, 1912, with about a million cubic feet at a pressure

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of 1,100 lb., was still producing 30,000 ft. in December, 1915, at a pressure of 100 lb. This well kept a pressure of 200 lb. for about 2 years. The life of this well has been, therefore, much longer than the average in the Cleveland field. The record of a well in the Clinton sand, as given by the drillers, with interpretations of the data, is given in Table 4.

TABLE 4.—*Log of Winton Gas Engine Co. Well, West 106th Street, between Madison and Western Avenue, Lakewood District. Clinton Sand*

	Feet		Thickness, Feet	Depth, Feet
8¼-in. casing	60	Drift	60	60
Top of lime	1,140	Devonian shale	1,080	1,140
Water	1,450	Devonian and Cayugan limestone	510	1,650
6⅝-in. casing	1,650	Salina	200	1,850
Salt	1,750–1,850			
Lime	1,850–2,590			
Slate	2,590–2,660	Niagara	830	2,680
Top of little lime	2,660			
Through little lime	2,660–2,680			
Slate	2,680–2,704			
Top of Clinton sand	2,704			
Gas	2,704–2,706	Cataract and Queenston	69	2,749
Through sand	2,706–2,713			
Red sand	2,713–2,721			
Slate	2,721–2,749			

Trenton Limestone

Because of the importance of the Trenton limestone of the Ordovician period, in the western part of Ohio, as a source of gas and oil, two wells have been drilled to that formation. They were both put down on the East Side of Cleveland, and both attempts were unsuccessful. One was drilled by the Park Drop Forge Co. at East 79th Street and the New York Central Railroad, and the other by the Cleveland Twist Drill Co. at Lakeside Avenue and East 49th Street. In the latter well a small flow of gas was found in the Trenton, and the highest rock pressure observed was only 37 lb. per square inch. An analysis of this Trenton gas, furnished by J. V. Emmons, follows:

Carbon dioxide, 1.4; heavy hydrocarbons (illuminant), 0.8; oxygen, 0.4; carbon monoxide, 0.6; methane, 95.7; hydrogen, 0.0; nitrogen, 1.1.

A condensed record from this well, in which the top of the Trenton was reached at 4,445 ft., is given in Table 5.

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TABLE 5.—*Partial Well Record, Cleveland Twist Drill Co., Lakeside Avenue and East 49th Street, Cleveland*

	Thickness, Feet	Depth, Feet
Drift	200	200
Devonian shale	760	960
Devonian and Silurian limestone "Big Lime"	1,722	2,682
Medinian limestone and shale	102	2,784
"Clinton" sand (no gas)	36	2,820
Queenston shale and sandstone	360	3,180
Cincinnatian shale and limestone	1,265	4,445
Trenton limestone	132+	4,577

Pressures

The largest authentic rock pressure reported in the Cleveland field was a well of the National Carbon Co. which registered 1,120 lb. per square inch. Another of 1,250 lb. was reported, but it was probably not authentic. The usual initial pressure for most wells in the Lakewood region was about 1,100 lb., but in August, 1915, with few exceptions, it had dropped to 125 lb. In the West Park district the initial pressure of about 1,000 lb. had decreased to about 100 lb. in August, 1915. The Stadler well decreased from 950 to 100 lb. during its period of service of about a year and a half.

OIL

Although there are vast amounts of petroleum in the Devonian Ohio shales, it is disseminated through them in such small quantities that it is not obtainable economically. At many places in the district are so-called oil springs. The Euclid bluestone at some localities contains small amounts of oil which come to the surface along cracks and bedding planes. The oil, however, probably originates in the shales immediately underneath. The Mills gas well showed oil at 2,200 ft., at a position in the Niagara Limestone of the Big Lime. This is probably the horizon of the Newburg sand. Several of the wells drilled and abandoned by the East Ohio Gas Co. in 1907 and 1908 were left uncapped, and oil either flows out at the top, or can be obtained at a depth of a few feet where it probably floats on water. During the period of the gas boom, during 1914 and 1915, about 30 wells are reported to have found showings of oil. Of these, two were found in the "Stray" sand at a depth of about 1,400 ft.; four occurrences were in the Newburg, and the remainder in the Clinton. Several wells were reported to flow 30 to 40 bbl. daily, either before or after shooting, but the majority came in at 10 to 20 bbl., and practically all were short-lived. Most of the wells of the Cleveland field, after producing a few barrels for a month or so, have ceased pumping. It is,

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therefore, evident that any who had hopes of an oil boom in the Cleveland area were disappointed. Probably the best oil well is the one which was the real pioneer of the district, that of the Newburg Brick & Clay Co. This gas and oil well came in on Oct. 17, 1911, with a natural flow of 35 to 40 bbl. After shooting, it produced 100 bbl. daily for a time, and in December, 1915, it was still making 2 to 3 bbl. a day. The oil was struck at a depth of 2,520 ft. in the Newburg sand. It is sold to the Standard Oil Co., and is said to contain little gasoline.

Table 6 gives three distillation tests made by the National Refining Co., of crude oil from the Cleveland district, all of which probably came from the Clinton sand.

TABLE 6.—*Three Distillation Tests of Crude Oil from Wells in the Cleveland District*

	Percentage	Gravity Beaumé	Flash point
			Degrees Foster
Lakewood Crude*			
Gasoline fraction	17.185	From 71.4 to 56.8	
Kerosene fraction	34.375	From 55.1 to 41.0	120
Gas oil fraction	12.500	From 40.6 to 36.7	260
Wax oil fraction	15.625	From 36.1 to 30.9	330
Residuum	20.315		
	100.000		
West Park Crude†			
Gasoline fraction	18.18	From 80.5 to 57.2	
Kerosene fraction	31.815	From 53.7 to 40.9	130
Gas oil fraction	13.635	From 38.6 to 35.7	345
Wax oil fraction	13.635	From 38.6 to 30.4	365
Residuum	22.735		
	100.000		
Cleveland Crude‡			
Gasoline fraction	20.310	From 79.4 to 55.8	
Kerosene fraction	31.250	From 54.6 to 41.2	122
Gas oil fraction	12.500	From 40.3 to 37.7	280
Wax oil fraction	15.625	From 35.8 to 31.8	355
Residuum	20.315		
	100.000		

* 16,000 c.c. of crude oil with a gravity of 41.2 Bé., from Lakewood, Ohio. This crude contains a large percentage of high-grade water-white oil which can be refined for market without the use of lead. The percentage of sulphur is very low.

† 11,000 c.c. from a 3-gal. sample of crude oil having a gravity of 41.1 Bé., from West Park, Ohio.

‡ 16,000 c.c. crude oil with a gravity of 42.2 Bé., from Cleveland, Ohio.

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Relation of Gas and Oil to Geological Structure

With regard to reservoir gas, too few accurate data have been obtained to warrant any very definite conclusion about structural relations. In general, the southeast dip of the gas-bearing rocks produces a monoclinal structure, along which the gas tends to rise to the highest levels. Compared with central Ohio, these higher levels would be located in northern Ohio, and in the Cleveland district. In a discussion of the structure of this region, H. P. Cushing has pointed out that certain warpings have materially increased the southwest dip of the Berea sandstone across the Berea quadrangle, and that this should cause a corresponding flattening in the dip of the Silurian rocks which contain the gas. The occurrence of such flats between pitching areas would probably tend to cause the accumulation of gas or oil, because at such places they would have little tendency to move farther. The chief gas-producing territory, as outlined by present development, lies in the Berea quadrangle, and the most successful drilling is being done to the southeast of Cleveland across that region, which, according to Cushing, should be characterized by gentler dips. Recently in the vicinity of Wooster, about 60 miles southeast of Cleveland, anticlinal structures have been proven to exist to a small extent in the Clinton sand, but in the Cleveland field the few measurements that have been made have not shown the presence of such structures.

Discussion

David T. Day, Washington, D. C.—The statement was made that shale gas was drilled for in 1883. Wells were drilled and gas obtained from each sufficient to supply three families, in 1865. I worked on one myself in 1868. The gas smelled of gasoline at that time

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The Evidence of the Oklahoma Oil Fields on the Anticlinal Theory

BY DORSEY HAGER,* TULSA, OKLA.

(New York Meeting, February, 1917)

THE information given in the accompanying table is submitted as evidence confirming the application of the anticlinal theory and the value of geology in the Kansas and Oklahoma oil fields.

The term anticline has been used to define a particular type of arched fold. The anticlinal theory of oil accumulation should, however, be broadened to include any structural fold of an arched type even if not a distinct anticline. A terrace may very properly come under the heading of the anticlinal theory of accumulation, as the folding is closely related to that of the anticline; in fact, in many cases is an arrested anticline, though not completely closed in all directions. Stratigraphic forms due to lensing or irregular cementation of the sands certainly cannot be considered to fall within the anticlinal theory, though the laws of accumulation may be similar. A dome is similar to an anticline and belongs to the same class. A dome is broader in proportion to its length than an anticline. Many anticlines undulate along their axes to form domes. The anticlinal theory in this discussion will then be applied to anticlines, domes and terraces.

In the following list, the writer has tabulated the main oil fields of Oklahoma, and a few recently discovered fields in Kansas. The fields discovered within the past 4 years are listed with the date of their discovery. Under "How found," the writer puts "Wildcat" where a geologist has not been employed, "Geology" where it has been used, and "?" where there is doubt. In some places the geologists have claimed pools that may rightly belong to them, but their claims have been questioned, so that they belong in an unsettled category.

Unless one has mapped all the old pools, it is not possible to say just how far they are dominated by structure, but from our own work, and from discussions with others, it is safe to venture that nearly all the "shallow-pool" districts, Nowata, Bartlesville, and the Eastern Osage, are controlled by structure; domes, noses, and terraces are the main structural features. The "shallow pools" of Kansas show the same relation. At Peru and at Sedan, domes are evident, though gas occurs high on the domes, and oil well down the flanks, leading some to believe

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Productive Field or Pool*	Type of Fold Domes and Anticlines	How Found	Date
Augusta, Kan.	5 minor domes on 2 anticlines	Geology	1915
Bald Hill	Dome	Wildcat	
Beaumont, Kan.	Dome	Geology	1915
Bigheart	Dome	Wildcat	
Billings	Dome	Geology	1916
Blackwell	Dome	Geology	1913
Boston Pool	Dome	Geology	1912
Boynton	Minor domes	?	1913
Cole Pool	Dome or lense	Wildcat	1913
Checotah	Dome on anticline	Geology	1914
Dexter, Kan.	Dome on anticline	Geology	1914
Drumright, Cushing Pool	Dome on anticline	Wildcat	1914
Dropright, Cushing Pool	Dome and terrace	Wildcat	1913
Eaton, Gas	Dome	Geology	1916
Eldorado, Kan.	Domes on anticline	Geology	1915
Foxburg, Cushing Pool	Dome on anticline	Geology	1915
Fox, North of Healdton	Dome on anticline	Geology	1915
Ft. Smith, Gas Pool	Dome on anticline	Geology	
Garber	Dome	Geology	1916
Healdton	Minor domes on anticline	?	1913
Hominy, Oklahoma Dome	Dome	Geology	1916
Ingalls	Dome	Geology	1915
Loco Gas Pool	Dome	Geology	1913
McMan Gas Pool, Haskell	Dome	Geology	1915
Morrison	Dome	Geology	1915
Muskogee	Domes on anticline	Wildcat	
Newkirk	Dome on anticline	Geology	1913
Ponca City	Dome on anticline	Geology	1913
Paola, Kan.	Dome	Wildcat	
Poteau Gas	Dome	?	
Quinton Gas	Dome on anticline	Geology	1915
Robinson Pool	Faulted dome	Geology	1915
Shamrock (Cushing)	Dome on anticline	Geology	1914
Stone Bluff	Dome	Geology	1915
Town of Cushing	Dome and terrace	Geology	1914
Vera	Dome	?	1915
Virgil, Kan.	Anticline	Geology	1916
Wagoner	Dome	Wildcat	
Wainwright	Dome on anticline	Geology	1914
Wheeler	Dome on anticline	Geology	
Winfield, Kan.	Dome	Geology	1916
Yale	2 domes on anticlines	Geology	1914

* Pools in Oklahoma unless otherwise mentioned.

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Productive Field or Pools (Continued)	Type of Fold Terraces	How Found	Date
Bixby (Franchot Pool)	Terrace	Wildcat	1916
Catoosa	Terrace	Wildcat	1914
Dawson	Terrace	Wildcat	
Tulsa	Terrace	Wildcat	1913
Glenn Pool	Double terraces	Wildcat	
Jenks	Terrace	Wildcat	
Jennings	Terrace	Wildcat	1915
Kellyville	Terrace	Wildcat	1913
Morris	Terrace	Wildcat	
Mounds	Terrace	Wildcat	
New York Pool	Terrace	Wildcat	1913
Owasso	Terrace	Wildcat	
Pumpkin Center	Terrace	Wildcat	
Ripley	Terrace	Geology	1914
Tiger Flat	Terrace	Wildcat	
	Lenses		
Avant	Lense	Wildcat	
Coweta	Lenses	Wildcat	
Misener Pool	Lense	Wildcat	1914
Wigton Pool	Lense	Wildcat	1916

Oklahoma Shallow-Pool District

Hard to Define without Definite Detailed Surveys

Productive Field or Pools (Continued)	Type of Fold Terraces	How Found	Date
Bartlesville	Terraces, small domes	Wildcat	
Bird Creek Pools	Terraces, small domes	Wildcat	
Childers	Terrace, domes	Wildcat	
Copan	Terrace, dome	Wildcat	
Dewey	Terraces, domes	Wildcat	
Flat Rock	Terraces, domes	Wildcat	
Lenepah	Domes, terraces	Wildcat	
Nowata	Domes, terraces	Wildcat	
Ochelata	.....	Wildcat	
Owen	.....	Wildcat	
Wann	Dome and terrace	Wildcat	

that the oil occurs in the synclines. I have yet to hear of a well-defined Oklahoma or Kansas syncline carrying oil in the synclinal trough.

At Eldorado, Kan., oil does occur close to the synclinal trough, but it also occurs on top of the dome. The deformation or height of the Eldorado fold is 110 ft. Oil is found on top of the fold, and 80 ft. down from the top. The syncline, however, carries water. Structure influences the accumulation even here.

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Conclusions

1. Of the 75 pools listed, which comprise the principal pools of Oklahoma and a few in Kansas, all but four are on well-defined structure.

2. It will be seen that by far the larger proportion of the representative fields listed are of the dome or anticlinal type. Terraces come next in order, while lenses form a small minority. Of the list 45 pools, or 60 per cent., are on domes and anticlines; 15 pools, or 20 per cent., are on terraces, and 11 pools, or 14.7 per cent., are undivided, while 4 pools, or 5.3 per cent., are on lenses. If those fields on undivided terraces and domes (14.7 per cent.) are split equally, which is fair enough, then 7.35 per cent. will be added to the dome, and to the terrace lists; so that 67.35 per cent. are on domes and anticlines, 27.35 per cent. are on terraces, and 5.3 per cent. on lenses, which is a fair ratio for the Mid-continent fields.

3. Also, it is interesting to note that of 45 pools found in 1913 and later, 30 pools, or 66⅔ per cent., have been opened by the geologists—a truly remarkable record. Of these the geologists have credit for but one terrace.

4. The evidence shows that nearly all the known fields are on well-developed folds. Lensing of the sands and cementation have both been factors in favoring accumulation, but well-pronounced folding has influenced most of the fields.

The brief for the geologist is complete, and I must rest my case feeling secure that the evidence shows: (1) That the anticlinal theory holds in Oklahoma and in Kansas; and (2) that the geologist is playing a most important part in the development of those oil fields.

So far little or nothing has been said in this discussion regarding the use of geology in developing the fields discovered. In all the recent "wildcat" pools, the geologist has been of material assistance in guiding development after the first well. This was especially true at Healdton, at Cushing, at Boynton, and other recent strikes. The services of the geologists have grown so well recognized and known, that all the large oil companies of the Mid-continent employ them in locating wells and in examining territory.

Discussion

Dorsey Hager, Tulsa, Okla.—I have been asked why the Dexter region is dry. I would like to know myself. I drilled two dry wells on that same anticline which has production to the east and west and south and north. That same anticline has seven domes that have been drilled and only three have proven productive out of the seven. In other words, you have one well-defined anticline with seven domes along its length, all mappable, all checked carefully, and only three of those seven have produced. Now why the others did not produce, I do not know.

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L. L. HUTCHISON, Tulsa, Okla.—There is no appreciable difference in the character or structure of the sands?

DORSEY HAGER.—No, I have never been able to see any appreciable difference in the sands where we got production and where we did not get it. I will say that the deepest wells we drilled, in the vicinity of Dexter, had no sands at the point where they got the production farther north.

I. N. KNAPP, Ardmore, Pa.—I first went to the Kansas-Oklahoma field in 1895 and occasionally thereafter until 1899, when I began drilling for oil. I started close to wells that were producing gas and which, when drilling, had made good oil showings in a sand above the gas. I thought these showings were good enough evidence of an oil pool for me without looking for any anticline and I made a paying development in what I was afterward told by geologists was probably a synclinal trough.

If I remember correctly Dr. Orton's report on the Iola gas pool (about 1898) he distinctly said that he found no evidence of anticlinal structure and concluded from the records of a large number of wells that there was a terraced structure of the productive gas sands, of which there were no surface indications.

As far as I could make out from my well records, the top of the oil sands was practically level in the region where I operated.

The lower sands were gas sands that seemed to me to be like sand dunes on the sea shore and the oil sands seemed like sedimentations forming sand bars in the shales and were not continuous sheets of sand rock. I imagined that conditions shown by the drill could best be explained in this way. I do not think there was any evidence of anticlinal conditions. I drilled in all 225 wells in Kansas by 1906 and came out in 1908.

Both the geologist and the operator undoubtedly know the Kansas-Oklahoma field far better than they did 8 years ago.

DORSEY HAGER.—In recent developments there has been a good deal of revival of activity in eastern Kansas in shallow districts, and anticlines have been mapped; the domes are not large, the reversals are from 10 to 20 ft. per mile, very low reversals. Indeed one might call them practically terraces. In some places the surface is such that if one has large shale bodies, it is practically impossible to get any outcroppings to work on. On the other hand, where one has good limestones and good sandstone formations, one can map them and define the structure without any difficulty.

C. NARAMORE, Washington, D. C.—I think it would be well for Mr. Hager to tell about mapping. I was very much interested a few months ago to hear of the methods used by Mr. Hager's men in the field. I have been told of one company in Kansas and Oklahoma that has 190 geologists in the field at the present time.

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DORSEY HAGER.—Any well-defined structure in Kansas or Oklahoma that has produced oil can be seen with the eye. Before we send a man out to do any detail work, he is sent out to run over the country using his eye and hand level. East of Tulsa one will find big shale beds that show no structure, but when one gets in the limestone and sandstone areas one can see structure with the eyes.

We first make a reconnaissance instead of detailing to find structure. If we discover structure, we then go in and make the detail map and bring out the minor points of the structure. We detail everything using a detailed scale of 1,000 to 2,000 ft. per inch, allowing an error in closure of levels of 5 ft. in 5 miles as a limit; most of the work checks within a foot. We take the tops of limestones or sandstones as a working base; finer work than that is out of the question. The contour intervals used are generally 10-ft. intervals. Some work has been done on 5-ft. intervals but only when one gets very clean-cut exposures can one use 5 ft.

F. J. HIRSCHBERG, Choteau, Mont.—Our structures as compared to the Appalachian and Pacific Coast structures are very gentle. Throughout Oklahoma and Kansas, however, it is in our favor that any little change in the topography that is at all noticeable is likely to be caused by structures. In other words, there is close coincidence between topography and structure. In reconnaissance work in those States, you must watch for the dip slopes. If the topography is not gentle to the westward all over the country, look out. If there is a gentle turn to the eastward, ten to one there is structure there, and if there is structure, ten to one you will get production.

R. H. JOHNSON, Pittsburgh, Pa.—Mr. Hager very clearly makes good his second contention that geology has been of the utmost importance in locating fields in Oklahoma and that structure there is of great importance.

My criticism is with the other contention that "the anticlinal theory holds in Oklahoma." I should rather say that the accumulation of oil there was very largely controlled both by structure and lithology. That is, we should treat the two things as coördinate rather than lay so much emphasis on structure.

The time has come when the producer needs to have his attention called to the importance of subsurface mapping. These maps that we make of the structure of the surface, are of course very valuable, but is not the producer of today depending on them too much? Is he spending enough time on the divergence, thickness and nature of these sands?

Now if our anticlinal theory is to really "hold," then our pools ought to consist of a gas pool, surrounded by a ring of oil wells. If we look over the map of Kansas and Oklahoma, we do not find rings like that except in rare instances. In the Bartlesville region we see that it is quite

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the uncommon thing. Where the oil does have relation to structure, usually lithology has played so important a rôle as to interrupt this theoretical ring.

The next thing that we ought to find, if the anticlinal theory "holds," would be a ring of water wells around every pool. Now if we examine the reason why these marginal dry holes are dry, we find that in about a third of them, the sand is too thin or too shaly or too close. Gas, as is quite apparent from Mr. Hager's list, can be located with anticlinal aid much more easily than oil, not only in the Oklahoma field but in other fields. One possible cause for this is that the gas moving up a homocline is arrested by the domes. If there is enough gas it will preëmpt the entire dome down to the spilling point. The oil will then be forced to spill over into the next dome further up the homocline. If the gas is still sufficiently abundant, the gas will be sent on to the next one. If the gas is still adequate to preëmpt all that dome, it is sent on up the homocline. Where does it finally rest? It will escape at seepages or, as I think is much more frequently the case, it will be caught at the upper end of the reservoir, giving us a pool which is essentially a monoclinal pool.

DORSEY HAGER.—Detailed mapping of the Glenn pool area has shown an east reversal of 20 ft., which makes a very low dome, or practically a broad terrace.

I. N. KNAPP.—I like to look at the diagrams of the geologists and read their books, but somehow or other when one drills one seldom finds conditions exactly as pictured or laid down by them. For instance, all writers on oil and gas matters that I have read give it as an axiom that gas is always found mingled with or just above the oil in the same sand.

Well! I am now drilling around an old Trenton Rock oil pool and there is not a bubble of gas with the oil or anywhere above it. This pool was opened in 1897, produced over 1,000,000 bbl. of oil and was abandoned about 1906. There never was any gas in it. A salt water sand, however, closely underlies the oil and there is some salt water with the oil.

The most popular pictures in books on oil geology seem to be ones that show an anticlinal with the gas on top, the oil in the middle and the salt water on the bottom of the same continuous sand rock strata or pitching sand lense. I do not question but that these conditions occur in some places.

I have produced a good many hundred thousand barrels of oil in a field where no such conditions were found, for the oil was in a sand by itself and not connected in any way with salt water or gas. There was, however, enough gas with the oil to make a froth but not enough to flow the oil even in the original wells. The oil was entirely free from water, and there was no evidence of anticlinal structure.

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R. H. JOHNSON.—With respect to the Glenn pool and the terrace there, it seems to me important for us to consider, Does the limit of the Glenn pool coincide with the spilling plane of the terrace? Now would it not be true, Mr. Hager, that this terrace would control only a small part of the Glenn pool, and if it controls only a part of the Glenn pool, what would place the oil in the other part? Take the Glenn pool as a whole and that terrace only would be capable of accumulating oil at that particular place. Since we get oil in a large additional area, it seems to me the oil would have been there even if this particular structure had not been present.

DORSEY HAGER.—I do not make the claim at all, that lithological character does not play its part. I claim that if one is hunting oil pools in Oklahoma and Kansas, the only thing one has to work on is structure, and a man is forced to use his surface formations and map his surface structure, and then drill. If one is working in a field where there are plenty of well records and plenty of development, then one can determine structure from the logs, and there is no question about lithology. Lensing does play an important part, but if one has a lensed condition and the lense lies in a syncline or on a normal dip, one may or may not have accumulation in that lense, but if that lense is on a terrace, the terrace will give a larger drainage area as there will be dips from at least three directions toward the top of that terrace. I think that is one of the main reasons why terraces do produce.

Now you will find that wells that are off structure are not, at the outside, over 7 per cent. There is no way of defining those pools before a well is drilled. If lithologic or lensed structures were on a normal west dip and a man drilled for them he would have an ordinary wildcat chance of getting a pool, which is about one in 150 in Oklahoma and Kansas. If a man is to do any development in that country, he must be governed by what structure he has.

We find in listing the pools that 75 per cent. of the domes of Oklahoma and Kansas have been productive.

With that percentage of successes on structure, I think a man would be very foolish to go out into a wildcat country and not be governed by a structure. You cannot tell anything about lithology until you have well records to work with.

L. L. HUTCHISON.—May I ask Mr. Johnson to define exactly what he means by lithology in this connection?

R. H. JOHNSON.—Lithology in this sense means change of porosity. In reference to what Mr. Hager has just said, any of us, no matter how strongly convinced we are that lithology requires consideration, quite agree that structure is the thing to be sought for in locating the test well. Lithology comes to be important after the test well. When we come to

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feel out from the first well to get additional wells, we need to keep up promptly and accurately the subsurface mapping so as to know our underground structure and lithology as well as possible.

Overconfidence in the structure as made out at the surface should not lead us to neglect any information we can get from better logs and better studies from those logs.

DORSEY HAGER.—I would like to have Mr. Johnson answer the question why it is that so many of these terraces are not one-sand propositions. Where there are one, two, three or four sands in a field we find very often that all those sands produce. It is not likely that lithological conditions will be the governing factor with all those sands; it might be with one sand, possibly with two, but with three sands, the chances are very much against it and I do know that some pools of Oklahoma are producing from three sands on well-defined terraces. If lithological character governs, the terrace would play a very small part.

Mr. Johnson brought up the point of pronounced pools underground where the surface shows very little. I have some blue prints here of some work we did a short time ago. At the surface of this pool, the beds show a well-defined nose or plunging anticline. The underground geology, based on well records, show a well-defined closure. The surface, in other words, did not show any closure, in fact showed a gentle dip to the west, and the underground contours based on the sand records show closures.

R. H. JOHNSON.—I quite agree that where you have a series of sands one below another showing oil or gas, you have very strong evidence indeed that you have a strong structural feature. In this instance, if there was a terrace, the terrace indicated a more marked fold below. I am inclined to think that a terrace in the sand would not be important; its importance lies in the hint to us that we have a more marked fold down below the actual producing sand.

The mere arrest of movement of the oil up dip by the terrace, can hardly back up oil enough to make a thick enough pay to be important.

M. M. THOMPSON, Morsemere, N. J.—One point that might well be brought out in connection with the data presented by Mr. Hager is, that the surface formations are more nearly parallel with respect to the oil sands in the case of Oklahoma than most other oil fields. That is really the fundamental feature of structure which makes surface geology as valuable as it is here.

There is pronounced false bedding in practically all of the thick sandstone beds, and extensive areas are blind geologically, due to the lack of sufficient satisfactory outcrops. However, we can frequently follow for many miles a thin layer of limestone which serves as an excellent horizontal marker (or key bed) and basing our elevations on this we can work

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out our changes in dip and determine with reasonable accuracy the surface folding where such a bed is found in the area examined.

Wherever convergence is more pronounced than surface folding, geology based on surface structure alone is of little value in "wild catting." This was demonstrated to me very clearly in West Virginia. I found the thickening to be in the opposite direction from the prevailing dip of the surface beds, the condition being that the formations assume the shape of a folded wedge rather than that of a folded blanket-type series. My structural map of the deep oil sand was worked out from reliable well records in developed areas. In comparing this map with the structural map of the same region based on surface formations, I found that in many important instances the surface dipped as much as 100 ft. per mile in one direction while the oil sand dipped in the opposite direction. This of course results in a discrepancy between the apparent and actual structure in the oil sand, a fact that is well realized by the operators.

In the case of Oklahoma it is impossible to obtain as satisfactory well records as could reasonably be desired. In my first investigations in Oklahoma, I had hoped to base my work largely on well records. I obtained from two of the large old established companies about 100 records covering the district that interested me. Not a single record gave the elevation of the top of the hole, and the classification of the key beds encountered in the wells were not reliable. Naturally such records are of little value except for approximate comparison over widely scattered areas.

While we fully appreciate the fact that in new, undeveloped areas, information derived from well records will be necessarily meager, still the geologist must obtain all the data of that nature he can to determine as far as possible what allowance must be made for convergence, pinching out of the sands and other underground conditions that would modify to some extent at least the significance which would otherwise be placed in pure surface indications.

H. A. WHEELER, St. Louis, Mo. (written discussion).—In the tabulation of 72 pools in Oklahoma, the work of the geologist seems to be subordinate to the results of the wild-catter, as the geologist is given credit for locating 31 of the pools, or say 43 per cent., whereas the wild-catter is credited with having opened up 37 pools, or 51 per cent., while 6 per cent. are uncertain.

To one who is acquainted with the Oklahoma fields, like Mr. Hager, this apparent greater success of the "shooting in the dark" system of the wild-catter is well understood, as the old pools in Oklahoma were discovered before the services of a geologist were utilized. For until 4 years ago, the geologist was conspicuous by his absence in the mid-continent fields, as the operators not only had no faith in him, but he was the laughing stock of the "practical" oil men, who then monopolized the field, and was

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classed with the divining-rod man and other fake oil-finders. Since the great success of the geologists in the famous Cushing pool, the change that has come over the old-time oil operators is revolutionary, as the majority of the large, successful operators now seek and utilize their services where formerly they would have been ashamed to consult them. This is best evidenced by there being a staff of at least 200 in the geological departments of the larger companies, or in consulting offices, where there was one lone, but determined, "voice crying in the wilderness" 4 years ago. Hence the seeming discrepancy in the relative success of intelligently prospecting for oil and "going it blind," as most of the pools were opened in the pre-geological era. For out of 37 pools discovered by the wild-catter, 24, or 70 per cent., were found prior to the employment of geologists.

Three pools credited by Mr. Hager to the wild-catter, or Drumright and Dropright (Cushing), and the Boynton pool, which latter is rated as "questionable," were the result of geologic aid, according to the best information available to the writer. Transferring these three pools to the credit of the geologists and deducting the 24 pools opened up before their advent, this gives the geologist the credit of opening up 34 out of 48 pools within the past 4 years, or 71 per cent. of the successes.

The wild-catter will always be useful, although extremely costly, in discovering new oil or gas pools, as the lense type of structure can only be found by their "hit-or-miss" system, while some terraces are so difficult to locate, or the evidence is so dim, that most geologists do not have the courage to recommend their drilling, and their future discovery will largely rest with the bolder and more courageous wild-catter.

M. M. THOMPSON.—Another topic which might be properly brought up in this connection is, the kind of advice a young geologist is frequently called upon to give.

Many companies in Kansas and Oklahoma have recently organized large geological staffs made up largely of recent technical graduates. The majority of these men have either not had much experience in other fields or have not had an opportunity to see their judgment confirmed by the drill. They, on the one hand, are often not versed in the logic of the "practical operator's wild-cat" methods, and their employers, on the other hand, are not sufficiently acquainted with the technical and difficult problems a geologist sometimes encounters.

Consequently, it frequently happens that a geologist gets an assignment where there is not sufficient evidence to justify a conclusive report. Some of these young men in their eagerness to acquire leases will report their personal guess, and depreciate the meager and unsatisfactory observations they made in the field. They take the optimistic attitude that if the guess was a lucky one it will be a feather in their cap. However, if the drill proves the land to be worthless, the employer will most assuredly

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say "the geologists recommended that lease, they gave us bad advice and we will not place so much confidence in them the next time." The practical operator still assumes a patronizing semi-tolerant attitude toward the geologist, which has replaced the former strong prejudice.

My policy has always been in a case of doubt to merely say that the geological facts are thus and so, and let the leaser do his own guessing. If geologists as a class would confine themselves to actual geological facts, they would stimulate far more confidence in themselves and in the profession than they have in the past.

If a geologist must turn in results that are largely guesswork, even in a blind and difficult area he is worse than useless to his employer. Most operators would prefer to obtain reliable information from a scout and do their own guessing, than to depend upon him.

THE CHAIRMAN (M. L. REQUA, San Francisco, Cal.).—I do not suppose the geologist is infallible by any manner of means; he has to do a certain amount of guessing. I made that statement at a meeting of the geological students of the University of California and I was taken up on it, and told that the function of geology was not a guess.

Now it seems to me that the function of the geologist is to eliminate as much of the chance as possible. I do not suppose there is any individual engaged in the oil business or who contemplates putting money into the oil business who is so foolish as to suppose that the geologist is going to hit it right 100 per cent. of the time, but I am very sure that he is going to hit it right a great deal nearer 100 per cent. of the time than the individual who has not any geological education.

I have been all through that period of decrying geologists in California and I have seen the same revolution take place there that has occurred, you gentlemen have said, in Oklahoma. The geologists have come to be recognized as an extremely important factor in the oil industry, just as they have been recognized for a number of years in the mining industry.

DORSEY HAGER.—In Oklahoma, we have found in summing up the pools that have been discovered by the geologists and those drilled on the geologist's advice, that in one in three of the pools the tests have been successful. That is, 33⅓ per cent. of the geological tests have been successful. Now I am fairly confident, though I do not want to make any statements that anyone can attack me on, but we have found that there is a great deal of structure that does not exist and if those earlier errors of the geologists in Oklahoma and the later ones too, due to careless work, insufficient time, and insufficient knowledge of the country—if those errors are eliminated, and they have been largely in the past 2 years, the percentage of the successes grows much larger and the geologist gets one success in two, tests eliminating from this calculation structures that are known not to exist.

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I. N. KNAPP, Ardmore, Pa. (communication to the Secretary*).—In reply to the discussion of his paper at the above meeting, Mr. Hager said in substance that I was on record in 1912 as saying there was no anticlinal structure in Kansas or Oklahoma. In the *Journal of the Franklin Institute*, November and December, 1912, appeared an article on Natural Gas and Other Bitumens written by myself. I made reference to the anticlinal theory and quote from what I said in 1912 as follows: "Through the investigations of geologists working in the great Appalachian field the structural or anticlinal theory of the accumulation of oil and gas was evolved.

"To such fields as the theory applies it is of economic importance, and the geologist should be consulted to locate favorable points in advance of any drilling."

I then gave a plan and section of a few wells drilled by myself in Kansas and said that the anticlinal theory did not in my opinion apply to this particular oil pool. The fact is, I was told by a prominent geologist that I had no business to find oil where I did, that it was contrary to all theory.

Quoting again from what I said in 1912: "The conclusion is that each oil and gas field must be considered by itself, both from a geologic and operating standpoint. The geologist, operator, or driller expert in one field has to learn the business over again when he goes into a new field.

I think what I said publicly in 1912 holds good today. If anyone wishes to know what Mr. Hager said about that time on the anticlinal theory, I would refer them to his article on the Value of Geology in the Petroleum Industry, which appeared in the *Mining and Engineering World* of Sept. 2, 1911.

* Received Mar. 1, 1917.

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The Possibility of Deep Sand Oil and Gas in the Appalachian Geo-Syncline of West Virginia

BY DAVID B. REGER,* A. B., B. S. C. E., MORGANTOWN, W. VA.

(New York Meeting, February, 1917)

Introduction

THE exhaustion of oil and gas in the United States is proceeding at a rapid pace. This is especially true in fields where the light oils that furnish the most fuel for internal-combustion engines are found, leading to a late estimate by Arnold that the petroleum resources are 36 per cent. exhausted.[1] The further remark by Dr. David T. Day at the New York meeting of the Institute, in February, 1916, that the operators of the country are now scarcely able to supply the ever-increasing demand for gasoline, leads the writer to believe that a determined effort will soon be made to secure deeper producing horizons in those regions that now furnish high-grade oil, and in which pump stations and pipe lines offer convenient means for the economical handling of new production. A study of Arnold's table shows further that the States that produce paraffin oil exclusively are 45 per cent. exhausted, this figure being much higher than the average of paraffin and asphalt States altogether. The most extensive of all the paraffin-oil-producing areas is the Appalachian Field, extending with the Appalachian Geo-Syncline from western New York southwestward through western Pennsylvania, western West Virginia, Ohio, Kentucky, and Tennessee, where oil of high gravity is always secured. Of these States, Pennsylvania and West Virginia rank highest in the quality of their product. In West Virginia, the oil sands dip to a lower level than in any other locality in the Appalachian Geo-Syncline, the Pittsburgh Coal of the Pennsylvanian being only 50 ft. above sea level along the Nineveh Syncline at Wileyville, Wetzel County.[2] The very significant fact that the sands of the Mississippian and Upper Devonian Measures furnished the richest oil pools of the State along this

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* Assistant Geologist, West Virginia Geological Survey.

[1] Ralph Arnold: Petroleum Resources of the United States, *Economic Geology*, vol. 10, p. 710 (December, 1915).

[2] Ray V. Hennen: Marshall-Wetzel-Tyler Report, *West Virginia Geological Survey*, p. 66. 1909.

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deep basin, most of them being non-water-bearing, should give pertinency to such information as may be obtained regarding deeper sands in this region, in which there may be a possible duplication of conditions in the more shallow sands.

Authority for Data

Information has been secured primarily from the published reports of the Geological Surveys of West Virginia, Ohio, Pennsylvania, and Kentucky, supplemented by various special reports and maps of the United States Geological Survey. The southwestward extension of the Chestnut Ridge (Warfield, Campton) Anticline through Kentucky has been made on the authority of a recent publication by James H. Gardner.[3] Several important well records used have never been previously published, having been lately secured by the West Virginia Geological Survey. Due credit has been given in the table of well records to the operators who furnished the various logs, in all cases where this information is available. Special acknowledgment is made to Dr. I. C. White, State Geologist of West Virginia, for many valuable suggestions from his extensive knowledge of the region under discussion.

Description of the Appalachian Geo-Syncline

The Appalachian Geo-Syncline, lying west of the Appalachian Mountain System, and being roughly parallel to it, extends in a south-southwestward direction from New York State through Pennsylvania, West Virginia, Kentucky, Tennessee, and Alabama. As partially shown on Plate 1 (in those regions where detailed structure maps are available) it has a course almost straight through Pennsylvania from Brookville, via Kittanning, Pittsburgh, and Washington, to the southwestern corner of the State where it enters West Virginia and extends southwestward across Wetzel, Tyler, and into Ritchie, where the Burning Springs Anticline cuts diagonally across it shifting the line of its axis nearly 20 miles farther west, to a point near Parkersburg. From this point its course is roughly southwest through Wood, Jackson, Mason, Cabell, and Wayne Counties, to the Kentucky State line 7 miles south of Catlettsburg. Its course through Kentucky is apparently interrupted by the Chestnut Ridge (Campton) Anticline which cuts across it, separating it from its southern extension.

Geology of the Appalachian Basin

The surface geology along the axis of the Appalachian Basin is principally that of the Coal Measures, but at the eastern rim these rocks rise

[3] James H. Gardner: A Stratigraphic Disturbance through the Ohio Valley, Running from the Appalachian Plateau in Pennsylvania to the Ozark Mountains in Missouri. *Bulletin of the Geological Society of America*, vol. 26, pp. 477–483 (December 4, 1915).

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rapidly toward the Allegheny and other mountain ranges of the Appalachian System until they disappear above the tops and the rocks of the

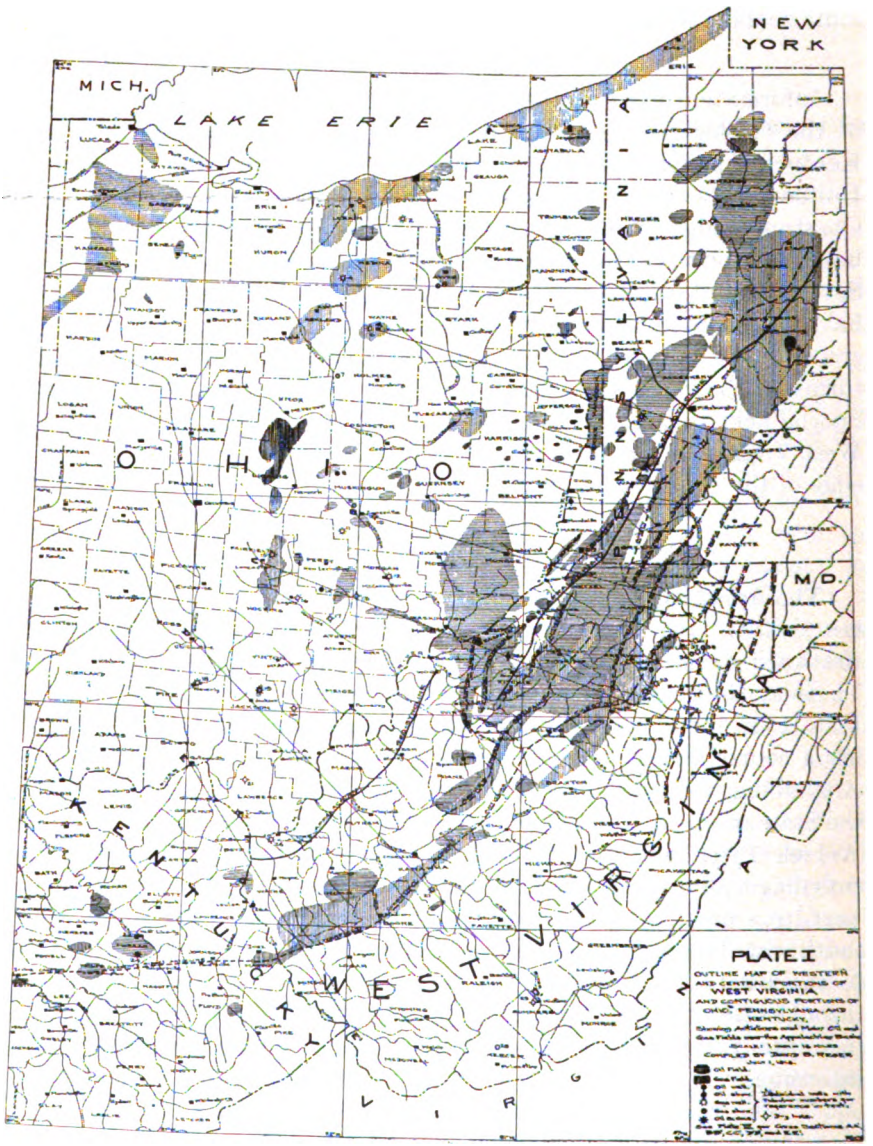

PLATE 1.—OUTLINE MAP SHOWING ANTICLINES AND MAIN OIL AND GAS FIELDS NEAR THE APPALACHIAN BASIN.

Devonian, Silurian and lower formations come to the surface, the pitch in this region being abrupt and irregular. On the western side of the axis the measures rise gradually toward the Cincinnati Anticline, showing

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none of the spectacular structural disturbances visible on the east. Northeastward from West Virginia, through Pennsylvania, the strata rise gradually along the axis of the basin, but the Pennsylvanian still forms the surface rocks at the New York State line, near Olean. Southwestward from West Virginia through Kentucky, the rocks rise along the axis, but very gradually, so that the Pennsylvanian still makes the surface deposits at the Tennessee line.

Plate 2 shows a columnar section made for the locality of Marion, Harrison, Wetzel, and western Monongalia Counties, West Virginia, where the rocks of the Appalachian Basin dip to their lowest level, and where the oil reservoirs of the State are the most prolific. In this region the surface rocks are principally the transitional Permo-Carboniferous type represented by the Dunkard Series. Throughout this region, numerous drillings are available that extend to the base of the Catskill Series, leaving no uncertainty as to the character of the sediments down to this level. Below the Catskill, the section is based on the deep wells drilled in Pennsylvania (Nos. 41 and 42 on Plate 1) and on numerous drillings in Morgan, Muskingum, and other Counties in Ohio where the Devonian shales have less thickness than in southern Pennsylvania, and on the fairly well-known thickness and character of these Devonian and Ordovician beds in the Alleghany Mountain region of West Virginia, farther east where these measures outcrop. This surrounding information makes the portion of the section below the Catskill more than hypothetical, the probability being that the intervals shown are fairly good while some of the individual formations may be locally absent.

Dunkard Series.—The Dunkard Series, composed of 1,150 ft. of sandy, red, and variegated shales, gray sandstones, with a few thin coals and limestones, is barren of oil and gas in this region and the lower 300 to 400 ft. of the series found below the main drainage channels must be drilled through without result.

Monongahela Series.—The Monongahela Series, composed of 400 ft. of gray sandstones, gray shales, limestones and coal beds, is also practically barren of oil and gas, and is encountered by the drill everywhere along the deeper part of the basin. At its base is the great Pittsburgh Coal bed used widely as a key rock by the drillers of Pennsylvania and West Virginia.

Conemaugh Series.—The Conemaugh Series, comprising 500 to 600 ft. of gray sandstones, red and sandy shales and a few coals and limestones, contains the Little Dunkard sand in the lower portion and the Big Dunkard at its base, both of which have produced oil at several localities in the State.

Allegheny Series.—The Allegheny Series, 250 ft. thick and composed of gray sandstones, gray shales and having a few coals and thin limestones, contains the Burning Springs and Gas sands, that produce oil in great quantity along the Burning Springs Anticline.

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COLUMNAR SECTION FOR CENTER OF WEST VIRGINIA OIL FIELDS

(Marion and Surrounding Counties)

Vertical Scale: 9/16″ = 1000 Ft.

		Name of Series	Columnar Section	Thickness Feet	Total Feet	Description
Permo-Carboniferous		Dunkard		1150	1150	Variegated Shales and Gray Sandstones with a few Thin Coal Beds
Carboniferous	Pennsylvanian	Monongahela (Pittsburgh Coal at Base)		400	1550	Gray Sandstones, Gray Shales, Limestones, and Coal Beds
		Conemaugh		600	2150	Gray or Brown Sandstones, Gray and Red Shales, and Coal Beds
		Allegheny		250	2400	Gray Sandstones, Gray Shales, and Coal Beds
		Pottsville (Salt Sands of W.Va.)		300	2700	Gray Sandstones and Shales, with a few Coal Beds
	Mississippian	Mauch Chunk (Contains Maxton Sand of W.Va.)		250	2950	Red Shales with a few Thin Sandstones
		Greenbrier ("Big Lime" of W.Va.)		100	3050	Limestone
		Pocono (Big Injun at Top; Berea Sand at Base)		500	3550	Gray Sandstones and Gray Shales
Devonian		Catskill (Gordon Group of Oil Sands)		800	4350	Brown Sandstones and Red Shales
		Chemung (No Productive Sands in W.Va.)		1500(?)	5850	Olive Brown, Shales with Sandstone Lentils
		Portage (No Productive Sands in W.Va.)		800(?)	6650	Gray Shales with Sandstone Lentils
		Hamilton (No Productive Sands in W.Va.)		700(?)	7350	Brown, Shales with Sandstone Lentils
		Marcellus or Romney (Gas in Ohio and Kentucky)		300(?)	7650	Brown or Black Bituminous Shales with Sandstone Lentils
		Corniferous Limestone (Raglund Sand of Kentucky)		50(?)	7700	Dark Flinty Limestone
		Oriskany		150(?)	7850	Gray Sandstone
Silurian		Helderberg Salina, and Niagara ("Big Lime" of Ohio)		800(?)	8650	Limestone
		Clinton		200(?)	8850	Variegated Shales
		Medina White Sandstone ("Clinton" Oil Sand of Southern Ohio)		50(?)	8900	White Sandstone
		Medina Shales		500(?)	9400	Red Shales and Thin Sandstones
Ordovician		Martinsburg or Cincinnati Shale (Contains Hudson Sand of Kentucky)		500(?)	9900	Gray Shales with Sandstone Lentils
		Utica		300(?)	10200	Black Shales with Sandstone Lentils
		Trenton and other Limestones (Oil and Gas Horizon of Northern Ohio)		1200(?)	11400	Limestones

PLATE 2.—COLUMNAR SECTION FOR CENTER OF WEST VIRGINIA OIL FIELDS.

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Pottsville Series.—The Pottsville Series, composed of gray conglomeratic sandstones, gray shales, and having numerous coal beds, varies in thickness from about 200 ft. in the northern panhandle to nearly 4,000 ft. at the southern end next to Virginia. Along the axis of the Appalachian Basin, its thickness at the Pennsylvania line is about 250 ft., and this increases gradually to nearly 1,000 ft. at the Kentucky line. Oil is often found in several of its sands, the upper one being called the Second Cow Run, and the lower ones being usually termed the First, Second, and Third Salt sands. Copious flows of salt water are usually found in one or more of these horizons, giving rise to their names.

Mauch Chunk Series.—The Mauch Chunk Series, composed mainly of red and green shales, with a few flaggy green fine-grained sandstones and impure limestones, varies in thickness from only a few feet in some of the western counties to about 3,500 ft. in Summers and Mercer. Along the oil belt it is usually about 250 ft. thick, with the Maxton oil and gas sand near the base.

Greenbrier Limestone.—The Greenbrier limestone, usually a solid mass of hard limestone, varies in thickness from 50 ft. in the northern panhandle to 1,500 ft. in Greenbrier and other southern Counties. Along the Appalachian Basin, it averages about 100 ft. and is known as the "Big Lime," and is the most dependable key rock below the Pittsburgh Coal, as the intervals from it to the lower sands are fairly constant over wide areas. It is seldom a producing horizon but makes some oil and gas from sandy streaks in Lincoln, Wayne, and Mingo Counties.

Pocono Series.—The Pocono Series, composed of gray sandstones and gray shales, is usually about 500 ft. thick in all the counties along the Appalachian Basin, and contains the Keener, Big Injun, Squaw, Weir, and Berea sands. Of these the Big Injun is the most prolific oil and gas horizon of the State, possibly deriving its oil from the Big Lime which often rests directly upon it; the Berea sand is a close second to the Injun, being of widespread occurrence and often productive over wide areas.

Catskill Series.—The Catskill Series, composed of red or brown sandstones, separated by red shales, varies from 500 to 800 ft. in thickness and contains the Gantz, Fifty-foot, Thirty-foot, Gordon Stray, Gordon, Fourth, Fifth or McDonald, Sixth or Bayard, and Seventh or Elizabeth sands. Of these the Gordon is the most valuable producer, ranking with the Berea in the quantity of oil but not having such a wide occurrence. The sands of this series are the lowest that have been commercially productive in the State. Southward toward Kentucky, the series thins out completely, being scarcely represented at all south of the Great Kanawha River.

Chemung, Portage and Hamilton Series.—The Chemung, Portage, and Hamilton, composed of green, gray and brown shales respectively, with lenticular sandstones, the Chemung being probably about 1,500 ft., the

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Portage about 800, and the Hamilton about 700, contain no productive sands in West Virginia, although the Speechley sand of the Chemung has made shows of oil and gas in various wells. In northern Pennsylvania, there are several productive horizons in this group. Westward in Ohio and southward toward Kentucky, these three series thin out almost completely, leaving the Berea sand only 700 to 1,000 ft. above the Corniferous limestone. These shales form a great barrier that has prevented any exploitation of the lower Devonian sands in northern West Virginia.

Marcellus Shale.—The Marcellus shale, known as the Ohio black shale in the State of that name and as the Chattanooga shale in Tennessee, and often called simply the Devonian black shale, is probably about 300 ft. thick along the Appalachian Basin in West Virginia. It is a black, bituminous horizon, and contains a few lenticular gas sands in Ohio and Kentucky.

Corniferous Limestone.—The Corniferous limestone, probably not more than 50 ft. thick in West Virginia, is the Ragland oil sand of Menefee and Morgan Counties, Kentucky. It has been penetrated in West Virginia by only three wells near the Kentucky line (Nos. 26, 26A, and 27 on Plate 1), and in two or three wells in Randolph and Tucker Counties in the Alleghany Mountains, the detailed records of only one of which (No. 32) has been obtained. It is probable that the most eastern oil counties of the State do not have this horizon, as its presence is not noted in the Alleghany Mountains where it should crop, and where it should be easily recognized from its well-known flints and fossils.

Oriskany Sandstone.—The Oriskany sandstone, gray in color and probably 150 ft. thick, and a producer of oil and gas in southern Indiana and central New York, was found in the Geary Well (No. 42 on Plate 1) in Pennsylvania, where it was 155 ft. thick, and in the Slaughter Creek Well (No. 27 on Plate 1) in Kanawha County, West Virginia, where it was 15 ft., and in the Parsons Well (No. 32 on Plate 1), where it was 80 ft. West of the basin it is not noted in the deep wells of Ohio but on the eastern side of the Alleghany Mountains it is the well-known glass sand of Morgan County, West Virginia, and it seems likely that it is represented along the basin in West Virginia.

Helderberg, Salina, and Niagara.—The Helderberg, Salina, and Niagara limestones, often comprising a solid limestone mass 800 ft. or more thick, and known in Ohio drilling parlance as the "Big Lime," has been penetrated in West Virginia only by the Bartram (No. 26A) and Slaughter Creek (No. 27) Wells, and was found in the Geary (No. 42) Well in Pennsylvania. Since this great limestone mass is found in these three wells and is universally present in Ohio, and is also found in much the same great development in West Virginia east of the Alleghanies, it seems safe to conclude that it is present along the Appalachian Basin with the same or greater thickness. It is possibly the parent reser-

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voir from which much of the White Medina sandstone, or "Clinton Sand" oil of Ohio is derived, so that its presence along the basin is of special interest.

Clinton Shales.—The Clinton variegated shales are usually found in Ohio between the "Big Lime" and the "Clinton" sand, varying in thickness from 50 to 200 ft., and probably occur in West Virginia along the Basin.

Medina White Sandstone.—The "Clinton" oil sand of southern Ohio, believed by I. C. White and others to represent the Medina white sandstone, is the great oil and gas-producing stratum of Hocking, Fairfield, Perry, and other southern Ohio Counties, being usually 10 to 50 ft. thick. No drill has ever reached it in West Virginia, but it is present in the Alleghany Mountains east of the oil fields and probably occurs along the Basin.

Medina Shales.—The Medina variegated shales, with probable lenticular sandstone horizons, noted in many deep wells in southern Ohio, may have a thickness of 500 ft. along the Appalachian Basin.

Martinsburg Shale.—The Martinsburg shale, often called the Cincinnati or Hudson, is found in Ohio, Kentucky, and eastern West Virginia, and may therefore be taken for granted along the Appalachian Basin in West Virginia. According to Hoeing,[4] this series furnishes several oil and gas sands in Wolfe, Morgan and other Kentucky Counties. Its thickness is possibly 500 to 1,000 ft. along the Appalachian Basin.

Utica Shale.—The Utica formation, consisting of black shales with probable sandstone lentils, may have a thickness of 300 ft. along the basin, although the information regarding it in surrounding States is vague and uncertain.

Trenton Limestone.—The Trenton group, composed of several thick limestones, often coalesced into one thick mass 1,000 to 1,200 ft. thick, is the great oil and gas-producing zone of north-central Ohio and also is productive in Indiana, central New York, and in some parts of Kentucky. The fact that it occurs in these surrounding States and also crops in great thickness in West Virginia east of the Alleghanies seems to leave little doubt that it occurs also, with its usual great thickness, along the Appalachian Basin.

Cross-Sections

Based on the evidence of the numbered wells shown on Plate 1, many of which were very deep tests, and on many hundreds of oil and gas wells, the records of which are published in the West Virginia State Reports, and also on the outcropping rocks around the rim of the Appalachian Basin, five cross-sections, the geographic locations of which are shown

[4] J. B. Hoeing: Oil and Gas Sands of Kentucky, *Bulletin No.* 1, *Kentucky Geological Survey*, pp. 39–40 (1905).

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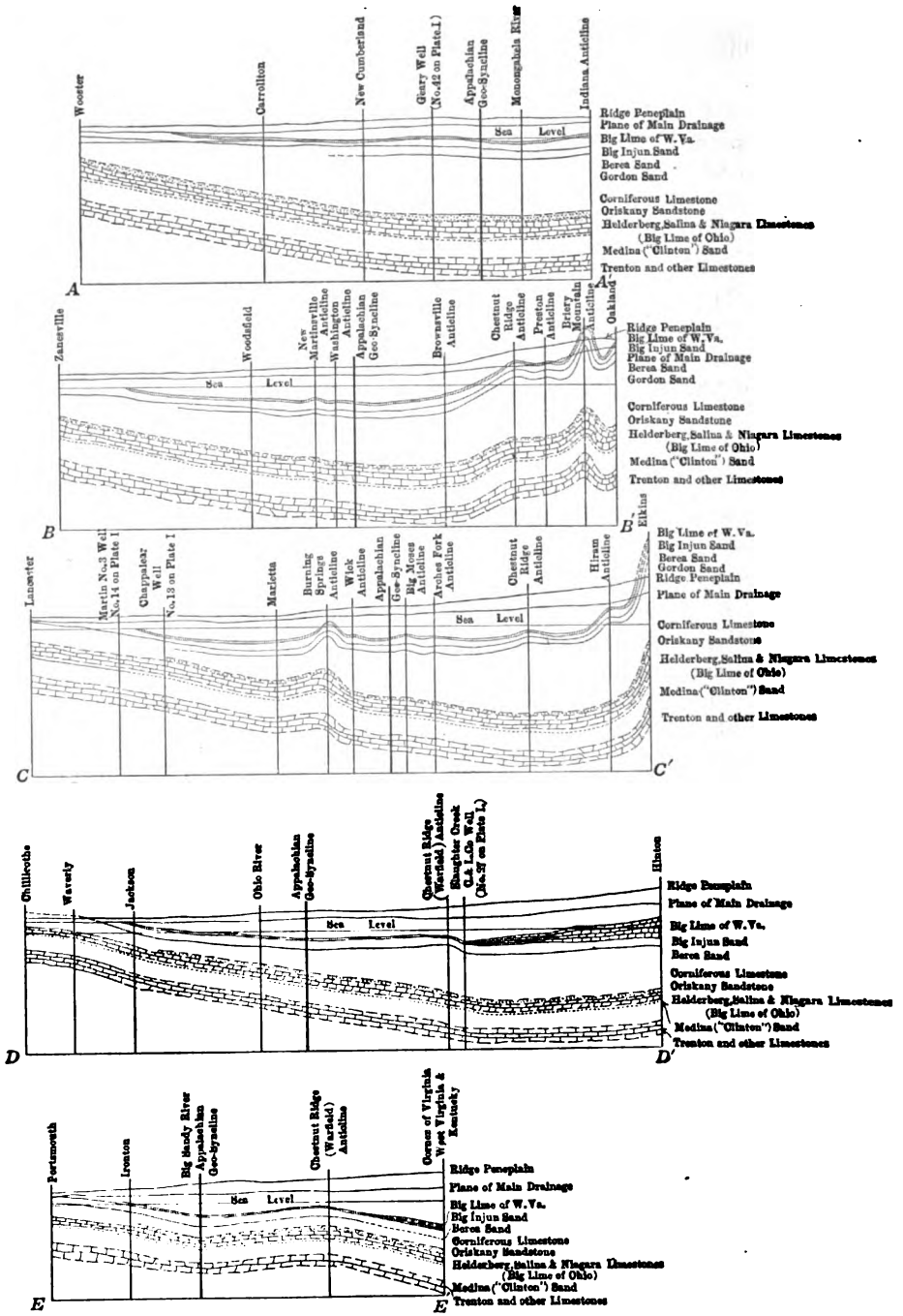

CROSS SECTIONS

Horizontal Scale: 11/32 = 16 Miles
Vertical Scale: 11/32 = 4000 Ft.

Solid Lines Indicate Sands Determined from Well Records
Dotted Lines Indicate Supposed Position of Deep Sands as Determined from a few Deep Wells and from Intervals East and West of Appalachian Basin

PLATE 3.—CROSS-SECTIONS AS INDICATED ON PLATE 1.

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on Plate 1, have been plotted across the basin in West Virginia (Plate 3). The scale of the plates does not permit the representation in detail of the surface hills and valleys but a solid line shows the ancient peneplain surface of the ridges, and another shows the plane of the main drainage channels with reference to sea level, from which the approximate interval from this latter plane to the principal sands may be determined, as all estimates for drilling must start from this drainage plane. Formations shown with solid lines are plotted from actual records but the gaps are filled in with dotted lines to represent the probable positions of the various deep sands and limestones as outlined above. Of the more shallow sands, only two or three are plotted for use as key rocks. These cross-sections reveal the very interesting probability that the well-known gradual eastward thickening of the Chemung, Portage, and Hamilton shales shifts the axis of the Appalachian Basin of the lower limestones and sands many miles farther east than the position it occupies when referred to the formations above these shales.

Deep-Well History

The accompanying table, containing the abbreviated records of a large number of wells, has been compiled from the various sources indicated to show the different deep tests that have been made in West Virginia and in neighboring States to find oil in the sands below the Catskill Series. In Ohio, where hundreds of productive wells have been drilled to the "Clinton" and Trenton, only those have been selected that lie nearest to the Appalachian Basin, being located along a fairly straight line reaching from Lake Erie to the Ohio River, and in Kentucky only a few wells are listed to show the typical formations and intervals. Each well is given a tabular number that corresponds to the same number placed beside the well symbol on Plate 1.

Of the wells tabulated for West Virginia, only four (Nos. 26, 26A, 27 and 32) were drilled as deep as the Corniferous limestone, and only three of these went through the Oriskany sandstone into the Niagara and Helderberg limestone. No well in the State has ever been drilled into the Medina White sandstone ("Clinton" oil sand of Ohio) or into the Trenton limestone. All the wells drilled to the Oriskany sandstone have been somewhat distant from the main oil fields of the State and are therefore indecisive as tests of its oil and gas possibilities. Briefly, then, the deep sands not yet tested in the main West Virginia oil fields are the Corniferous limestone ("Ragland" of Kentucky), Oriskany, Medina white sandstone ("Clinton" of Ohio), and the Trenton limestone, with a possibility of another sand horizon in the Martinsburg (Hudson) group, which produces oil in Kentucky.

The well most typical of the formation and conditions likely to be

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Summarized Record of Deep

No. on Plate 1	Farm Name and Number and Oil Company	County and State	Elevation above Tide, Feet	Big Lime of W. Va.		Berea Sand		Corniferous Limestone		Oriskany Sandstone	
				Depth Top, Feet	Thickness, Feet	Depth Top, Feet	Thickness, Feet	Depth Top, Feet	Thickness, Feet	Depth Top, Feet	Thickness, Feet
1	J. A. Giddings lot, Jefferson O. & G. Co.	Ashtabula, Ohio.	...	...	...	...	...	...	...	...	...
2	Garfield & Caine, East Ohio G. Co.	Cuyahoga, Ohio.	780	...	...	10	80	...	...	...	...
3	Citisens No 1.	Lorain, Ohio.	...	...	...	...	...	...	...	...	...
4	E. S. Albert No. 1.	Medina, Ohio.	920	...	...	386	10	...	...	...	...
5	Barberton Chem. Co. No. 1.	Summit, Ohio.	...	...	...	460	10	2,180	...	...	...
6	Wooster well, H. B. Odenkirk.	Wayne, Ohio.	860	...	...	465	30	...	...	...	...
7	Kaylor.	...	...	...	...	615	10	...	...	...	...
8	Fairall.	Muskingum, Ohio.	...	...	...	834	16	...	...	...	...
9	Dillon Falls well, Chicago-Zanesville O. & G.	Muskingum, Ohio.	...	...	...	895	12	...	...	...	...
10	George Handchsy, Zanesville G. & O. Co.	Muskingum, Ohio.	...	...	..	1,010	23	...	...	...	...
11	W. J. Roberts.	Muskingum, Ohio.	...	250	38	847	14	...	...	...	...
12	McConnellsville Fair Grounds, McConnellsville City.	Morgan, Ohio.	725	625	44	1,279	66	3,082	2	3,125	8
13	T. J. Chappalear, J. P. Fishel.	Morgan, Ohio.	750	...	...	1,150	20	...	...	...	...
14	Martin No. 3. E. S. Martin.	Perry, Ohio.	...	...	...	920	20	...	...	...	...
15	Crouse No. 1. B. S. Stretton.	Fairfield, Ohio.	...	...	...	...	...	...	...	...	...
16	Logan town, Logan town.	Hocking, Ohio.	748	...	...	683	45	...	...	...	...
17	Columbus Hocking C. & I. Columbus Hocking C. & I.	Athens, Ohio.	...	...	...	...	...	...	...	...	...
18	Waverly O. & Gas. Co. Waverly O. & Gas Co.	Pike, Ohio.	575	...	...	...	...	...	...	...	...
19	Buckeye Coal Co. No. 2. Buckeye Coal Co.	Jackson, Ohio.	...	...	...	640	20	...	...	...	...
20	At Vinton village.	Gallia, Ohio.	...	...	...	...	...	...	...	...	...
21	At Mt. Vernon village.	Lawrence, Ohio.	...	...	...	1,095	25	...	...	...	...
22	At Ironton town.	Lawrence, Ohio.	500	...	...	1,010	47	...	...	...	...
23	Burns well.	Morgan, Kentucky,	...	368	106	1,095	10	...	...	...	...

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Wells Shown on Plate 1

Helderberg, Salina and Niagara, "Big Lime of Ohio"		Medina ("Clinton") Sand		Trenton Limestone		Total Depth, Feet	Reference to Detailed Record	Remarks
Depth Top, Feet	Thickness, Feet	Depth Top, Feet	Thickness, Feet	Depth Top, Feet	Thickness, Feet			
1,810	288	2,127	13	...	...	2,140	Ohio G. S. 4th series, *Bull.* 1, p. 303.	Oil well in "Clinton" sand.
1,210	1,455	2,680	...	...	...	2,770	Ohio G. S. 4th series, *Bull.* 12, p. 65.	Heavy flow of water and oil show in Niagara lime. Gas in "Clinton" sand.
440	850	...	...	2,645	...	....	Ohio G. S. 4th series, *Bull.* 12, p. 64.	No Clinton" sand.
1,512	1,127	2,749	14	...	...	2,777	Ohio G. S. 4th series, *Bull.* 12, p. 62.	
2,180	350	2,500?	30	...	...	3,006	Ohio G. S. 4th series, *Bull.* 1, p. 296.	Gas show(?) in "Clinton" sand. Rock salt and lime in lower 300 ft. of hole.
1,830	1,085	3,104	31	...	...	3,140	Ohio G. S. 4th series, *Bull.* 12, p. 60.	1,440,000 cu. ft. of gas daily and oil show in "Clinton" sand. Heavy flow of brine at 2,055 ft.
1,725	960	2,851	28	...	...	2,957	Ohio G. S. 4th series, *Bull.* 12, p. 59.	Gas show in "Clinton" sand.
2,035	934	3,141	21	...	...	3,194	Ohio G. S. 4th series, *Bull.* 12, p. 37.	"Clinton" sand oil well.
2,120	1,275	3,543	21	...	...	3,567	Ohio G. S. 4th series, *Bull.* 12, p. 35.	Salt water 2,560 ft. and 3,205 ft. Shows of oil and gas in "Clinton" sand.
2,640	1,005	3,709	38	...	...	3,805	Ohio G. S. 4th series, *Bull.* 12, p. 36.	Dry hole.
2,195	970	3,296	...	...	...	3,314	.....	Gas in "Clinton" sand.
...	...	...	...	...	...	3,186	Ohio G. S. 4th series, *Bull.* 1, p. 145.	Gas in Berea sand.
2,700	900	3,935	9	...	...	3,947	Ohio G. S. 4th series, *Bull.* 12, p. 38.	No oil or gas; 1 bailer of water per hour in Niagara lime.
2,170	1,170	3,175	17	...	...	3,213	Ohio G. S. 4th series, *Bull.* 12, p. 23.	Oil well in "Clinton" sand.
1,378	...	2,180	5	3,470	...	3,569	Ohio G. S. 4th series, *Bull.* 12, p. 28.	No oil or gas noted in record, but was probably a "Clinton" gasser.
1,715	715	2,627	18	...	...	2,694	Ohio G. S. 4th series, *Bull.* 12, p. 39.	Dry hole.
...	...	...	...	...	...	....	Ohio G. S. 4th series, *Bull.* 12, p. 41.	Drilled to "Clinton" sand. No oil or gas.
485	490	...	...	1,960	808	3,335	Ohio G. S. 4th series, *Bull.* 12, p. 48	Some salt water and gas in Niagara lime; oil and gas show in Trenton.
660	1,454	2,255	8	...	...	2,315	Ohio G. S. 4th series, *Bull.* 12, p. 44.	4,000,000 cu. ft. of gas daily in "Clinton" sand.
...	...	...	...	...	...	3,200	Ohio G. S. 4th series, *Bull.* 12, p. 44.	Dry hole. No "Clinton" sand.
1,810	620	...	...	...	...	2,730	Ohio G. S. 4th series, *Bull.* 12, p. 50.	Dry hole. No "Clinton" sand.
1,827	584	...	...	3,440?	...	3,600	Ohio G. S. 4th series, *Bull.* 12, p. 49.	Dry hole. No "Clinton" sand.
...	...	1,408	94	...	...	1,508	Ky. G. S. *Bull.* 1, p. 159.	"Clinton" sand oil well.

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No. on Plate 1	Farm Name and Number and Oil Company	County and State	Elevation above Tide, Feet	Big Lime of W. Va.		Berea Sand		Corniferous Limestone		Oriskany Sandstone	
				Depth Top, Feet	Thickness, Feet	Depth Top, Feet	Thickness, Feet	Depth Top, Feet	Thickness, Feet	Depth Top, Feet	Thickness, Feet
24	Caney Creek well, G. M. Sullivan.	Morgan, Kentucky	...	453	69	1,035	24	1,370	...	...	...
25	Blaine Creek well.	Lawrence, Kentucky	...	...	...	1,660	2	2,592	−5	...	...
26	Central City well, T. H. Harvey.	Cabell, W. Va.	530	970	150	1,730	25	2,760	10	...	...
26 A	David Bartram No. 483, United Fuel Gas Co.	Wayne, W. Va.	595	1,220	80	1,875	20	2,760	410	...	...
27	Slaughter Creek C. & L. Co. W. S. Edwards Oil Co.	Kanawha, W. Va.	640	1,453	207	2,093	12	4,945	90	5,035	15
28	Griffith No. 1, Rich Creek O. & G. Co.	Mercer, W. Va.	2,090	...	...	...	...	...	...	...	...
29	B. T. Baker No. 1, Summers O. & G. Co.	Summers, W. Va.	1,586	1,020	1,057	2,600	25	...	...	...	...
30	P. C. Daniels No. 1, Tygarts Valley Oil Co.	Randolph W. Va.	1,990	...	...	...	...	...	...	...	...
31	Alonso W. Murphy No. 1. Mead *et al.*	Randolph, W. Va.	1,995	...	...	...	...	...	...	...	...
32	Parsons Pulp & Lumber Co. Parsons Pulp & Lumber Co.	Tucker, W. Va.	1,655	...	...	...	...	3,825	20	3,980	80
33	J. C. Benson No. 3,612 Hope Natural Gas Co.	Barbour, W. Va.	1,026	1,235	90	1,615	138	...	...	...	...
34	Porter Maxwell No. 3,869, Hope Natural Gas Co.	Harrison, W. Va.	1,050	1,531	...	...	...	...	...	...	...
35	M. J. Coplin No. 1, Tri-State Gas Co.	Harrison, W. Va.	1,030	1,288	77	1,711	13	.....	...	...	...
36	Absalom Knotts No. 1, Greensboro Gas Co.	Taylor, W. Va.	1,460	960	50	1,460	120	...	...	...	...
37	R. S. Hauser No. 1, Greensboro Gas Co.	Taylor, W. Va.	1,480	950	40	...	...	...	...	...	...
38	Russell McCartney No. 1, Greensboro Gas Co.	Taylor, W. Va.	1,580	1,055	35	1,500	90	...	...	...	...
38 A	T. E. Dye No. 1, Am. Hydroscope Co.	Ritchie, W. Va.	...	...	...	...	...	...	...	...	...
39	J. F. Dobbs No. 1, South Penn Oil Co.	Marshall, W. Va.	980	1,760	30	...	...	...	...	...	...
40	Boggs Run well, Wheeling Dev. Co.	Marshall, W. Va.	900	...	...	...	...	...	...	...	...
41	Wm. Bedell, Forest Oil Co.	Allegheny Penna.	...	1,050	50	1,515	50	...	...	...	...
42	R. A. Geary No. 1,770, Peoples Nat. Gas Co.	Washington, Penna.	1,050	953	29	1,610	12	6,008	37	6,045	155
43	Witherop, Conway Bros.	Venango, Penna.	955	...	...	...	...	3,870	10	...	...

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Helderberg, Salina and Niagara, "Big Lime of Ohio"		Medina ("Clinton") Sand		Trenton Limestone		Total Depth, Feet	Reference to Detailed Record	Remarks
Depth Top, Feet	Thickness, Feet	Depth Top, Feet	Thickness, Feet	Depth Top, Feet	Thickness, Feet			
...	...	1,408	...	...	...	2,021	Ky. G. S. *Bull.* 1, p. 72.	Oil and gas in "Clinton" and Caney Sands. Salt water in Clinton. Probably stopped just short of Trenton.
...	...	...	...	...	...	2,597	Ky. G. S. *Bull.* 1, p. 78.	Gas in Pottsville Conglomerate and Berea sand.
...	...	...	...	...	...	2,770	W. Va. G. S. Vol. I (A), p. 495.	Oil and gas in Berea sand; gas in bastard limestone at 2,485 ft.
2,760	410	...	...	...	...	3,130	Unpublished.	Dry hole; oil show in Weir sand at 1,790 ft.; Corniferous and Niagara recorded together.
5,050	545	...	...	...	...	5,595	W. Va. G. S. Kanawha Report, p. xix.	Squaw sand gas; water in Niagara Lime at 5,592 ft.
...	...	...	...	...	...	4,105	W. Va. G. S. Raleigh Report.	Still drilling; base of Pottsville, 105 ft.
...	...	...	...	...	...	2,664	W. Va. G. S. Raleigh Report.	Dry hole.
...	...	...	...	...	...	2,000?	W. Va. G. S. Barbour-Upshur-Randolph Report.	Starts near base of Chemung Series. Shows of oil and gas.
...	...	...	...	...	...	2,385	W. Va. G. S. Barbour-Upshur-Randolph Report.	Starts near base of Chemung Series. Dry hole.
...	...	...	...	...	...	4,250	Not published.	Starts near base of Chemung Series. Pockets of gas at 825 to 835 ft., 1,053 and 3,830 ft. Strong salt water at 3,990 ft. to 4,000 ft.
..	...	...	...	...	...	4,570	W. Va. G. S. Barbour-Upshur-Randolph Report.	Gas in Big Lime, Big Injun, Berea, and Kane(?) sands.
...	....	...	...	...	...	4,386	W. Va. G. S. Barbour-Upshur-Randolph Report.	
...	...	...	...	...	...	4,028	W. Va. G. S. Doddridge-Harrison Report, p. 120.	Little gas in lime and shells at 2,950 ft.
...	...	...	...	...	...	3,955	W. Va. G. S. Barbour-Upshur-Randolph Report.	Gas show at 1,210 ft.
...	...	...	...	...	...	4,437	W. Va. G. S. Barbour-Upshur-Randolph Report.	Light gas in Big Injun sand; Kane(?) sand, 4,108 to 4,162 ft.
...	...	...	...	...	...	3,506	W. Va. G. S. Barbour-Upshur-Randolph Report.	Gas show, 2,425 ft.
...	...	...	...	...	...	4,450	W. Va. G. S. Doddridge-Harrison Report, p. 284.	Flow of oil in Speechley sand at 3,125 ft.
...	...	...	...	...	...	3,411	W. Va. G. S. Marshall-Wetzel-Tyler Report, p. 390.	Gordon sand oil; Speechley sand, 3,344 to 3,376 ft. with some dark oil.
...	...	...	...	...	...	4,500	W. Va. G. S. Vol. I, p. 364.	Base of Big Injun at 1,570 ft.; show of oil at 2,955 ft.
...	...	...	...	...	...	5,575	W. Va. G. S. Vol. I (A), p. 104.	Speechley sand, 3,150 to 3,165 ft.; Bradford sand, 3,500 to 3,520 ft. with trace of oil; hole stopped in Marcellus shale.
...	...	...	...	...	...	6,299+	G. S. A., *Bull.* Vol. 24, p. 275, 1913.	Gas, 4,850 ft., 4,870, 5,900, 5,905, 5,910, 5,915, 6,060 ft. Not yet completed.
...	...	...	...	...	...	3,880	W. Va. G. S. Vol. I (A), p. 86.	Slight shows of oil and gas.

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encountered in drilling for these deep sands in West Virginia is the Geary deep well (No. 42 on Plate 1) drilled near McDonald, Pa., and only about 50 miles due north of the main West Virginia fields. This well holds the unique distinction of being the deepest hole ever drilled in the United States, and its record down into the Ohio "Big Lime" was published by I. C. White.[5] Judging from the record of this well and that of the Slaughter Creek (No. 27) deep well, it seems likely that troublesome salt water may be expected in the Oriskany and Salina beds along the minor synclines that lie parallel to the main Appalachian Basin, and that oil and gas will more probably be found along the axes of the anticlines or at no great distance down their slopes.

This theory that oil may be found principally along the anticlines is contrary to the experience of operators who have exploited the sands of the Catskill Series in the same region, and who have learned through the drilling of many thousands of wells that these latter sands are usually not water-bearing and the oil is therefore almost universally found along the synclines.

Whether deep-sand oil and gas exist in commercial quantity along the Appalachian Basin in West Virginia will be known only through the ultimate test of the drill, but the fact that these sands are widely productive in Ohio, Kentucky, Indiana, and New York, in regions where structural relief is similar to that in West Virginia, and the further strong probability, and almost certainty, that these porous reservoirs actually exist in close proximity to the great limestones that, along with the Devonian black shales, are evidently their main source of oil in other localities, indicates that they will be found commercially productive along the Basin. The fact that the bituminous shales are undoubtedly much thicker in West Virginia than in Ohio, Kentucky and Tennessee indicates an even more hopeful possibility of oil.

If the doubt as to the presence of oil be dismissed as unreasonable, the problem of the operator resolves itself into the double question; first, as to the location of the pools of oil and gas; and, second, as to the mechanical possibility of drilling wells 5,000 to 10,000 ft. deep. As outlined above, the most favorable localities seem to be along or near the axes of the anticlines where the danger of finding salt water would be minimized and where the gas, if any, should most certainly collect. The search for oil would naturally be made on either side of the anticlines by making locations at increasing distances from the axes.

That drilling may be carried on successfully to the great depths indicated by the figures above seems open to little serious doubt. In 1860, when the search for oil and gas may be said to have been actively begun in the Appalachian Basin, few, if any, wells had been drilled to a

[5] I. C. White: Notes on a Very Deep Well Near McDonald, Pennsylvania, *Bulletin of the Geological Society of America*, vol. 24, p. 275 (June 10, 1913).

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depth of 1,000 ft. In 1890, when development was at its height, 3,000 ft. was considered an unusual depth, but by 1910 several holes had been drilled more than 5,000 ft. and at the present time at least two wells in the world have been drilled more than 7,000 ft. The advance in drilling has been just as rapid as in any other line of human endeavor where mechanical ingenuity and skill must be used and in these qualifications the Scotch-American well driller stands second to none. To say that drilling cannot be continued to greater depths than the Geary and Slaughter Creek wells would call for a total cessation of the enterprising experimentation that has characterized oil-well drilling from its very beginning.

Suggestions for Drilling

A comparison of the cross-sections on Plate 3, in conjunction with the data exhibited on Plates 1 and 2, shows that ideal conditions may be found along several of the anticlines east and west of the main Appalachian Basin, among which may be mentioned the New Martinsville, Washington, Brownsville, Arches Fork, Big Moses, Chestnut Ridge (known as the Warfield in southern West Virginia), and last and best of all, the Burning Springs, which stretches in a north and south direction for more than 30 miles through the very heart of the oil fields and in a region where the Devonian shales are probably much thinner than at points farther east. A well drilled at the point where this anticline crosses Walker Creek, near Sandhill, 10 miles southward from St. Marys, and 5 miles north of Petroleum on the Baltimore and Ohio Railroad, and 5½ miles south of the point where cross-section *CC'* crosses the anticline, would have a tidal elevation of 920 ft. Being at the summit of the great Volcano dome, it would start at an approximate interval of 1,000 ft. below the Pittsburgh Coal and should find the Big Lime of West Virginia at about 100 ft., the Berea sand at about 600 ft., the Corniferous limestone at about 3,500 ft., the Oriskany sandstone at about 3,550 ft., the Medina white sandstone ("Clinton") at about 4,700 ft., and should reach the Trenton limestone at about 6,000 ft. The dome-shaped structure at this point would almost insure the absence of water in the sands, at the same time making the financial risk much less than at any other point in the well-developed oil fields of the State. Oil or gas secured at this point would immediately open a prospective field for 30 miles along the crest of this great anticline where conditions are similar all the way from St. Marys on the Ohio River to Burning Springs near the corner of Wirt, Roane and Calhoun Counties.

Should this region prove to be commercially productive from the lower sands, attention would naturally turn to the other anticlines mentioned, where drilling would be more expensive but the possibility of finding oil and gas reasonably good.

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Discussion

David B. Reger.—I want to add that since this paper was prepared a well is now being drilled at the location recommended on the last page. It was not, however, done at my instance; it was done at the instance of Dr. White, the State Geologist of West Virginia, who has been trying for several years to induce the large oil and gas companies to make a test of some of these deep sands in West Virginia and a well is now just being started in Wood County, right at this immediate locality, and doubtless in a year or so we will have a test in some of these deep sands probably to the Trenton limestone.

Well No. 33 in the table of well records in the back of the booklet, "The Benson Well in Barbour County", which encountered gas at a depth of somewhere near 4,100 ft. in what is possibly the Kane sand of Pennsylvania, had a rock pressure of 1,625 lb. Now, if you take 4,100 ft. and multiply that by 0.42, the coefficient for hydrostatic pressure, you will find it comes so close to 1,625 lb. that it surprises you. I do not mean to start a discussion about hydrostatic pressure and rock pressure and what gas pressure really is, but I simply furnish this as a very interesting fact that has recently come to light in connection with this deep-well discussion.

M. M. Thompson, Morsemere, N. J.—About a year and a half ago I did considerable detailed work in the region around Charleston, W. Va.

I was at first surprised to learn how little dependence the operators place in surface geology, in this general region where our present oil-producing industry practically had its birth.

The State Geologist's reports are very thorough indeed, having been worked out by counties with excellent maps indicating surface geology and the results of drilling. The wells are carefully indicated and numbered in accordance with brief tables giving the essential facts concerning them.

A brief study of these geological maps demonstrates the important fact that in the region south of the Little Kanawha River, the developed oil and gas pools cut right across structural contours apparently regardless of any relation between oil and geology. In the northern part of the State where the folding is more pronounced, they follow more closely the direction of the axes of the folds, but usually along a line offset some distance from such axis.

These marked discrepancies are very largely the result of convergence. The actual contour of the oil sand is not parallel to that of the surface beds. The irregular thickening to the southeast modifies and often outweighs the effect of the folds determined from surface observation. Much more information than mere surface structure must be utilized in successfully applying geology to oil-field development here.

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Another unique feature of West Virginia is the fact that there are several important oil sands which do not carry any appreciable quantity of water. In regions of this character, where I have worked out the structural contours in developed pools from well records, I found that the oil pools are usually in the synclines. Not often in the trough of the syncline, however, because the porous portion of the sand frequently pinches or becomes "recemented" before it gets down to the lowest part of the trough. The pools seldom extend symmetrically onto both flanks of the fold.

The foregoing conditions are a few of the factors of uncertainty which must be considered by the geologist in West Virginia. Unfortunately, many important points connected with convergent series in fairly old formations cannot be determined very far in advance of drilling. Still, accurate intelligent geological investigations are a great help to the operator even in as difficult a region as this.

DAVID B. REGER.—Regarding the occurrence of oil and gas in West Virginia, in reference to structure, I might say that one thing that has doubtless confused Mr. Thompson is the fact that the sands of the Catskill red beds, in which a great many of our oil wells are found, are nearly all of them lenticular. You will very often find the sand not water-bearing, and lying well up against the slope. When you get the well records of the pool, you find that the sand pinched out completely going westward toward the syncline, so there could not be any oil in it. It got down to the bottom as far as it could, and then the oil pinched out in the shale. You normally find your oil directly along the syncline, because the sand is non-water-bearing and your oil goes down in there, and you must call it gravity because you cannot call it anything else. When you get up the slope in a sand that is regular and uniform you get gas.

The sand at the top of the Pocono is not always water-bearing; that is true of the basin immediately to the northwest of West Union, and there the oil occurs right along the axis of the syncline. You can trace it for miles and miles on the Doddridge County Geological Map and you will find the same thing true in a large proportion of the field maps.

We have a great deal more variable lithology in West Virginia than in Oklahoma; it must be that your sands are more regular there. Lithology has a bearing in West Virginia, no question about it, and we find it out after we drill these wells, but when we locate wildcat wells we usually put them on top of the anticline first to locate the gas and then go on down. If it is up in the Pocono or down in the Catskill we come down in the basin and hunt for it there without much fear of trouble. Of course we get a good many dry holes, but that is largely due to the lithological character of the sands. We figure that the anticlines and synclines are the great controlling feature of the oil fields of West Vir-

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ginia and that these numerous exceptions are simply caused by the accompanying lithological conditions.

If we had absolutely regular lithology then we would have regular oil fields and we could predict them with great certainty, but lithological structure is something that we do not know ahead of time and it takes a lot of drilling, usually, to bring it out.

L. L. HUTCHISON, Tulsa, Okla.—Is the series there conformable from the lower producing member up to the top?

DAVID B. REGER.—It is conformable except in local places where the sands are lenticular. We have no great local unconformity at the base of the Pocono; of course, there is always a non-conformity at the bottom of the Pottsville where the soft red beds of the Mauch Chunk come in. . I made this section for Marion and surrounding counties as typical of the West Virginia oil fields. The Pottsville measures are 300 ft. thick but gradually thicken southward until you get to Virginia where they are 4,000 ft. thick. The Mauch Chunk measures in some parts of West Virginia are not more than 50 ft. thick, composed of a few red shales and green sandstones, but in southern West Virginia, in Mercer County, thicken up to 3,500 or 4,000 ft. The Greenbrier Limestone also thickens from 50 ft. in the north to nearly 2,000 in the southern end, and you therefore have that great divergence in those three series going southward across the State.

South of the Great Kanawha River which runs approximately through the center of the State, the Catskill measures thin out completely and these lower shales here, the Chemung, Portage and Hamilton, at the top of the Devonian, thin out also so that when you get down to the Kentucky line, you have the lower Devonian and Silurian measures right up close against the Pocono. All these 4,000 ft. of measures in here have thinned out so that makes it possible to reach these sands in Kentucky.

This great barrier of shales, 3,500 ft. or more in thickness, that we are positive exists between the producing sands of West Virginia and the producing sands of Ohio, has kept the operators from drilling to the Ohio deep sands in West Virginia. There have been some attempts; there is a well now in the State of Pennsylvania, number 42 in that booklet, that is more than 7,200 ft. deep and it is still drilling. That well has recently been cased and I do not know how far down they intend to drill it. It is away down now in the Salina beds. Whether they will ever get down to the Trenton beds or not, I do not know.

L. L. HUTCHISON.—I have always worked on the theory that the anticline, to control accumulation outside of the formations, contained the three liquids, oil, gas and water. The series must be conformable. If we have an unconformable series, the structure you might interpret

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at the top does not necessarily interpret the structure below the conformity. You have not sufficient non-conformity so that would exist, have you?

DAVID B. REGER.—We have the convergence of the Pottsville and Mauch Chunk measures, which sometimes overrides the surface structure. I had an instance of that last year down in one of the southern counties. I examined some territory that had an apparent terrace structure, which looked favorable for oil. When I obtained enough well records to plot it on the top of the Big Lime I found, instead of a terrace structure, there was a syncline going right straight across the property and that spoiled our argument right at the start because it gave different conditions. Down in those counties south of the Great Kanawha, where this great thickening of the Pottsville has taken place, you frequently find the basin of oil sands is a mile or so away from the basin on top of the Coal or the anticline shown on the top of the Coal and that circumstance oftentimes explains the apparent shifting of the pools away from the axes.

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The Influence of the Movement in Shales on the Area of Oil Production

BY RICHARD A. CONKLING,* A. B., TULSA, OKLA.

(New York Meeting, February, 1917)

A SHALE layer, buried beneath two or three thousand feet of strata, in some instances, will upon folding become thicker in the synclines and thinner on top of the anticlines.

This can be accounted for, in part, by the stretching on the crests of the fold and the compressing in the troughs; but this will by no means account for all of it, as is shown in the example herein set forth.

It is my firm belief that the rest of the thickness is due to flow. What causes the movement, however, does not concern us here, so long as there is movement, for this article purposes to show its effect upon the producing area of different sands.

It is the author's hope, in this way, to drop a hint or two that may be valuable to the oil geologist, in making estimates of future productions and values of oil properties. An example will be given from property in the famous Cushing Pool in northeastern Oklahoma, where the author has had occasion to make a detailed study before recommending some property to his company, and has then been able to watch the results of this work.

The author never mapped the surface structure in this pool, as this had been done by Frank Buttram when he was with the Oklahoma Geological Survey. We are using, therefore, a section of his map in this article for our surface contours. The main east dip comes farther east than this section shows.

From the 10-ft. contours in Fig. 1, it will be seen that the surface folding is very slight, but in the contour map of the Bartlesville sand (Fig. 2), which was the greatest producing sand, we see much more complex folding. It seems that the folding becomes greater with the depth. Folding may have started in Middle Pennsylvanian times and continued through the Upper Pennsylvanian which is found at the surface. This may, or may not, have been the cause of the increase of folding with depth. However, the fact that mostly concerns us is the interval of shale between the Bartlesville sand and the Tucker sand. The Tucker is the next productive sand below the Bartlesville. Not enough wells have been drilled to the Tucker sand to make a contour map of it, but

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the figures beside the wells on Fig. 2 show the interval between the top of the Bartlesville and the top of the Tucker.

The entire area shown in Fig. 2, except the southeast corner, was productive in the Bartlesville sand. On the west edge of Section 10, a syncline will be observed. Water was encountered in this syncline at the bottom of the Bartlesville, but it did not rise high enough to ruin the wells for some time. The producing area of the Tucker sand is much smaller, however, as the surface of the sand dips more steeply

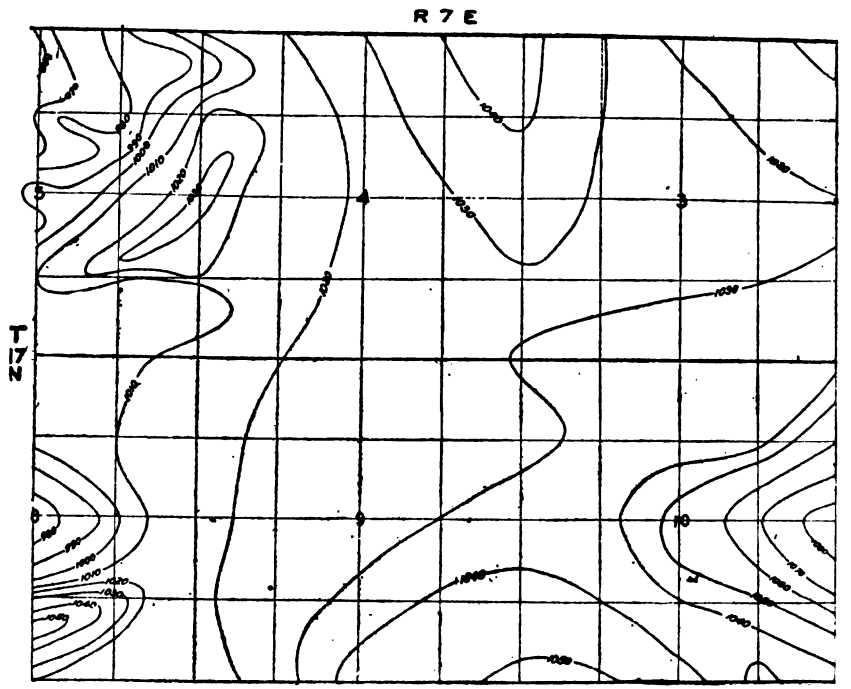

FIG. 1.—SECTION OF CUSHING FIELD. CONTOURS ON PAWHUSKA LIMESTONE.

and goes more rapidly into water. It will be seen from the figures on the north line of Sections 8 and 9, and from the cross-section (Fig. 3) taken along that line, that the Tucker sand dips almost twice as steeply as the Bartlesville sand. The latter dips 70 ft. from Well No. 12, Maley Yarhola, to Well No. 4 of the Gypsy Oil Co., ¼ mile east, while the former dips 123 ft. in the same distance. The difference is entirely due to the thickening of the shale interval between the two sands.

The producing area of the Tucker sand is, therefore, only a little over ¼ mile across. On top of this narrow anticline, however, was drilled the biggest producer of light oil known to the writer, namely, the No. 11 Jackson Barnett of the Gypsy Oil Co. of Oklahoma, which came in at 14,000 bbl. per day.

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After a few wells had been drilled to this sand, the exact limit of production was figured out, and our company is drilling accordingly.

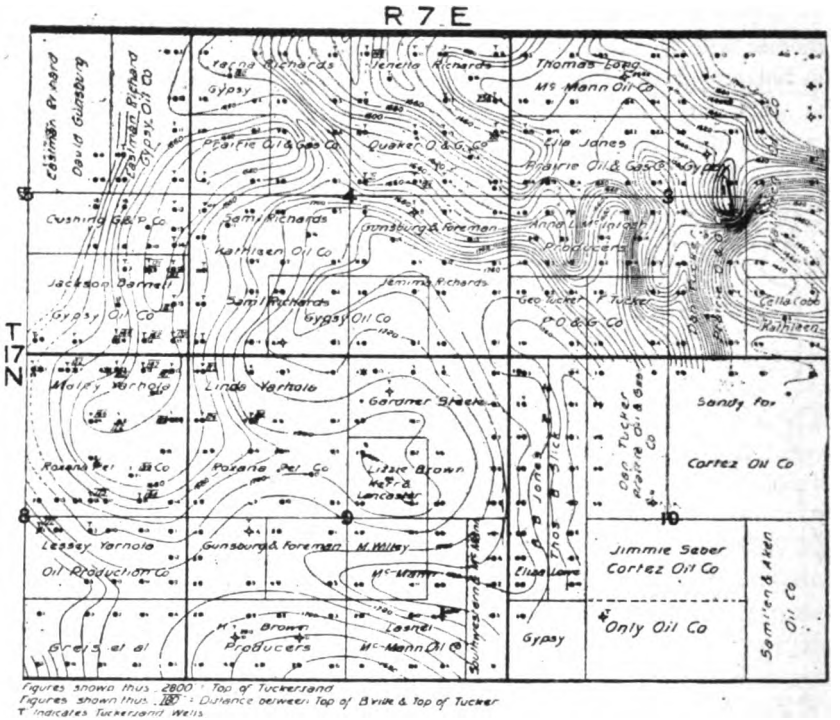

FIG. 2.—SECTION OF CUSHING FIELD. CONTOURS ON BARTLESVILLE SAND.

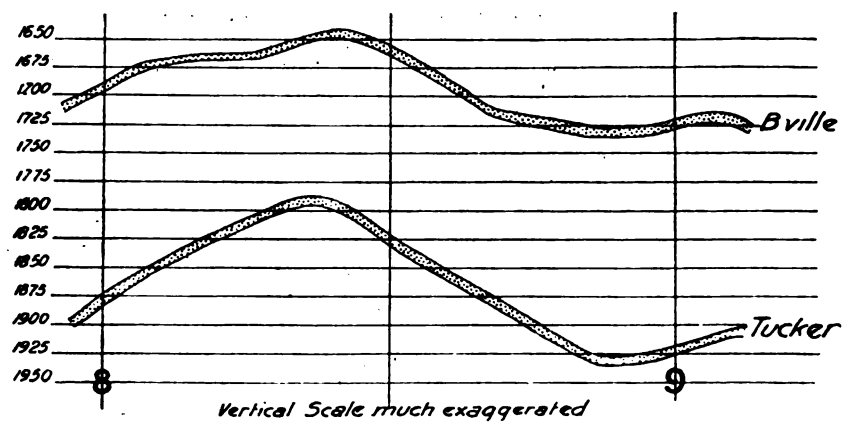

FIG. 3.—SECTION SHOWING TOP OF BARTLESVILLE AND TUCKER SAND.

By taking this thickening into account, millions of dollars were made for our company on purchasing of land.

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It will be seen that the same is true of the other domes on the maps. Of course these details cannot be told from the surface, but if one recognizes the conditions that these facts set forth, one need not go far astray when estimating the possible deeper-sand production in a developed field.

Discussion

D. W. Ohern, Oklahoma, Okla. (communication to the Secretary*). —The author states: "A shale layer buried beneath 2,000 or 3,000 ft. of strata, in some instances, will upon folding become thicker in the synclines and thinner on top of the anticlines." In any series of folded strata there is a level of no strain. Provided the strata do not slip on each other, there would be above this level a thickening of strata in the synclines and a thinning on the anticlines, as the author states, while the converse would obtain below this level. In order that the author's position be tenable, it would, in my judgment, be necessary to show that in the series of folded strata of the area under discussion the level of no strain was below the Bartlesville sand. In order even to approximate this level one should know both the thickness of strata removed by erosion since the time of folding and also the depth to which folding extends. Neither of these factors can be obtained.

There is considerable evidence in favor of the view that there is an unconformity below the Bartlesville sand in this region. Might it not therefore be that the variation in the thickness of the shale interval just below the Bartlesville sand is due to erosion? Personally, I am inclined to think this is a partial explanation in this particular case. On the other hand, the more rapid dip of the deeper strata seems to be the rule on structures in the Mid-Continent field. Mr. Conkling's observations on these sands are valuable, but one must wish for further data to substantiate the proposed explanation of their relations.

Dorsey Hager, Tulsa, Okla.—Recently we have been doing a little work along the same lines Mr. Conkling has outlined in his paper. I am not prepared to say that Mr. Conkling is not correct, but our results do not agree with his. I think it is a question of difference in correlation and interpretation of the well records. Our cross-section of the Cushing pool does not agree with that of Mr. Conkling and we find instead of the beds thickening in the syncline, there is a tendency to thicken on top of the anticline. We took the north dome of the Cushing field, and are now trying to get all the records. It will probably be another month before we have all the data complete, but our results at present do not agree with those of Mr. Conkling at all.

* Received Jan. 2, 1917.

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RICHARD A. CONKLING (communication to the Secretary*).—Mr. Hager says that his results on the correlation of well logs in the north Cushing field are the opposite to those of the author. Can he, then, explain why the structural dip of the formations increases with depth, as Dr. Ohern, in his criticism, sets forth that it does, and as all who have worked in these fields will admit, if the shales thicken on the crest of the anticline, as he says they do? We are willing to agree with Dr. Ohern that there is some evidence of a slight unconformity below the Bartlesville sand and think that we can prove, from a specimen in our office, that this unconformity comes at the base of the Bartlesville sand in the south Cushing pool.

On the Ingalls dome, in the southeast corner of Township 19 North, Range 4 East, however, we get results the same as those given by the author on Cushing. The Ballard well in the center of the Northeast Quarter of Section 33 is just 10 ft. higher, structurally, according to the surface contour, than the Ringold well in the center of the Southwest Quarter of Section 34. The dip in the Bartlesville sand, between these wells, is 40 ft. and the dip on the top of the Mississippi lime is 75 ft. This also might be accounted for, partly by the unconformity, but the author used the interval between the top of the Oswego lime, which is about 420 ft. above the Bartlesville sand, and the oil horizon found in the lime in the Ballard well—just mentioned above—to find the depth at which the same oil should be found in a well in Section 5, Township 22 North, Range 3 East, 25 miles North. Using this interval below the Oswego in the former well, we predicted within 2 ft. where they would find the oil in the latter well. Of course, we know that it was accidental that we missed it only by 2 ft., but if there were much of an unconformity in this interval it would be more of an accident if we came within 100 ft. of the depth. Since it can be proven from most of the structures in this section that the dip continues to increase downward, below the Bartlesville at least, to the Mississippi lime, it does not seem necessary to the author to try to prove otherwise that the depth at which the shales will tend to thicken on top of a fold, has not been reached above the Tucker sand, if, as Dr. Ohern says, there is a certain place where such will be the case.

* Received, Apr. 20, 1917.

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The Need and Advantages of a National Bureau of Well-Log Statistics

BY W. G. MATTESON,* HOUSTON, TEX.

(New York Meeting, February, 1917)

IN 1915, the State of California passed a law of great scope and importance. This law has been in successful operation for a year and may be briefly described as an act "establishing and creating a department of the State Mining Bureau for the protection of the natural resources of petroleum and gas from waste and destruction through improper operations in production; providing for the inspection of petroleum and gas wells; requiring all persons operating petroleum and gas wells to make certain reports" and providing the necessary officers and assessments to carry out the provisions of the act.

The statute provides for a separate department of the State Mining Bureau, under the general supervision of the State mineralogist, with a staff composed of a "state oil and gas supervisor" and four deputies.

It rules that all petroleum and natural gas wells shall be drilled, operated or abandoned under the supervision of the State supervisor, who shall order such tests or remedial work as seems to him necessary to protect the interests of property owners and the general public.

It also provides for a board of arbitration to consider orders given by the supervisor which are not acceptable to the well owner.

Section 18 of the act, which is the provision that is directly related to this paper, requires "the owner of any well referred to in this act to keep a careful and accurate log of the drilling of such well," and further requires that this log shall show the character and depth of the formations passed through or encountered in the drilling, particularly the location and depth of the water-bearing strata; the character of the water encountered (so far as ascertained); whether the water was shut off, and, if so,at what point; that it shall show completely the amounts, kinds and size of casing used; the depth at which oil-bearing strata are encountered; the depth and character of the strata, and whether all water was successfully and permanently shut off so as to prevent percolation or penetration into the oil-bearing strata.

This log is to be kept in the local office of the owner or operator, subject to the inspection of the supervisor or any of his deputies at any

* Geologist and Mining Engineer.

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time during business hours, and a copy is to be filed with the deputy supervisor immediately upon the completion of the well, or upon the completion of any additional work on the well.

The owner of any well drilled previously to the enactment of this act must furnish to the supervisor a complete and correct log of the well, so far as may be possible, together with a statement of the present condition of the well.

It is evident from the foregoing that the main object of the act is to extend the life and increase the productivity of oil fields by preventing the injudicious drilling of wells without taking the proper precautions to prevent encroachment of salt water in the oil- or gas-bearing sand, and to shut off overlying and underlying water sands which might flood the oil-bearing strata. In a field where many operators are drilling quickly to obtain first production, the necessity of taking adequate measures against the encroachment and infiltration of water is fully understood. Yet the act, as outlined, is capable of even much broader application. The ideas, included or suggested, are so pertinent and important that similar statutes in all States having oil-producing properties or prospects should be strongly urged. For this and other reasons which follow, the enactment of a Federal law, similar in design to the California statute and supplemented by the establishment of a National Bureau of Well-log Statistics, seems advisable.

No one realizes the urgent need for such a law more than the geologist, who, in his frequent examination of territory where outcrops are scarce, scattered or nil, often discovers that logs of wells, drilled several years previous, were not taken, or are not available, or that such logs as are obtainable are almost useless because of the careless and unsystematic manner in which they were recorded. Hundreds of such monuments to inefficiency may be found in our oil-producing States today.

The problem, moreover, bears a close relationship to the theme of conservation. Various experts have calculated that with current consumption and production, our present oil fields will be exhausted in from 25 to 50 years. If America is to maintain her industrial supremacy, this exhaustion of our present oil supply must be offset by the discovery and development of new fields. The larger companies have foreseen this contingency, and, acting on their convictions, are organizing efficient geological corps and spending hundreds of thousands of dollars annually in an attempt to open up new territory. From an economic and industrial viewpoint, their efforts demand the strongest coördination on the part of State and Federal government. A good percentage of the territory now under private investigation has been marked by the work of the wildcatter, and were accurate logs of such test wells at hand, probably much of such territory would be condemned at once, permitting the time, labor and money to be devoted to the investigation of

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some other region. Conservation of private capital devoted to the exploitation of natural resources must be given serious consideration as well as the conservation of the natural resources in question.

Since our recent progress in drilling methods and our increased understanding of the many peculiarities of oil and gas horizons, the geologist can no longer be satisfied with the statement that any wildcat test was a dry hole. He must ascertain which of the many factors might account for the dry hole in question and often only careful examinations of well logs will convey this information. In some districts where outcrops are few and scattered or covered by soil and detritus, the geologist is often forced to rely entirely on well logs in working out structural relationships. The Gulf Coast Tertiary fields, characterized largely by palustrine and marine deposits of cross-bedded, unconsolidated, often unstratified sands, clays, sandy clays and clayey sands, furnish an excellent illustration. Hundreds of wildcat wells are being drilled today by independent operators in Texas, Louisiana and Mississippi. In many instances well logs constitute practically the only data by which the structure of such virgin territory can be worked out, yet such data are either neglected or taken in such an inconsistent manner as to be of little value. It was during the examination of such territory that the potential possibilities of some such law and organization as is suggested in this article was forced upon the attention of the writer.

Although a State has taken the initiative in this important matter there are many reasons which favor national legislation and the establishment of such a bureau as part of the government organization, preferably as an adjunct to the U. S. Geological Survey. Doubtless there will be a conflict of opinion in this regard, but it is evident that State legislation is contingent on too many probabilities such as political favor, lack of funds, etc. State legislation is apt to be vacillating, delayed, and lack uniformity. A national law would insure standard methods of procedure, would become operative in all States simultaneously, and the question of funds should not prove a serious handicap. The U. S. Geological Survey possesses a statistical department unsurpassed in its ability to handle such work, while government geologists could render a much-needed service by devising a standard form and method of taking and recording logs. The importance of uniformity can not be over-emphasized.

The information accruing from such a bureau is as vital and valuable to the government as to the oil producer, since well logs are generally sought by the government geologists wherever possible in working out the structure of the various quadrangles comprised in the geologic folios being annually issued, and often such logs constitute the only positive information obtainable. All these considerations are strong arguments for government supervision and legislation and the importance of obtaining such data in intelligent form becomes more evident.

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Of course, it might be objected by some operators that logs which were obtained by private investment are valuable to the original investors and should not be open to public scrutiny. Such an objection is often legitimate, but there are many ways of overcoming it, such as specifying that the logs will not be available for public inspection so long as the operator in question holds valuable options on said property.

Obviously the details of such a project as the establishment of a National Bureau of Well-log Statistics must command careful thought and development. The California law suggests an outline into which may be incorporated numerous, far-reaching provisions. Doubtless the California statute will be altered from time to time as experience gained from operation points to the advisability of new amendments. The author has merely attempted to give some definiteness to an idea which he believes to be sound, practical and worthy of serious consideration. He trusts that it may promote such discussion and suggestion as will eventually result in some constructive action embodying the principle which has been defined.

Discussion

Benjamin L. Miller, So. Bethlehem, Pa.—In discussing the question personally with Mr. Matteson I have found that he simply proposed this as a suggestion and he recognizes, as well as a great many other oil geologists and oil operators, that to put into practice such a suggestion would mean the overcoming of many difficulties. Yet perhaps it may be possible to overcome the various objections that may be offered.

Undoubtedly one of the most serious objections is that of incomplete and inaccurate well records. Faulty records are worse than no records at all. Perhaps, however, definite instructions and insistence on their observation may remedy this defect after a time.

The Chairman (M. L. Requa, San Francisco, Cal.).—This subject is a very important one, but just how far the National Government can go in taking action is a question. It involves, of course, the question of State rights. California, I think, has done very well up to date and probably will do better in the future. The law that was passed at the session of the Legislature 2 years ago is working as satisfactorily as anything of that kind can work with so little experience to guide the framers of the statute.

We recognize in California the vital necessity of conserving the oil of the State. We have come to the conclusion that there is not as much oil in California as we thought there was. In the month of December the State drew on its oil stocks to the extent of 1,800,000 bbl. and I think that the January statistics will show probably as much more. There is more or less of a panic at the present time among the large marketing concerns regarding the question as to where they are going to get their oil for future requirements.

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The reversal of form within 18 months has been startling. The whole viewpoint of the large marketing companies has been changed. The Standard Oil Co. has been very busily engaged in acquiring property; the Associated Oil Co. spent upward of a million dollars last year in drilling without appreciably raising its production. The Union Oil Co. that has large contracts in Chile has recently made an arrangement with the Mexican Petroleum Co. to supply practically all if not all of that Chilean business from Mexico, and we of California at the present time are very much awake to the necessity of conserving the oil resources, and property, wherever we can. Proper well locations, proper supervision as to the shutting off of water and plugging of wells that are making water, we believe are a very essential part of the program along with proper well logs.

We have also come to the conclusion that that work cannot be left to the individual producer. It is a curious thing, but no producer is ever willing to admit it is his well that is causing trouble, it is always his neighbors'. Under the existing law, I think the producers of California are well satisfied that a step in advance has been made, and I am sure they will never go backward; they will make the law, if anything, more stringent for future operation, but as to just how far the National Government will enter into that field is a question that seems to me to be very debatable.

I. N. KNAPP, Ardmore, Pa.—It is very true that the average oil operator and driller have not fully appreciated the importance of keeping accurate well logs in the past, but they are all getting educated up to the point of being appreciative.

One is obliged to depend on the coöperation of the oil operator, the contractor and the driller to get accurate well logs and samples. Their hearty coöperation can be had to sustain reasonable laws and regulations as enacted. Impractical laws and regulations soon become dead letters, and are worse than none.

DORSEY HAGER, Tulsa, Okla.—I know good well logs are kept in California. Oklahoma is doing its best now to get good well records. A campaign has been going on, the companies are demanding good records from their men and it is really surprising to note how excellent the records are now, compared to what they were a few years ago. I think it will be a matter of just another year or so before Oklahoma and Kansas will take care of that well-record question themselves without any national legislation. By the time national legislation is put through, the States will have accomplished it themselves.

D. B. REGER, Morgantown, W. Va.—We have had a great deal of work from the oil companies in getting well records in West Virginia. I suppose we have copied maybe 5,000 or more records from various

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companies' offices and so on, where we could get them. We do not have much trouble now with the large companies; they usually have their records in good shape and they usually give them to us willingly because they realize the value that their preservation and use in making up the structure maps have for their companies. Our main trouble is with the wild-catters, who often have inexperienced drillers who do not keep the right kind of records. We have often wished, in our State work, that we had some way to get these records without fail. We have always regarded compulsory legislation as hopeless because so many of the operators think that it is an infringement on their rights.

I found an amusing circumstance 2 or 3 years ago. Some men who had been working in a small way, independently, in one of the counties at which we were at work, refused to let us have any of their records. They did so abruptly because they said they had spent too much money drilling these wells and getting this information to give it to anybody. We let the matter rest, for about a year, and sent this particular operator a copy of one of our reports on a county adjoining his own. He went in there, speculated on the subject matter of the reports and made a lot of money that year. The next year when I went back to his office he turned over all his record books to me, left his office and told me to help myself. We usually find that a little work of that sort brings the desired result.

Of course the wells drilled where no records are kept are gone, and cannot be had. Such a provision as suggested in the paper before us would have a good result, and would of course remedy that.

R. H. JOHNSON, Pittsburgh, Pa.—It seems to me that there are some wells that are so important, for instance, the first test in some foreign concession, that a company is making a great mistake when it relies upon the drillers merely for the log. In a number of such cases I believe an observer should be maintained at the well for the purpose of keeping the log. But of course there is a limit to the possibility of this method and in general we are compelled to fall back, as has been said, on education. I think we ought not to be cynical about well logs. They are so vitally important we must keep at this educational process until drillers keep good logs. The trouble is, of course, the attitude toward this work of many drillers and contractors. It seems to me the executives of the company ought to take the attitude, "If these men are not going to do it we will get somebody that will."

S. A. TAYLOR, Pittsburgh, Pa.—Before leaving the question of records of wells, I think there is one item that it might be well to have brought out. In Pennsylvania, Ohio and West Virginia, and some of the States that are mining coal, they have already passed laws, at least Pennsylvania has, in connection with the mining of coal requiring that the wells

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shall have kept a very accurate log and if the well is abandoned it must be plugged below the lowest seam of coal. At the same time an accurate location of the well is determined for the reason that if it is abandoned they must know the location of it in order to protect the mining of coal. I, too, realize that this matter comes back to State's Rights and is governed and controlled by the police powers of the State. Consequently each State must pass its own laws, but I am sure those States engaged in the mining of coal or other minerals ought to have an exact location of all wells and complete logs of them.

Another matter of great importance to oil and gas companies, and which has come up, especially, in the State of Pennsylvania and also in West Virginia, is the amount of coal or mineral that is to be left around a well in order to protect it when operating or mining the materials surrounding it. Up to the present time I think the amount that has been decided to be left is very largely guesswork. In some places they leave a certain stipulated amount and a short distance away a larger or smaller amount. Even in different counties in the State of Pennsylvania they require a different amount. This has been decided largely, or rather arrived at, by some petty court decision or agreement and no method really scientific has been applied to this scheme. I think that by keeping an exact record of the overlying strata it will be possible to determine how much coal or other mineral should be left around a well in order to protect it and make it safe for the mining of whatever material is to be mined, largely coal on account of the gas leakage into the mines, etc. On the whole, I am sure that a careful record of wells will be an advantage not only to you as operators or drillers of oil and gas wells but to the entire mining fraternity.

C. NARAMORE, Washington, D. C.—I have been on both sides; I have been a geologist for one of the larger companies, and have been asked to scout around and get well data. Later I have had charge of the work and had the privilege of telling the drillers what I wanted and at first getting the same treatment that the speaker before me did. I find that if you try to drive a driller you lose out; it is a question of team work. You can educate a driller; he is better than the average worker. If you will let the average driller know that you are very anxious for that record he will give you a record which is valuable.

The use of the 5-ft. stick covers only a small interval of the well log because ever so often, if so requested, the driller will string in and check the measurements carefully. I have had to hire many drillers and only had occasion to discharge three or four for absolutely falsifying the logs, after I have worked with them a while.

The driller who is not familiar with log recording is usually opposed to it at first.

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In the matter of States collecting logs, I was in the California field when that law was put into effect. As Mr. Requa said, the records in California are accurate, but I am sorry to announce that from now on, they will not be as accurate because of rotary drilling. Where we used to take from a year to a year and a half to drill those deep wells with standard tools, 3,000-ft. holes are now put down in from 30 to 35 days and drilled by contract. On different occasions I have asked for a well record from a contractor and from his reply knew that he had failed to keep a detailed log. In fact, I have received copies of logs of the same well at different times and found that no two were alike. Other contractors keep splendid records. That is leading to very serious practice. On one of the largest and most productive anticlines in California, a large company was drilling wells with the rotary method in 1911 and 1912. The man in charge had his idea where the oil should be found. They drilled through the big oil sand at night, went 200 ft. below and found no oil. Ever since that day that well has been considered as limiting the possible productive oil land on that limb of the anticline. Just a year ago now the superintendent desired to make a producer of this well or to salvage the casing—as casing is at a premium on the coast now.

His engineer had constructed a very accurate peg model, on which it appeared that the oil sands in this well had been passed through and cased off.

Here is a case where practical geology meant something to that company.

With our theoretical point of view he began to abandon this well 50 ft. at a time. One hundred and fifty feet up the hole he found a little trace of oil. After baling a number of days, he then proceeded to pump the well, and for a week nothing but rotary mud appeared. To make a long story short, he brought in a 400-bbl. well of 30 gravity oil.

It meant that the limit of the productive territory near this well had to be extended, so the keeping of well logs is all that we contend. A poorly kept log is better than no log and allows for a partial correlation. If the records are approximately perfect, when you come to project them on a scale of 100 ft. to the inch, they furnish an approximation which is often of much value.

I am sorry to learn that the keeping of logs is handled so carelessly in some of the large producing fields. I may say that in the State of California we have now some sort of a log for nearly every well and the record of enough wells so that we can project almost anywhere within a proven area.

CHAIRMAN REQUA.—How are you going to correct that question of the rotary drilling?

C. NARAMORE.—It is an economic difference; a difference of cost. No

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correction is possible. Where the rotary method is the more efficient, it will be used. The same procedure is in order as suggested for cable tools, the coöperation of the driller must be enlisted to furnish us the best record obtainable.

Until recent years, each company drilled its own wells, now many companies have their wells drilled by contract. Some operators are careful to secure a dependable log from the contractor, others are careless in the matter—but with the State Mining Bureau regulating such matters, the State of California is in a better position in this regard than many other States.

I. N. KNAPP.—If you wish to get an accurate log with samples of an unconsolidated formation at any particular horizon in drilling with the rotary, it is necessary to take a core. It is a pretty hard proposition to do this, but it can be done. Arthur Knapp and myself working together finally hit on a design of bit and core barrel with which a core could be taken. The cores we finally secured revealed that we had been fooling ourselves as to what the formations actually were when we were guided by the cuttings from the overflow only. I have many times saved samples from wild-cat wells, also washed out and preserved fossil shells, but the geologists examining the same never agreed on an interpretation of the record.

Geology is not an exact science, and when experts cannot make demonstrations that come somewhat near agreeing, what is the use of expecting that the layman can correctly classify wild-cat formations for record?

The A. T. elevation of the well, the lengths of pipe or casing used, and the depths, can be made matters of record.

Drilling in a dry hole with the cable tools, sudden changes as from a soft shale to limestone can be easily noted. In a great many cases one formation shades off into another and the point of change is guess work, particularly in rotary drilling. Naturally the operator makes measurements necessary for his protection as, for instance, the lengths of casing used and the top of the oil or gas sands.

CHAIRMAN REQUA.—There is no doubt that the rotary drilling in California is very fast superseding the old standard tools. It is purely an economic question and there is so much in favor of rotary, that broadly speaking I think the standard really is practically obsolete for all deep-well work. It is unfortunate that we are not going to get the well records that we have had in the past.

ARTHUR KNAPP, Ardmore, Pa. (communication to the Secretary*).—The author has failed to consider the point which in my mind is the most convincing in advocating some sort of a bureau for the collection

* Received Mar. 12, 1917.

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of well logs. I venture to say that 90 per cent. of the logs as taken today are absolutely worthless to any one but the one who has taken the log. This is largely due to the careless use of nomenclature. The author says on page 883, "He must ascertain which of the many factors might account for the dry hole in question and often only careful examination of well logs will convey this information." I believe that even a most careful examination of the average log reveals nothing.

I have at hand a book on petroleum, by a well-known author. This book has a number of logs from fields all over the world, from which I shall illustrate. In a well log from Kansas I find "Soft gray shale" and in one from Humble I find "Hard blue shale." As a matter of fact the shale from Humble is softer than the shale from the Kansas well. The difference is that the drillers in Kansas, being used to limestone and sandstone, call the shale soft because it is soft in comparison to most of the formations that they drill. On the other hand, the shale encountered in the Humble well was about the hardest formation encountered, and was called hard.

Again, I find such things as "mixed rock and sand." I am familiar with the formation in the field from which this log was taken, but I am at a loss to explain what is meant. In one log I find "shell formation" and in the next "4-ft. shell." I know that in the first case the reference is to a formation having shells in it and that in the second case a "shell" is a hard layer. However, one not acquainted with both fields might be misled.

What does "quicksand" mean when found at a depth of 870 ft.? How coarse is a "coarse gravel?" What is the difference between "sand rock" and "sandstone?" What is "pulverized shale?" What is "shale" anyway? I find one log that has in it "laminated shale."

One of the greatest benefits to be derived from the establishment of any bureau would be the necessary standardization of nomenclature.

Also, standardization should be extended to the method of taking samples and determining their properties. The color of a sample is rarely the same after it has been dried as when it was fresh from the well. Even rewetting does not always produce the same color. Many samples disintegrate after a short exposure to the air. Either the laboratory or the field determination must be taken every time. In general, it is impossible to compare a sample from the laboratory with one fresh from the well.

W. G. MATTESON (communication to the Secretary*).—The criticism and question raised by Arthur Knapp regarding the standardization of nomenclature is most important. Faulty rock classification is generally the result of carelessness or ignorance on the part of the driller. Two

* Received Apr. 23, 1917.

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remedies might be suggested. In a field where several large companies are operating on an extensive scale, a geologist from each concern might be assigned to superintend the gathering of well-log data and the classification of samples. The geologists from the several operating companies might then combine their data, which would doubtless give valuable information to all interested parties. At present, coördination is not a big factor, unfortunately, but the time will come when conservation will demand such systematic coördination on the part of competing companies. The second remedy lies in instructing and educating the driller to a thorough understanding of the importance of an accurate log and correct classification of the various formations.

The writer does not agree with the statement that "90 per cent. of the logs as taken today are absolutely worthless to any one but the one who has taken the log." While mistakes in nomenclature often occur, the driller is generally able to classify the important water-, oil-, and gas-bearing formations with a fair degree of accuracy. Such information is often sufficient to permit accurate correlation and also constitutes valuable data for projection purposes. It must be borne in mind that to attempt to use well-log data in a strict scientific sense of accuracy is impracticable and impossible. A broad interpretation and a comprehensive knowledge of the practical limits of such information are essential in applying it successfully.

Even experienced geologists do not always agree on rock classification. The question as to what constitutes correct nomenclature should receive the earnest and immediate attention of geologists, operators, and producers. Although standard textbooks on geology give correct definitions and descriptions of the general rock species, such textbooks fail to develop the subject in sufficient detail for the work under consideration. A committee composed of appointed members from the American Institute of Mining Engineers, the Geological Society of America, and the U. S. Geological Survey could render invaluable service to the oil industry by addressing themselves to the task of providing an accurate, comprehensive system of nomenclature for general adoption.

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The Magnetic Concentration of Low-grade Iron Ores

BY S. NORTON,* CARBONDALE, PA., AND S. LE FEVRE,† FOREST GLEN, N. Y.

(New York Meeting, February, 1917)

In the West, capitalists have expended many millions of dollars developing the low-grade porphyry ores of copper. Half a dozen of these great enterprises have proved to be wonderful commercial successes. They have demanded improved crushing and concentrating machinery and consequently it has been developed. Many improved methods, cheap power, superior business organization, all these have contributed to this success, but the main feature is the handling of the material in enormous quantities, on a manufacturing scale. The mining chance of "striking it rich" has been eliminated by the manufacturing certainty of handling large quantities of material of known value, which while of relatively low grade, is available in large tonnages, assuring a supply for many years' run of the mill. Then the returns on the money invested are sure.

The concentration of low-grade magnetic iron ores, separating the magnetite crystals from the gangue by the use of magnets, is a field of work in which the lessons taught by the development of the porphyry coppers can be studied to advantage. Large-scale operations, and the liberal expenditure of enough money at the start to insure the most economical operations, are the means of securing the desired results.

The problem is to utilize millions of tons, and we may safely say billions of tons, of now worthless iron-bearing rock and to produce from it 10,000,000 to 20,000,000 tons per year of high-grade ore carrying 60 per cent. iron or higher; to take the lean material as found in nature, varying widely in iron content, and bring it up to a uniform standard of shipping ore. At present these ores are mined carrying from 25 to 50 per cent. iron, and the shipping product is brought up to 60 or 65 per cent. Fe. If future economies of operation make it possible to extend this process so that 15 per cent. iron in the crude ore can be treated as a commercial success, the additional tonnage available will be enormous. A 15 per cent. Fe crude ore raised to 60 per cent. Fe concentrate with 5 per cent.

* Consulting Engineer.

† Consulting Mining Engineer.

Mr. Norton and Mr. Le Fevre were associated in the development of the plants at Mineville, N. Y.

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Fe loss in tailings would require 5.5 tons of crude for 1 ton of concentrates. The cost of crushing and concentration can be brought down to 12 c. per ton crude or possibly to 10 c., and the cost of quarrying on a large scale, probably 40 c., would be low enough to leave a profit even now. There are mountains of gabbro rock in the Adirondacks that will average 15 per cent. iron in the form of magnetite crystals of good size, say ⅛ to 1/16 in., but the concentrate would also carry some titanium.

Magnetic Iron-Ore Resources

A thorough examination of some of the iron-ore properties and the knowledge acquired by development of extensive underground workings makes it possible to make quite definite estimates of tonnage available in certain areas, which show very large reserves.

F. S. Witherbee in his paper read before the American Iron and Steel Institute last October gave an estimate of 1,100,000,000 tons of crude

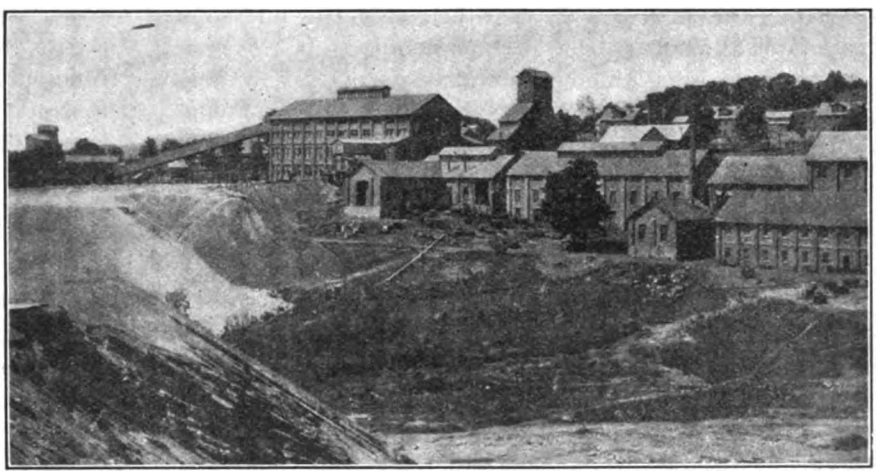

Fig. 1.—Outside of Mill No. 3.

magnetic ore above 30 per cent. Fe available for concentration in the Adirondack region alone, not including any titaniferous ores except the one deposit at Lake Sanford. He practically confined his estimate to the area of the iron-bearing gneisses which surround the central core of later eruptives, the anorthosites and gabbros, in which the titaniferous ores are found.

There are also in New Jersey and southeastern New York large areas that give conclusive evidence of vast amounts of non-titaniferous magnetites. The map accompanying the report of the State Geologist of New Jersey, year 1910, shows the area of iron-bearing gneiss rocks running northeast and southwest across the State about 18 miles wide by 50 miles

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long, from Phillipsburgh to Greenwood Lake. In this area are located by name 366 magnetite mines that have been worked more or less. There are also 24 limonite and 8 hematite mines. These lenses may easily be capable of producing an average of 1,000,000 tons each and there are probably double the number listed not opened up. Here we have 900 sq. miles of iron-bearing gneisses in New Jersey, or more than in the Adirondack region, with nearly as much more additional in southeastern New York, reaching from the New Jersey line across the Hudson at Fort Montgomery and extending to Brewster.

Mr. Witherbee's method of computation estimated 20 ft. thickness of ore over 10 per cent. of the surface area. He afterward cut the estimate in half to be conservative, which was equivalent to 10 ft. thickness of ore on one-tenth of the surface. This would give 2,700,000 tons per square mile or on 900 sq. miles in New Jersey 2,300,000,000 tons, with a goodly area in New York to fall back on to make up deficiencies.

A recent examination of a small area of 4 sq. miles near Sterling and Tuxedo Lakes in New York near the New Jersey line, showed ore reserves estimated at 5,000,000 tons per square mile.

Magnetic ore is found quite widely distributed, in Canada, Minnesota, California, New Mexico, New York, New Jersey, Pennsylvania, North and South Carolina, Tennessee. A detailed study of these deposits might be an interesting subject for the Bureau of Mines to follow up.

History of Development of Magnetic Separator

Some time in the year 1887 my[1] attention was called to the magnetic separation of ores. At that time Edison was experimenting with his deflecting magnet and the Wenstrom, a Swedish machine of the drum type, was in use. The Conkling machine, which was also on the market, was the forerunner of the modern belt machine, but the magnetic attraction came from a single magnetized plate.

My first experiment was with Port Henry old-bed ore, which I crushed to pass through ⅛ in. mesh, and then ran through an old-fashioned fanning-mill, such as are used on farms. I had better results than those obtained by Mr. Edison with his deflecting magnet. I then made a trial of the Conkling idea but found that the magnetic plate picked up a large part of the gangue with the ore, so that the ore had to be sized and fed very slowly to get good results. The same trouble was experienced with the Wenstrom machine.

I then made a small machine, substituting common horseshoe magnets or the magnetic plate of the Conkling machine. Since the magnets were

[1] References are to Mr. Norton and his experiments leading to the invention of the Ball-Norton separator.

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of north and south polarity, the ore turned end for end in moving from one pole to the next—not only the loops of ore and gangue but each individual piece turning. In this way the gangue was allowed to drop out, the ore was held, passed on to the next magnet, and so finally cleaned of the non-magnetic rock.

This use of magnets of alternating polarity is the fundamental principle of the Ball-Norton separator and greatly increases the capacity and efficiency of the machine.

However, as I was not an electrical engineer, I went to a friend, Clinton M. Ball, explained the operation of the machine, and told him that if he would make electromagnets of sufficient size and power, of alternating poles, I thought they would be a great improvement over anything previously used. Mr. Ball made the magnets, a small machine was built (shown in Fig. 2), and taken to the Benson mines, where about

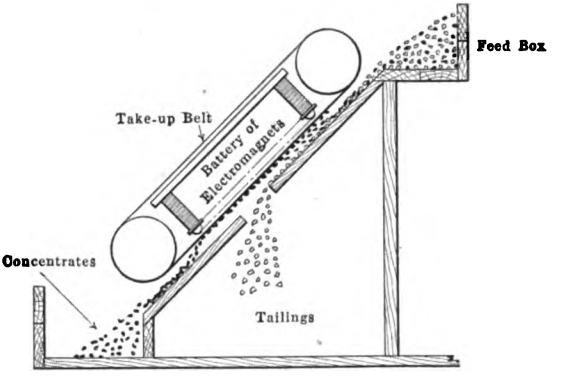

FIG. 2.—EXPERIMENTAL BALL-NORTON BELT SEPARATOR, 1888.

1,000 tons of 63 per cent. Fe ore was made, shipped to Braddock furnace and used there by James Gayley. This, I think, was the first use of magnetic iron-ore concentrates in a blast furnace.

The small machine was of the belt type. Mr. Ball soon after designed a drum-type machine, and later a double-drum machine in which a three-part separation was made. There are now magnetic machines of many types, but the majority use the alternating pole magnets.

Until 1908, I thought I was the first to use this form of magnet, but I then found that Z. Palmer had used the idea at Palmer Hill in 1854, so that it was not a new idea after all.

Mr. Palmer's machine is an interesting example of an early crude use of an important scientific principle. It was simple and primitive in the extreme, consisting primarily of a row of horseshoe magnets spiked around a log, like the spokes of a wheel. Finely crushed crude ore was allowed to slide through a wooden trough underneath the magnets, which were rotated by a crank attached to their supporting log. As the magnets

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rotated, they dipped into the trough, the good ore became attached to them and was lifted up. It was then transferred to another trough, set above, by employing the simple device of a broom wielded by a husky Irishman.

The number of so-called magnetic separators for which patents have been taken out has been so large that it would be a waste of time even to try to enumerate them. Many of them were mere toys and a number were mechanical monstrosities. The belt and drum machines of the Ball and Norton patent have accounted for 90 per cent. of magnetic concentration by the dry process; while the wet magnetic process has been entirely monopolized in this country by the Gröndal-type machine. There are no patents today controlling magnetic separation, and there is no longer any chance for any new or startling discoveries in this line.

The first magnetic separator that I constructed was of the belt type. It was operated with a feed belt running 125 ft. per minute, while the take-off belt ran 250 ft. per minute. I wished to make a careful test of the capabilities of the machine when working on an ideal material, so I prepared a special mixture for the purpose. This consisted of crushed white marble, washed and sized between $\frac{1}{8}$ and $\frac{1}{20}$-in. mesh, mixed with iron ore of the same size in a proportion of 2 parts marble to 1 part iron. It was evident that the particles of iron ore and marble would not be attached to each other, since the mixture was purely artificial. This mixture was then fed to the machine in a stream $\frac{1}{4}$ in. deep. The separation was almost perfect, giving an iron product over 99 per cent. pure. In this way, the possibility of a complete separation was conclusively demonstrated. In actual practice, however, such thorough preparation of material is impossible, and, owing to the difficulty of properly preparing the ore, there are some cases where separation cannot be made a commercial success.

Determination of Methods of Treatment

The magnetic iron ores found in different localities vary widely, not only in their iron content, but also in their physical structure. The ores from the various districts require, consequently, radically different treatment.

In the first place, bodies of ore differ widely in crystallography. For example, the ores of the Champlain Valley are more coarsely crystalline than the ores of New Jersey, the Benson mine, or the Cornwall ore bed. Obviously the mill treatment of these ores cannot be the same. Among other things, ore containing the coarser crystals would not require to be crushed to so fine a size as ore of the Cornwall type. It is very important to find the exact size at which any particular ore is most economically separated, and this size can easily be determined by experimental

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tests in a suitable laboratory. Moreover, the degree of fineness to which the ore must be crushed determines the process of separation to be employed. An ore which must be crushed to ⅛ in., 1/16 in. or lower will require the wet method of separation, while for larger sizes the dry method can be most profitably employed. The exact size that determines the method to be used is also somewhat dependent on the amount of moisture contained. Quite fine sizes can be separated if perfectly dry and fed in a thin film, but the dust problem is then somewhat difficult to deal with.

Present Practice, and State of Development

The largest development in the iron-ore industry, using magnetic concentration, is at the plants of Witherbee, Sherman & Co. at Mineville, N. Y., where about 1,200,000 tons of crude ore were mined and separated in 1916. The dry process of separation is used. The Chateaugay Ore & Iron Co., at Lyon Mountain, N. Y., the Empire Steel &

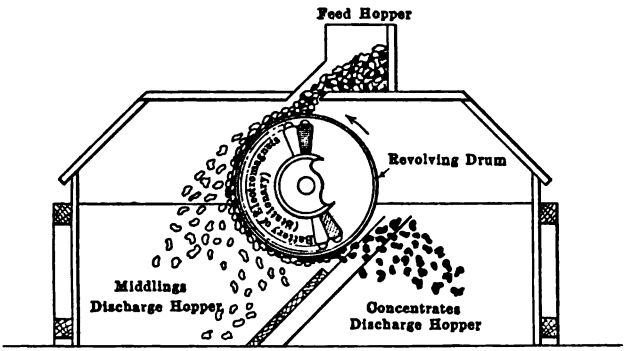

Fig. 3.—Ball-Norton Drum-Type Machine.

Iron Co. and the Ringwood Co. in New Jersey, also use the dry process successfully. The Gröndal wet separators have been recently installed at the Benson mines in New York. The largest development of the wet process in this country is on the Cornwall ore at Lebanon, Pa. This work is in charge of B. E. McKechnie, who is the highest authority on the wet process.

In the practical application of magnetic separation the most vital part is the preparation of the ore. It must be crushed so that the crystals of magnetite, or groups of crystals, are sufficiently freed from rock to bring the percentage of iron up to the standard set for shipping ore. On the other hand, it must not be crushed too fine, if it is possible to avoid it, otherwise the blast does not pass through readily in the furnace, or the ore blows over the top.

To maintain the best possible physical structure the separation must be made at each stage of the crushing.

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If the material going to the separators is sized, the strength of the magnets can be adjusted to pick up the ore of more nearly uniform quality, but a separation can be made without very close sizing.

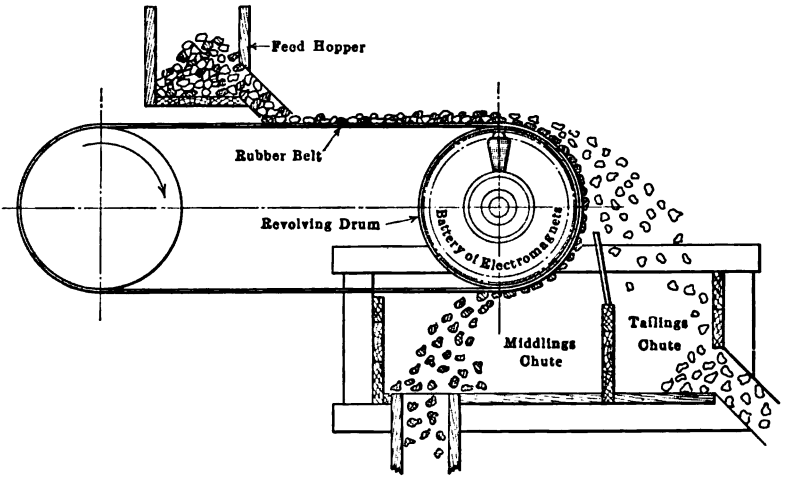

FIG. 4.—BALL-NORTON PULLEY MACHINE.

The drum type of machine (Fig. 3) has a section of fixed magnets inside of a revolving drum. It is used on the larger sizes where the richer

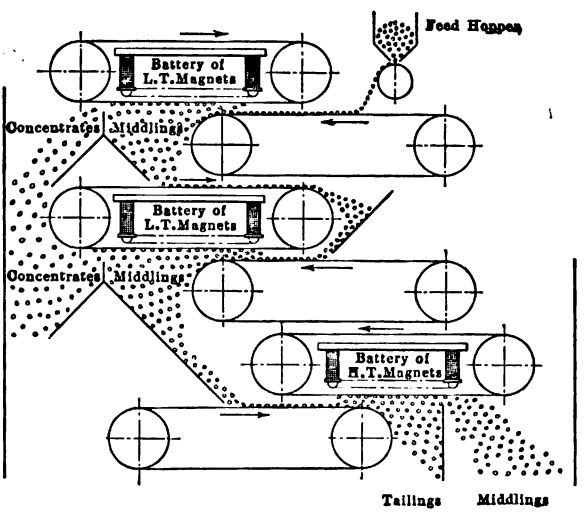

FIG. 5.—TRIPLE-DECK BALL-NORTON BELT MACHINE.

ores can be treated as coarse as 1½ in. and a considerable percentage recovered. This size is as large as is desirable for rapid working in the furnace.

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The pulley-type machine (Fig. 4) has a full circle of magnets which revolve with the drum. The magnets are wound to carry more current than the drum machine and will attract any lean ore, throwing off pure rock or tailings.

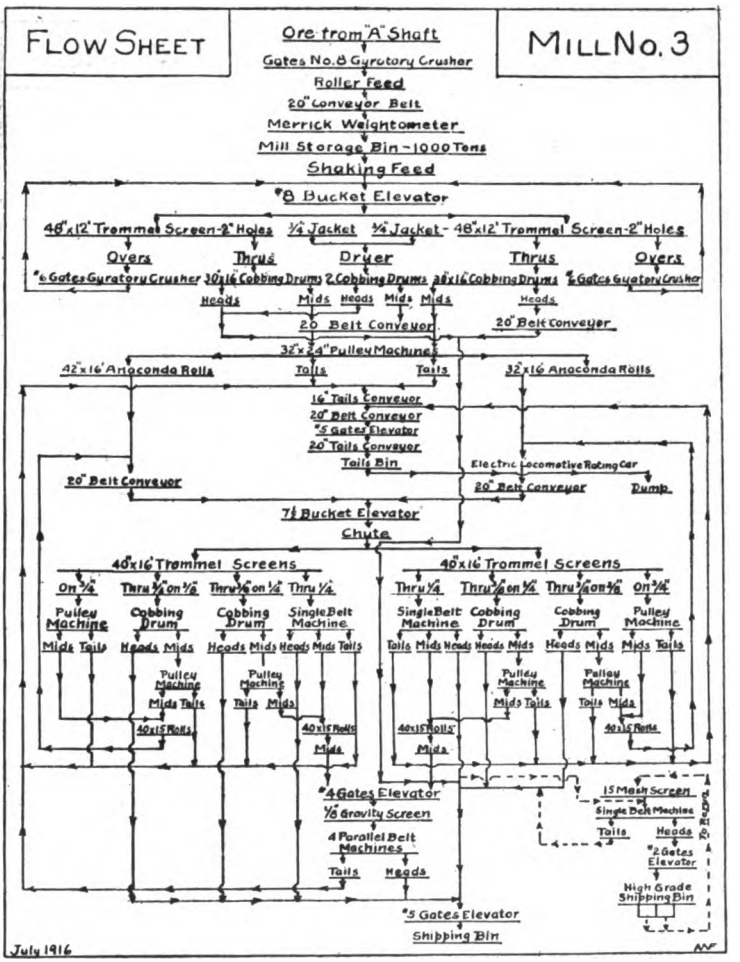

FIG. 6.

The drum and pulley machines will handle 30 to 50 tons per hour and are used together. The drum picks out any ore, as heads, rich enough for shipment. The pulley throws out rock lean enough to discard; what is left as middlings is crushed to about half its size and passed to machines treating finer sizes.

The belt-type machine (Fig. 5) is used when the ore is reduced to ¼ in. or below. The magnets are open to the air, so keep comparatively

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cool and are easily inspected. Since the magnets of the belt machine lift the ore from the feed belt, the gangue is less likely to be held in suspension and a cleaner concentrate is insured. In the triple-deck machine shown in Fig. 5 the two top machines make heads and the bottom one makes tailings, and middlings to be reground.

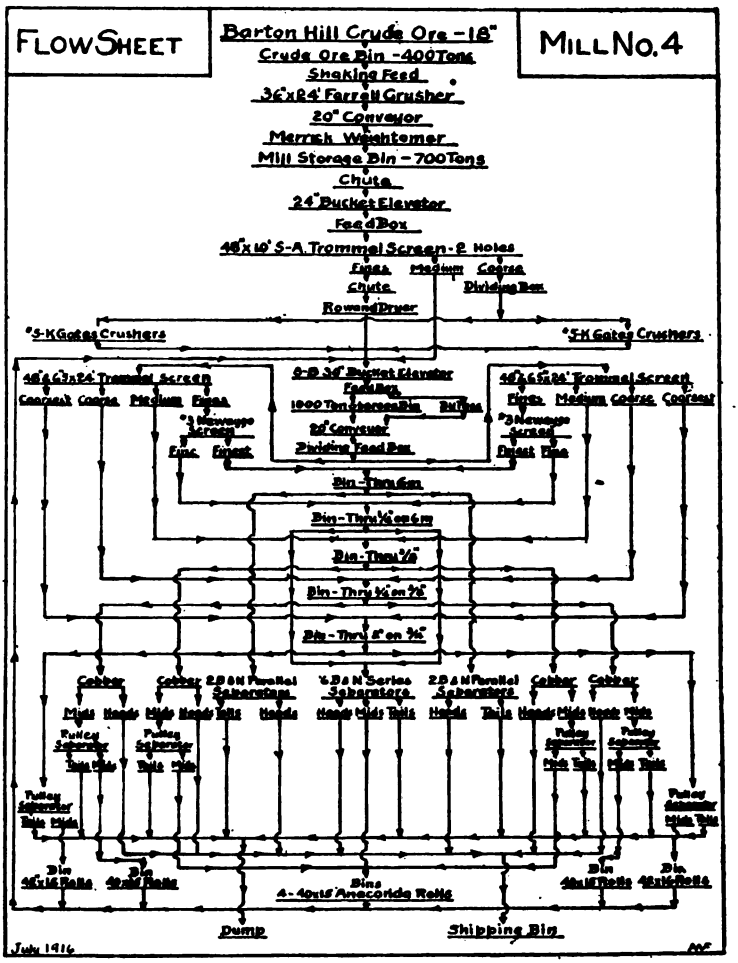

FIG. 7.

All these machines are used in the dry process.

If fine grinding is necessary to separate the crystals of magnetite from the gangue, wet separation is indicated. In this case treatment by sintering, or other processes, to agglomerate the ore is also required. The sintering process solves another difficulty by removing sulphur. Low iron and high sulphur content are handicaps which can now both be overcome by the combination of magnetic concentration and sintering.

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The accompanying flow sheets of mill No. 3 (Fig. 6), mill No. 4 (Fig. 7), and mill No. 5 (Fig. 8), of Witherbee, Sherman & Co. at Mineville, N. Y., show arrangements for treating three different ores. The richness of the ore determines at what size the first separation can be made.

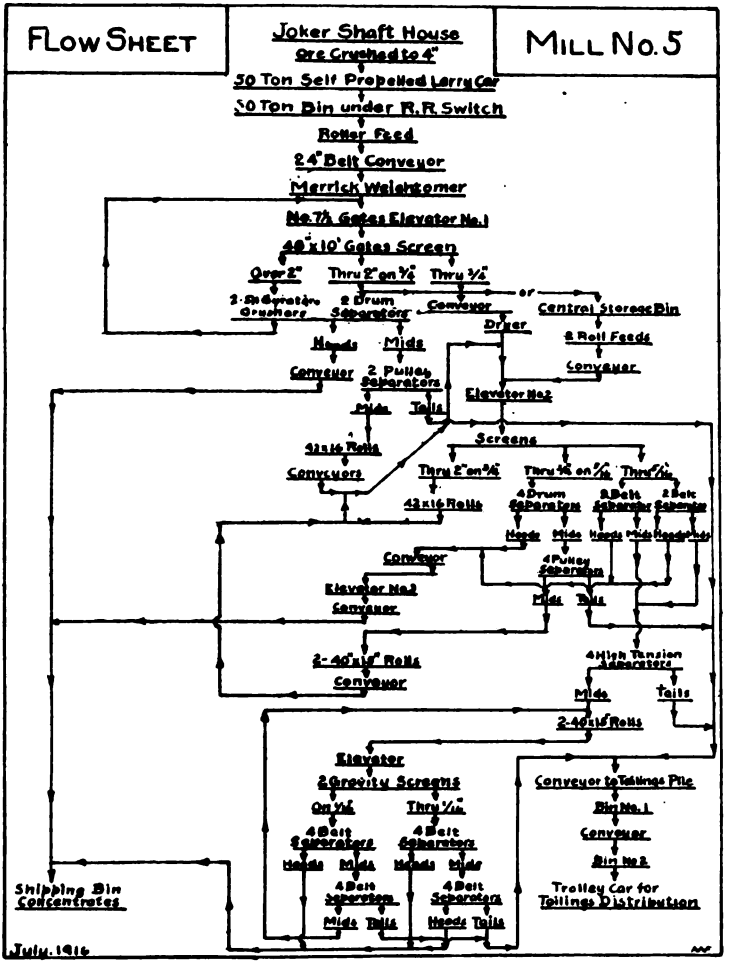

FIG. 8.

Mill No. 4 has eight sets of rolls instead of six in the other mills, because the ore is leaner and requires finer grinding.

WET MAGNETIC SEPARATION OF CORNWALL ORE[2]

During the dry magnetic separating tests on Cornwall ore, it became evident that this process of magnetic separation was not suitable for this ore, for the following reasons:

[2] Test conducted at Colebrook Furnaces, Lackawanna Iron and Steel Co, Lebanon, Pa. Described by B. E. McKechnie.

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1. *Dust.*—The ore must be very dry in order to secure freedom of motion between the particles, or poor separation will result. This condition allows the very fine particles to escape as dust. No system of fans or other arrangements for eliminating or controlling this dust has been developed which can be successfully operated at a cost not prohibitive on this ore.

2. *Low Grade of Concentrates.*—Owing to the tendency of the fine particles of talcy gangue to cling to the magnetic pieces, it was found impossible to raise the iron constant above 52 per cent. when separating the average grade of Cornwall ore. This fact is demonstrated by washing concentrates from the dry magnetic separation, when the iron content was easily raised from 52 to 58 per cent. This suggested using a combined process of dry magnetic separation and of washing the magnetic product in some such apparatus as the Dorr classifier.

The objections to this arrangement are:

1. Dust problem is not solved.

2. The combination of a dry and wet process has the disadvantage of both cost of drying and the cost of pumping water supply and handling of wet products.

The same or better results could probably be obtained by a wet magnetic separation. This process would eliminate the cost of drying, the dust problem, and should give a higher recovery of iron, due to the fact that a certain amount of iron would be lost in the slime from washing of dry concentrates. In the wet magnetic separation this washing is carried out in a strong magnetic field, which greatly reduces the loss from this cause.

In connection with the results obtained from the experimental wet magnetic separator constructed for investigating the wet process of magnetic separation of Cornwall ore, attention is called to the following points:

It is evident that in the separation of any ore by magnetic or other forces, the ore must be crushed sufficiently fine to free the valuable minerals from the gangue, and also that the degree of fineness required in the crushing depends upon the physical characteristics of the ore. As it is impractical to carry the crushing far enough to free all the mineral from the gangue, there will be a certain percentage of attached particles or "middlings" consisting of both mineral and gangue.

In the case of magnetic separation, these attached particles may go either as concentrates or tailings, depending on the strength of the magnetic field and the ratio by weight of magnetic to non-magnetic material in each. From this it follows that the stronger the magnetic field, the lower in iron will be both the concentrates and tailings product, due to a larger quantity of attached particles being attracted to the magnets. The reverse also holds true, that the lower the current, the higher in

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iron will be both the concentrates and tailings, as fewer attached particles will go to the concentrates and more to the tailings.

The richer the crude ore, the higher will be the grade of concentrates and the higher will be the iron content in the tailings. This is due to the fact that the rich ore carries a greater proportion of rich particles and a smaller proportion of rock. The grade of concentrates is raised, due to the smaller percentage of attached particles, while the percentage of iron in the tailings is greater, because of the smaller amount of clean rock present to balance the small quantity of magnetic material entering the tailings.

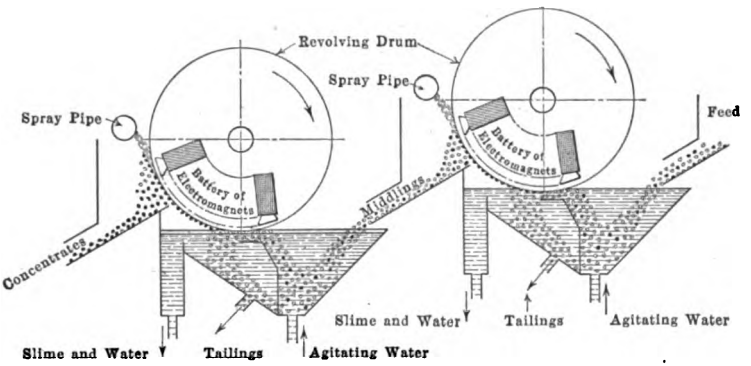

FIG. 9.—GRÖNDAL WET SEPARATOR.

Assuming that the amount of magnetic particles dropped by the separator is a nearly constant quantity, a higher percentage of recovery of iron is obtainable from a rich ore than from a leaner ore as the percentage of iron lost is evidently less.

The wet magnetic separator constructed for these experiments is a drum-type machine, constructed on the Ball-Norton principle. It consists of a number of stationary electromagnets, of alternate positive and negative polarity attached radially to a central shaft. About these magnets revolves a non-magnetic, water-tight drum, which carries a thin rubber belt.

In practice the magnets do not extend the entire circumference of the machine, but a gap is left between the points of feeding and delivery of concentrates. In this machine, which was built for experimental work, any desired number of magnets could be cut out by short-circuiting the current around them.

Six arrangements of the drum were experimented with. Of these only Arrangement 1 was successful.

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Arrangement 1.—The revolving drum drives the thin rubber belt which covers the face of the drum and passes over pulleys. Ore and water, or "pulp," are fed by a launder or feed sole in such a manner that the feed is thrown against the moving belt. The magnetic particles are held to the drum, while the non-magnetic material falls into the tank and is drawn off. As the magnetic material held against the belt passes through the water, the influence of the alternating polarity of the magnets is to cause the magnetic particles to take a rolling action, which allows any entrapped gangue to fall out. As the drum further revolves, the magnetic concentrates are lifted out of the water and carried up the belt and around the pulley, where they are washed off by a spray of water.

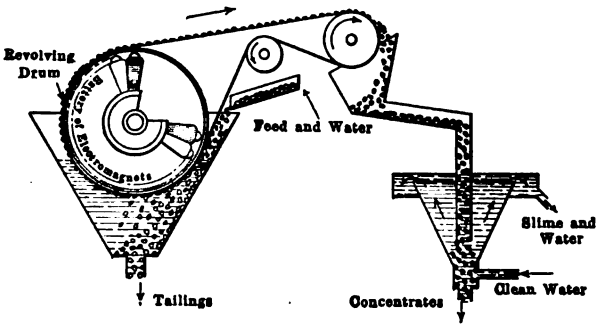

FIG. 10.—BALL-NORTON TYPE WET SEPARATOR.

In practice on Cornwall ore, it was found that a certain amount of very fine gangue was carried by the water into concentrates. They were, therefore, led to a classifier consisting of an inverted pyramid or tank, the bottom of which was fitted with a small hole and a connection above this hole for supplying clean water under slightly greater head than the depth of water in the tank. This water supply was regulated to furnish all the water required to supply the hole or the "spigot" and to furnish a slight raising current against which the heavy magnetic particles would fall but the very fine gangue could not, but would escape over the edge with surplus water.

Arrangement 2 was similar to 1 except that the water level in the tank was lowered until it was below the drum. This was done in an effort to reduce the amount of dirty water carried over the concentrates. The separator failed to make a separation operated in this manner, due to the fact that the surface tension of the water on the drum caused this water to act as a blanket, which did not allow the non-magnetic material to fall out.

In arrangements 3 and 4, the motion of the drum was reversed and the idler pulley removed. The feed sole was placed above to feed the pulp in the direction of travel of the belt. The tailings were to be removed at

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the tank and the concentrates, carried past the division board placed under the last magnet were to be removed by the spray of water. Due to the surface tension of the water, no separation took place above the water level. The separation accomplished beyond this point was destroyed by currents set up in the water by the rotation of the drum.

Arrangements 5 and 6 were similar to 3 and 4 except that the belt was removed and the material fed directly on the drum. The action was the same as in 3 and 4.

Following are results obtained from Arrangement 1.

Test No. 1.—This was a preliminary run to test arrangement of separator. The crude ore was a mixture of concentrates and tailings from a dry separation.

14-mesh	10 Amp.
Crude	No analyses.
Concentrates	54.80 per cent. total Fe.
Tails	13.70 per cent. total Fe.

Test No. 2.

20-mesh	12½ Amp.
Crude	44.10 per cent. total Fe.
Concentrates	59.00 per cent. total Fe.
Tails	11.90 per cent. total Fe.
	4.88 per cent. S, 1.14 per cent. Cu.

A series of determinations showed the iron in these tailings distributed approximately as follows:

4.24 per cent. combined with sulphur.
4.76 per cent. combined as silicates.
2.90 per cent. combined as magnetite.

It should be noted that 9 per cent. represents non-magnetic iron, or that about 75 per cent. of the iron occurring in this tailings sample is non-magnetic and cannot be charged to the inefficiency of the separation.

Test No. 3.

14-mesh	11 Amp.
Crude	41.10 per cent. total Fe.
Concentrates	51.80 per cent. total Fe.
Tails	12.50 per cent. total Fe, 2.61 per cent. as magnetite.

Crude should be crushed finer.

Test No. 4.

20-mesh	12.5 Amp.
Crude	41.10 per cent. total Fe.
Concentrates	52.50 per cent. total Fe.
Tails	10.10 per cent. total Fe, 1.74 per cent. Fe as magnetite.

Concentrates were again separated with a lower current of 10 amp.

Concentrates	56.60 per cent. total Fe, 54.23 per cent. Fe as magnetite.
Tails	28.95 per cent. total Fe, 20.49 per cent. Fe as magnetite.

The tailings from this separation are really a middling product consisting of attached particles.

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Test No. 5.—28-mesh, crude-ore sample divided into two parts, with 13.5 amp.

Crude.....35.20 per cent. total Fe, 30.30 per cent. Fe as magnetite.
Concentrates.....57.20 per cent. total Fe, 54.95 per cent. Fe as magnetite.
Tails.....12.50 per cent. total Fe, 2.18 per cent. Fe as magnetite.
4.28 per cent. sulphur.

With 10 Amp.

Crude.....35.20 per cent. total Fe, 30.30 per cent. Fe as magnetite.
Concentrates.....58.40 per cent. total Fe, 55.97 per cent. Fe as magnetite.
Tails.....13.30 per cent. total Fe, 1.02 per cent. Fe as magnetite,
5.70 per cent. S.

Test No. 6.

28-mesh 11 Amp.

Crude.....42.30 per cent. total Fe.
Concentrates.....58.90 per cent. total Fe, 57.28 per cent. Fe as magnetite.
Tails.....15.60 per cent. total Fe, 0.70 per cent. Fe as magnetite,
10.20 per cent. S.

Note high tails due to large amount of iron combined with sulphur.

Test No. 7.—20-mesh crude divided into three parts, with 12.75 amp.

Crude .....40.20 per cent. total Fe, 34.60 per cent. Fe as magnetite.
Concentrates.....56.70 per cent. total Fe, 55.68 per cent. Fe as magnetite.
Tails.....10.10 per cent. total Fe, 0.87 per cent. Fe as magnetite.

With 10.5 Amp.

Concentrates.....57.40 per cent. total Fe, 56.12 per cent. Fe as magnetite.
Tails.....10.30 per cent. total Fe, 1.31 per cent. Fe as magnetite.

With 8 Amp.

Concentrates.....57.40 per cent. total Fe, 56.30 per cent. Fe as magnetite.
Tails 17.00 per cent. total Fe, 8.70 per cent. Fe as magnetite.

Note that in using lower currents the iron content of the tailings increases more rapidly than in the concentrates.

A screen test on the crude ore sample above and on the concentrates from the separation with 12.75 amp. gave the results shown in Table 1.

TABLE 1.—*Screen Test on Crude and Concentrates*

Size	Per Cent. by Weight	Per Cent. Total Fe	Per Cent. Fe as Magnetite
Crude:			
On 28	13.04	37.00	30.74
On 35	15.19	38.30	31.61
On 48	13.27	39.50	33.93
On 65	9.86	42.80	39.73
On 100	8.05	46.30	42.05
Through 100	40.59	40.10	35.96
Concentrates:			
On 28	14.22	48.40	44.37
On 35	15.31	49.59	45.39
On 48	13.71	52.90	49.88
On 65	9.28	57.50	54.81
On 100	12.01	60.20	58.73
Through 100	34.70	63.40	62.78

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Note low grade of the coarse sizes of concentrates and that iron content does not reach 60 per cent. until 100-mesh size is reached. This shows the necessity for fine crushing.

Concentrates upon which the preceding screen tests were made were reseparated with 12 amp.

Concentrates.....57.90 per cent. total Fe, 56.55 per cent. magnetic Fe.

A screen test on this sample follows:

Screen	Per Cent. by Weight	Per Cent. Total Fe	Per Cent. Fe as Magnetite
On 28	14.77	48.30	44.50
On 35	16.32	52.00	48.72
On 48	14.01	55.60	52.93
On 65	12.50	59.70	58.87
On 100	11.59	62.90	61.77
Through 100	30.50	66.50	65.54

Note that the greatest gain in Fe content is in the fine sizes, due to washing out of fine gangue.

Test No. 9.

20-mesh. 11 Amp.

Crude ore was crushed in rolls four times to secure a large amount of fine material.

Screen Test on Crude

Screen	Per Cent., Weight
On 20	0.00
On 28	6.03
On 35	10.70
On 48	12.25
On 65	11.28
On 100	10.12
Through 100	49.61

Crude.....38.30 per cent. total Fe, 33.64 per cent. Fe as magnetite.
Concentrates.....58.40 per cent. total Fe, 55.97 per cent. Fe as magnetite.
Tails.....10.20 per cent. total Fe, 2.17 per cent. Fe as magnetite.

Test No. 10.—Crude ore crushed three times through rolls. Flakes screened out with 10-mesh sieve.

12.5 Amp.

Crude.....39.40 per cent. total Fe, 34.65 per cent. Fe as magnetite.
Concentrates.....56.60 per cent. total Fe, 54.09 per cent. Fe as magnetite.
Tails.....17.50 per cent. total Fe, 8.99 per cent. Fe as magnetite.

Speed of separator and feed were too great in this test.

20-mesh 11 Amp.

Crude.....37.50 per cent. total Fe, 31.90 per cent. Fe as magnetite.
Concentrates.....58.30 per cent. total Fe.
Tails.....13.70 per cent. total Fe, 5.51 per cent. Fe as magnetite.

Rate of feed 1,890 lb. per hour.

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Test No. 11.

10-mesh 12 Amp.

Crude.....37.50 per cent. total Fe, 31.90 per cent. Fe as magnetite.
Concentrates.....57.80 per cent. total Fe, 55.24 per cent. Fe as magnetite.
Tails.....9.60 per cent. total Fe, 1.30 per cent. Fe as magnetite.

Test No. 12.

12 Amp.

Crude.....38.80 per cent. total Fe.
Concentrates.....56.80 per cent. total Fe.
Tails.....8.80 per cent. total Fe, 1.16 per cent. Fe as magnetite, 2.58 per cent. S.

Test No. 13.

Weight of feed 139 lb.....12.5 Amp.
Crude ore three times through rolls.
Width feed 10 in.

Crude.....38.70 per cent. total Fe, 34.37 per cent. Fe as magnetite.
Concentrates.....57.30 per cent. total Fe, 54.23 per cent. Fe as magnetite.
Tails.....13.10 per cent. total Fe, 1.16 per cent. Fe as magnetite.

Sulphur in tailings..... 6.70 per cent.
Iron in tailings due to sulphur..... 6.99 per cent.
Rate of feed 1,471 lb. per hour.
Estimated rate of feed for 30-in. separator, 4,410 lb. per hour.

Results of Dry Separation in Testing Laboratory

The following reports show results of samples tested to determine treatment required and quality of concentrates that could be expected. These tests were run on a regular mill size separator and the results could be duplicated in actual practice. The separate determinations of iron as magnetite, and total iron, were made so that the difference between the two would show the amount of iron combined as silicates in hornblende and other gangue minerals.

No. 93, *Sample* 5, 150 *ft. North of* 4

	Crude 93 lb. Per Cent.	Heads 59 lb. Per Cent.	Tails 34 lb. Per Cent.
Fe as magnetite	51.000	.....	6.140
Total Fe	52.980	65.490	
SiO_2	.....	4.160	
Al_2O_3	.....	4.290	
CaO	.....	0.550	
MgO	.....	1.030	
P	0.010	0.005	
S	.....	Trace	
Mn	.....	0.050	
Ti	.....	0.390	

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No. 94, Sample 6, 20 ft. East of 5

	Crude 99 lb. Per Cent.	Heads 46 lb. Per Cent.	Tails 53 lb. Per Cent.
Fe as magnetite	32.640	.....	6.140
Total Fe	35.120	63.140	
SiO_2	.....	6.510	
Al_2O_3	.....	4.600	
CaO	.....	0.520	
MgO	.....	0.970	
P	0.021	0.010	
S	.....	Trace	
Mn	.....	0.040	
Ti	.....	0.450	

No. 95, Sample 7, from South End

	Crude 113 lb. Per Cent.	Heads 70 lb. Per Cent.	Tails 43 lb. Per Cent.
Fe as magnetite	45.740	.....	6.030
Total Fe	45.740	65.440	
SiO_2	.....	4.780	
Al_2O_3	.....	3.980	
CaO	.....	0.230	
MgO	.....	0.530	
P	0.011	0.009	
S	.....	Trace	
Mn	.....	0.040	
Ti	.....	0.390	

Jan. 20, 1913. H. D. GEHRET.

No. 234, *Separation Test on Jackson Hill Ore, Arnold, N. Y.*

307 lb. crude ore was crushed to pass ⅛-in. screen; separated, by screening, into two sizes, on 16 and through 16-mesh.

Through 8 on 16-mesh 132 lb., through 16, 175 lb.

8–16 size, treated on belt machine using 3½, 4, 5 amp. and finally with 4 amp. for heads. Then 12 amp. for middlings and tails.

Crude 132 lb.	
Heads 20½	Fe 63.35, P 0.006.
Midds 42½	Fe 25.10.
Tails 62	Fe 3.80

The middlings were rolled through 16-mesh and added to crude through 16; the whole passed over belt machine using 2 amp.

Crude 224½ lb.	Fe
Heads 115	Fe 66.15, P 0.005
Tails 108½	Fe 3.00
General crude 307 lb.	Fe 30.85, P 0.008.
General conc. 135½	Fe 65.60, P 0.005.
General tails 171½	Fe 3.27.

Ratio, 2.265 tons crude to 1 ton concentrates.

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Screen Test of Concentrates

Through 8 on 10-mesh	2.56
Through 10 on 20-mesh	26.75
Through 20 on 40-mesh	33.70
Through 40 on 60-mesh	20.50
Through 60 on 80-mesh	5.86
Through 80 on 100-mesh	2.93
Through 100	7.70
	100.00

NOTE.—Owing to the iron being present in very small crystals, it is necessary to crush this ore to at least ⅛ in. before separation, but since the ore is extremely brittle this is easily accomplished with little power.

The tailings consists almost wholly of red feldspar which could be utilized for potash extraction by the Cushman process which is now commercially successful.

Dec. 10, 1915. H. D. GEHRET.

No. 235, *Separation Test on Palmer Hill Ore, Arnold, N. Y.*

325 lb. crude ore was crushed through 4-mesh and screened through sizes:

		Per Cent.
Through 4 on 8	102½ lb.,	31.54
Through 8 on 16	78	24.00
Through 16	144½	44.46
	325 lb.	

The 4 on 8 size, passed over belt machine using 4¾ amp. for heads and 15 amp. for midds and tails.

Heads	6 lb.	Fe 54.80
Midds	85	
Tails	11½	Fe 7.15

The above heads being too low in iron, were added to middlings and rolled through 8-mesh, fines screened out and added to crude.

8 on 16 size, passed over belt machine on 5 amp. for heads and 15 amp. for middlings and tails.

Crude	125 lb.	
Heads	31	Fe 61.65, P 0.006
Midds	62	
Tails	32	Fe 6.35

The middlings ground through 16-mesh, added to crude and passed over belt machine on 8 amp.

Crude	250½ lb.	
Heads	146½	Fe 65.30, P 0.006
Tails	104	Fe 8.65,

General crude	325 lb.	Fe 39.00, P 0.018
General conc.	177½	Fe 64.50, P 0.006
General tails	147½	Fe 8.05

Ratio, 1.83 tons crude per ton of concentrates.

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Screen Test of Concentrates

	Per Cent.
Through 8 on 10-mesh	3.47
Through 10 on 20-mesh	26.60
Through 20 on 40-mesh	33.50
Through 40 on 60-mesh	17.92
Through 60 on 80-mesh	6.36
Through 80 on 100-mesh	2.90
Through 100	9.25
	100.00

NOTE.—In order to reduce the iron in the tailings, finer grinding through 16-mesh will be necessary at the last stage making a three-part separation on the through 16 size and retreating resulting middlings.

Dec. 13, 1915. H. D. GEHRET.

No. 236, *Separation Test on Ore from Battie, Arnold, N.Y.*

290 lb. crude ore was crushed through 4-mesh, screened to size as follows:

		Per Cent.
Through 4 on 8-mesh	130 lb.	44.81
Through 8 on 16-mesh	72 lb.	24.95
Through 16	88 lb.	30.34
	290 lb.	100.00

4–8-mesh size, passed over belt machine using 4½ amp. for heads, and 15 amp. for middlings and tails.

Heads	44 lb.	Fe 60.50, P 0.009
Midds	74	
Tails	12	Fe 8.65

The middlings were rolled through 8-mesh, screened and added to crudes. 8 on 16, 48 lb.; through 16, 26 lb.

8–16 size, passed over belt machine on 4 amp. and 15 amp.

Crude	120 lb.	
Heads	43,	Fe 62.40, P 0.011
Midds	55	
Tails	22,	Fe 8.20

The middlings ground through 16-mesh and added to crude. Through 16-mesh, over belt on 7 amp.

Crude	169 lb.	
Heads	90	Fe 61.80, P 0.006
Tails	79	Fe 8.30

General crude	290 lb.	Fe 41.00, P 0.017
General conc.	177	Fe 61.70, P 0.008
General tails	113	Fe 8.30

Ratio, 1.64.

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Screen Test of Concentrates

			Per Cent.
Through	4 on	8-mesh	23.43
Through	8 on	10-mesh	19.15
Through	10 on	20-mesh	27.45
Through	20 on	40-mesh	20.00
Through	40 on	60-mesh	8.57
Through	60 on	80-mesh	2.85
Through	80 on	100-mesh	2.28
Through	100		6.26
			100.00

Dec. 15, 1915. H. D. GEHRET.

The demonstration of the dry process of magnetic separation is the result of 14 years' work at Mineville, N. Y. Witherbee, Sherman & Co. have now in operation three mills having a combined capacity of 6,000 tons per day of crude ore. The Empire Steel & Iron Co. and the Ringwood Co. have demonstrated what can be done with New Jersey ores. The Ringwood Co. has also worked out a dry process of jigging for their tailings to recover the martite, which is non-magnetic. Martite is a hematite in composition, but is very similar in appearance and crystallization to the magnetite. Some of the magnetic ores have varying amounts of martite mixed with the magnetite.

SUMMARY

The known and partially developed orebodies of New York and New Jersey could, if equipped with the best modern mining and milling machinery and using the best methods, produce at the present time 25,000 tons of 60 per cent. iron ore per day. This can be delivered for an average freight charge of $0.75 per ton from mill to tidewater. The operating cost of production should reach the "dollar rock" ideal of the Lake Superior Copper region, and the cost of mining and milling 1 ton of crude ore should be about $1 for underground mining when handled in large quantities.

The ratio of concentration would be 2 tons of crude per ton of concentrates for an average. There are reserves of magnetic ore sufficient to double the above production, and then last probably 100 years.

DISCUSSION

GEORGE C. FOOTE, Port Henry, N. Y. (written discussion).—The paper by Mr. Norton and Mr. LeFevre will bear the most careful consideration by all interested in the iron business, particularly in the East. There is much information contained therein which should be emphasized.

The idea of handling iron-bearing rock in quantities comparable to the porphyry coppers does seem at first blush to be beyond the pale of commercial possibility. Yet, today in the Adirondack region 25, per cent.

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iron-ore is being mined and milled in small quantities at a profit and an "even break" can be made at present prices with a 20 per cent. ore with present equipment. Large tonnages mined and milled with equipment designed to properly handle and treat this 20 per cent. ore would today show substantial returns to the producers.

Another latent point contained in this paper deserves particular consideration.

It is well known that the old available low phosphorus ores the world over have been growing scarcer each year and even with the normal demand it is only a question of a comparatively short time before furnaces will be facing a famine in this ore.

F. S. Witherbee, in his estimate referred to in the paper under discussion, gave 1,100,000 tons of magnetite ore above 30 per cent. iron content available in one section of the Adirondack region. It is entirely safe to say that of this amount one-half is ore that will average less than 0.020 in phosphorus.

Your attention is directed to the separation tests of the Jackson Hill and the Palmer Hill ores, and particularly to the reduction of the phosphorus in magnetite ores of this character, accomplished by magnetic separation. These tests, together with hundreds of others made by Witherbee, Sherman & Co., demonstrate that the phosphorus in the crude magnetite is reduced one-half by concentration; also that the iron content can easily be increased to 60 per cent. and a furnace product obtained of the same physical characteristics as the well-known old standard concentrates of this region. A crude ore, then, containing 20 to 30 per cent. iron and 0.020 phosphorus will make, by magnetic separation, a product of ideal size for blast furnaces and of 60 per cent. iron and 0.010 phosphorus content.

There are millions of tons of ore of this character in the Adirondack region, notably in the Ausable district of Clinton County, owned by Witherbee, Sherman & Co., and in the Lake Sanford district of Essex County owned by MacIntyre Iron Co., where quarrying and steam-shovel operations could be conducted and low operating costs insured.

Mining and milling costs with modern equipment and large tonnage should not average more than $1 a ton crude ore: The cost of concentrates on board cars at the mines, not over 4 c. per unit; freight from mines to Eastern furnaces, not over 3 c. a unit, making a total cost of this special low phosphorus standard ore, free from copper, of not over 7 c. a unit at the furnace. The average selling price of this grade of ore at Eastern furnaces in normal times has been about 10 c. a unit. Present prices range from 15 to 20 c. a unit. The opinion seems to be that after the war a price of about 11 c. a unit will be maintained for a number of years.

When the enormous tonnage of ore available is considered; the

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coarsely crystalline character of the ore in crude and concentrates; the cheap mining and milling costs; the freight cost to furnaces and the high iron content of the product, the possibilities of this region as a producer of various grades of iron ore begin to be realized.

JOHN L. W. BIRKINBINE, Philadelphia, Pa.—Much that I wish to mention in regard to the paper by Mr. Norton and Mr. LeFevre has already been taken up in the discussion of Mr. Foote, but there are a few points left that I would like to call to your attention. I think Mr. Norton and Mr. LeFevre did not mean to say that any magnetite iron ore can be concentrated for 10 or 12 c. a ton. They probably intended to state that coarse-grained ore, easily granulated, can be concentrated at this price.

In examining the flow sheets for mills 3, 4, and 5, your attention is called to the policy of subjecting the rock to the minimum amount of crushing. You will note that in mills 3 and 5 the rich ore was removed wherever possible without further reduction. This makes a good furnace material, while in mill 4 the barren material is rejected so as not to crush this to small size, as fine grinding not only causes great wear and tear on the machinery but also utilization of a large amount of power.

The authors' recommendation that before deciding on any method of concentrating iron ore, there should be a great deal of experimentation, should not be neglected, for at Mineville the whole system of mills has been an experiment on an enormous scale and the success of that plant has been due to the willingness of the men behind it to spend large sums of money for experimental purposes.

For the past year or so, I have been devoting a great deal of time to studying the lean magnetites that occur in the East, and although many of these deposits, especially those in eastern Pennsylvania, were formerly operated when the iron industry was in its infancy in the United States, they were afterward abandoned. In going over the deposits, we are surprised to find the great diversity in the characteristics of the ore.

In one section in Berks County, Pennsylvania, in a distance of 12 miles there are five different ranges of ore, each one distinct in its mode of occurrence and character; some being high in sulphur, some of Bessemer grade, some titaniferous, some rich in iron and high in titanium and sulphur, others lean in iron and carrying little of the detrimental substances. Of these the best known are the magnetites of the Cornwall type, which are universally high in sulphur, the gangue generally consisting of lime, but sometimes of shale. The ore of the Cornwall type occurs between the Mesozoic conglomerates and the Paleozoic limestones, near diabase intrusions, the other magnetites occurring in gneiss.

It is my hope that in the early future I may be able to present to the Institute more detailed information regarding these deposits.

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FRANK L. NASON, West Haven, Conn. (communication to the Secretary*).—I shall have to depend on memory for most of the following statements, as I have not a copy of Mr. Witherbee's paper before me.

The total area of the magnetic iron ore field of the Adirondack Region is about 9,000 sq. miles. Exclusive of the gabbros, which cover about 2,500 sq. miles, inclusive of the Grenville, or white limestone series, the iron-ore gneisses cover 6,500 sq. miles. I am quite sure that Mr. Witherbee, in estimating the reserve tonnage, took into consideration only the areas in which great tonnages have been actually produced, leaving entirely out of consideration all other possible areas, including the gabbros exclusive of the Tahawus titaniferous ores. In Clinton County, the distance from Palmer Hill to Lyon Mountain is about 20 miles. The width of the field from east to west is about 15 miles. The total area is thus about 300 sq. miles.

In Essex County, the area is about 200 sq. miles; exclusive of the Gouverneur red hematite field, the Benson mine area is about 20 sq. miles; the total area covered by Mr. Witherbee's estimates is thus about 520 sq. miles. This is 8 per cent. of the total area of magnetic iron ore-bearing gneisses.

As to the tonnage per square mile, there seems to be something wrong with the writers' estimates. There are 43,560 sq. ft. in 1 acre. With a thickness of 10 ft. of iron ore, allowing 10 cu. ft. per ton, there are 43,560 tons per acre or 27,878,400 tons per square mile, instead of 2,700,000 tons as stated in the paper.

Taking one-tenth of the estimated area (not the total area) gives a productive area of 52 sq. miles. According to data assumed, this 52 sq. miles would yield 1,449,800 thousand tons, practically Mr. Witherbee's estimate. This is exclusive of the 70 to 100 million tons of titaniferous iron ores in the Tahawus field. In New Jersey, the authors' estimate of iron ore area is 900 sq. miles. They are not as conservative in their estimates as is Mr. Witherbee, for they assume that the entire belt of iron-ore gneisses is underlaid with minable ore. This gives them the total of 2,300,000,000 tons. If they had followed Mr. Witherbee's excellent example, one-tenth of 8 per cent., the productive area would be 7.2 sq. miles with a total reserve of 19,440,000 tons as against their estimate of 2,300,000,000 tons. Still farther, the area of iron-bearing gneisses in southeastern New York, including the Tilly Foster and Theal mines areas, is about 400 sq. miles. As above, this would reduce to a productive area of 3.2 sq. miles, adding about 7,640,000 tons to the New Jersey reserves, making a grand total of 27,080,000 tons in the southern magnetic iron ore field.

It seems very clear to me that Messrs. Norton and LeFevre have not

* Received Feb. 20, 1917.

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thoroughly studied Mr. Witherbee's paper. At least they have held him to "strict accountability" in his own field and have applied his methods only nominally in dealing with the New Jersey field. In dealing with this field (the New Jersey) they seem to me to have ignored some very important data. To point these out would take more time than is allowable in discussion. They may be made the subject of a separate paper later on. Personally, however, while engaged on the geological survey of New Jersey and private work later, my estimate of reserves in this State and in southeastern New York, which I have considered liberal, amount, in round numbers, to about 50 per cent. of the total given in the paper under discussion.

S. Le Fevre, Forest Glen, N. Y. (communication to the Secretary*).—F. L. Nason thinks I could not have studied Mr. Witherbee's paper and doubts the arithmetic used, but in his discussion arrives at practically the same conclusion. He says 10 ft. thickness of ore over a square mile should give 27,878,400 tons, allowing 10 cu. ft. per ton of ore. This is correct, but if he reads carefully he will see that this depth of ore was figured as covering one-tenth of the surface, which reduces the figures to those used by Mr. Nason.

I did not say, however, that there were 2,300,000,000 tons of ore in New Jersey, but that applying the same method of computation as used by Mr. Nason, to the area of the iron ore-bearing gneisses in New Jersey, we would get a total of 2,300,000,000.

I did say, "In this area are located 366 magnetite mines that have been worked more or less. These lenses may easily be capable of producing (on an average) 1,000,000 tons each, and there are probably double the number listed not opened up."

Thus my estimate for New Jersey, as far as any was made, would be:

Probable ore	366,000,000
Possible ore in addition	732,000,000
	1,098,000,000

Mr. Nason's final conclusion of his personal estimate was one-half of what he erroneously read to be the estimate given in the paper, or half of 2,300,000,000, which equals 1,150,000,000.

So the two estimates seem to differ less than 5 per cent., and we have the two sister States of New York and New Jersey on an equal footing of productive possibilities in magnetic ore.

* Received Apr. 24, 1917.

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Modern Methods of Mining and Ventilating Thick Pitching Beds

BY H. M. CRANKSHAW,* B. S., HAZLETON, PA.

(Arizona Meeting, September, 1916)

THE early methods of mining anthracite in the steep pitching Mammoth bed consisted in driving breasts up the pitch from the gangways and airways driven in the bed along the strike (Plate 2, Fig. 1). Breasts are simply rooms driven up the pitch the full thickness of the bed (Plate 2, Fig. 2), having a width of from 18 to 30 ft., and a manway or traveling way on one or both sides. (For detail of breast method see Whildin, *Trans.*, vol. 50, p. 704).

Many difficulties were encountered, and the maintenance cost of the timbered gangways was so great as to be practically prohibitive. To reduce this expense, the gangways were driven in the thin Skidmore bed underlying the Mammoth, and the coal mined through rock holes. (Plate 2, Fig. 2, *A*.)

It was found that after the breasts were driven up 80 to 100 ft., the coal almost invariably rushed or "ran away," making good ventilation almost impossible and the extraction of coal from the upper portion of the lift very difficult. The economic length of a lift is from 200 to 250 ft., and the percentage of extraction by the old method above mentioned was often under 50 per cent., while the coal recovered was so badly broken up as to reduce materially the percentage of large sizes.

Principles of Present Mining Methods

The present methods designed for the recovery of a maximum percentage of coal and a maximum yield of the prepared sizes depends largely on the following principles:

First.—Formerly the practice in mining bituminous coal was to space rooms so that as much coal as possible could be extracted in first mining (Plate 3). The small pillars were often lost, and an improvement in total extraction was found in the system of driving narrow rooms with wide pillars, with a much larger final recovery (Plate 4).

Second.—The early method of ventilation was to maintain a single

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* Manager, Harwood Coal Co.

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current of air throughout the mine. This has been modified by systematically splitting the air and so establishing more efficient ventilation.

Third.—The coal should be so mined as to allow the overlying measures to break and relieve the pressure, the break to be controlled and regulated by the method of mining so that it occurs where it is desired. This principle, which governs longwall mining in England, is fully recognized by the Consolidation Coal Co., and was well brought out in Mr. Grady's paper,[1] but in the type of mining under discussion it has rarely been applied. It has been claimed also by some mining engineers that the principle is inapplicable to the anthracite region, as the tough overly-

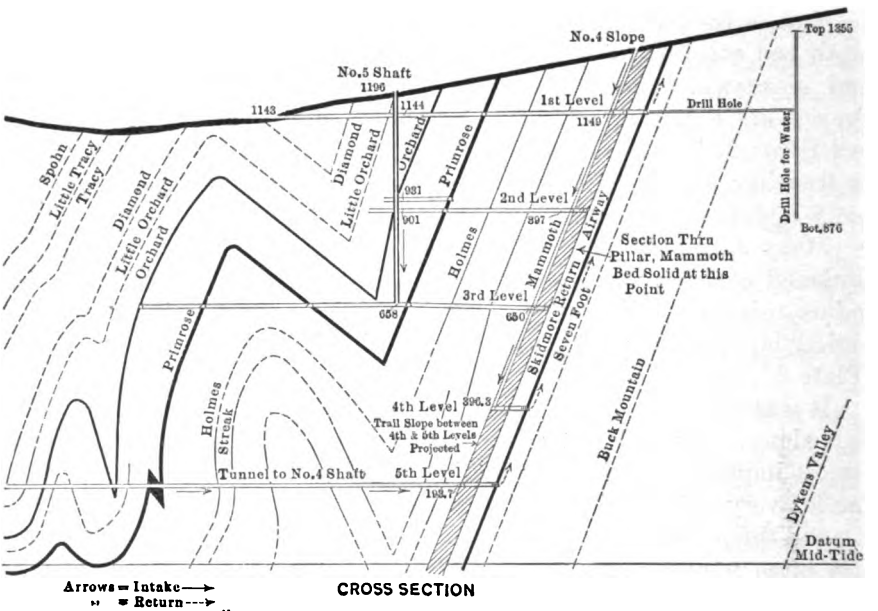

PLATE 1.—METHOD OF MINING STEEP PITCHING MAMMOTH BED.
(Typical Section)

ing strata would not break; that this conclusion is unwarranted is indicated by the fact that in longwall bituminous coal mining it is not unusual for several acres of roof to stand before the first break occurs, after which the roof breaks without further trouble.

Therefore, we believe it has been proven that we must in first mining leave strong pillars; we must scientifically split the air to secure efficient ventilation; and we must so regulate the extraction of pillars that the roof breaks may be controlled.

[1] *Trans.*, vol. 51, p. 138.

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Disadvantages of Old Method

In the old method of mining the Mammoth bed, the coal was first partly worked over, after which the ground squeezed and settled through a period of years until the coal left behind had been so consolidated by pressure as to again allow of chutes being driven in the squeezed bed; and

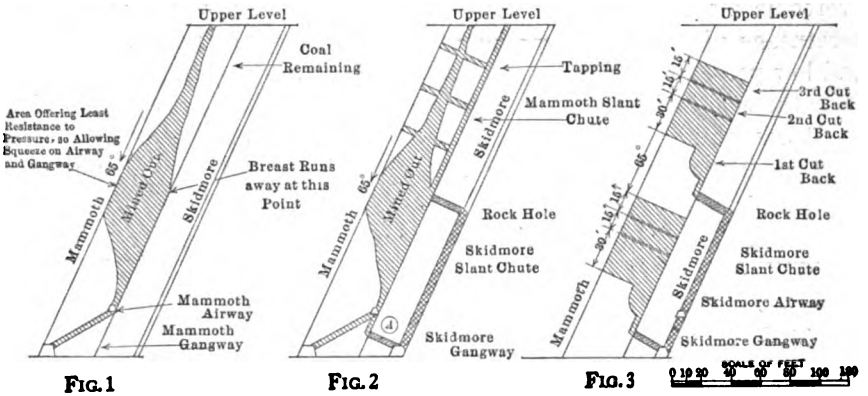

FIG. 1 FIG. 2 FIG. 3

PLATE 2.—OLD AND NEW METHODS OF MINING MAMMOTH BED.

the original method of mining repeated, this process sometimes being gone over several times before the final extraction of the coal from the bed. The squeezing considerably shattered the coal and resulted in a much decreased percentage of the prepared sizes in the product of the second and subsequent minings.

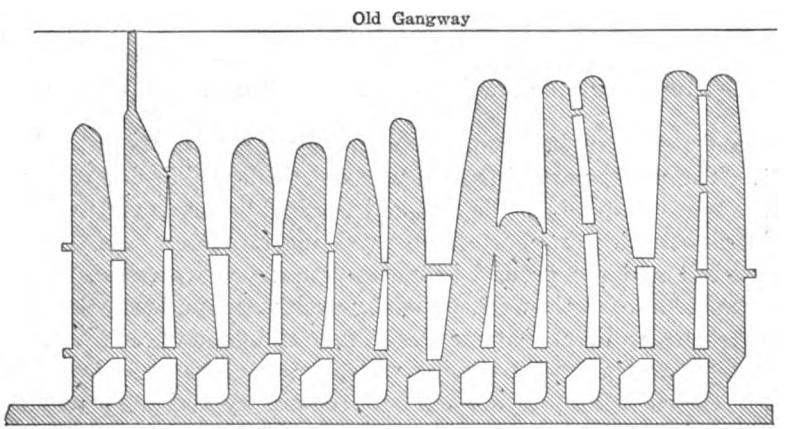

PLATE 3.—SKETCH SHOWING PRIMITIVE METHOD OF MINING.

The modern methods aim to take out as much coal as possible in the first mining, and to avoid going over the same ground several times as was formerly necessary.

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The method described is the result of 3 years of careful work and experiment, using at the same time all knowledge obtainable from the failures of the past (Plates 5 and 6). The paper by Mr. Whildin[2] described the methods developed for reworking mined-over ground. While doing this work constant studies were directed toward the development of a method that would be applicable to virgin ground, and which would overcome the defects of the old methods, the chief of which were: Low percentage of extraction; excessive cost of timbering; poor ventilation, and low percentage of prepared sizes, due to reworking the same ground after the coal had been crushed and squeezed.

It was found that after a breast had "run away" and been drawn until the rock from old workings appeared at the battery, the greater portion

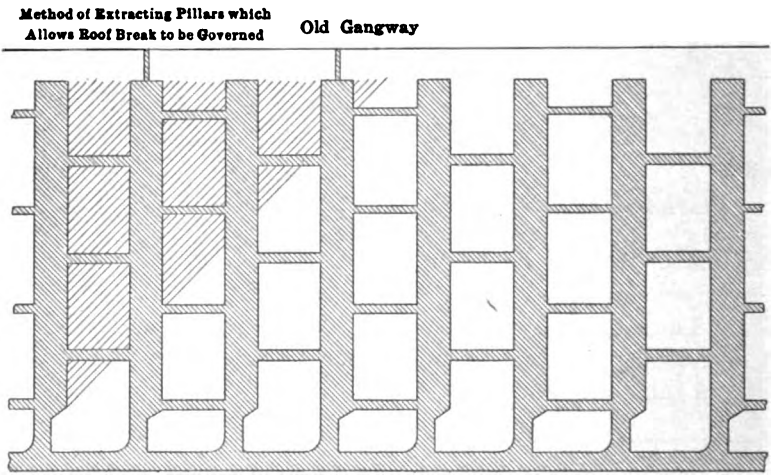

PLATE 4.—SKETCH SHOWING MODERN METHOD OF MINING. SCALE 1 IN. = 15 FT.

of the coal recovered had come from the lower part of the lift, Plate 2, Fig. 1. The upper part of the lift was practically solid, due to the fact that the space from which the coal had run had become practically filled with loose rock which supported the overlying measures. As the pillars were robbed and the ground began to squeeze, the pressure naturally did the most havoc at the weakest point, which was found near the bottom of the breasts, or directly above the airway and gangway.

Since in most cases the mining was not done so as to allow a clean break of the roof, the pressure came on the airway and gangways in the form of a slow squeeze which broke the timber. This resulted in the conclusion that heavier timber was needed, and in many cases, sticks 24 to 30 in., and even 36 in. diameter were used for timbering purposes, resulting in an excessive timber cost.

[2] *Trans.*, vol. 50, p. 698.

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A careful study of Plate 2, Fig. 1, will show why the cost of timbering in Mammoth airways and gangways is excessive, and how the squeeze was brought on by the old method.

To reduce this timber cost the gangways were driven in the Skidmore bed underlying, instead of in the Mammoth (Plate 2, Fig. 2), involving the use of rock chutes from the Skidmore and the driving of slant chutes in the latter. The result was the development of a complete method of working the Mammoth bed from the underlying beds (Plate 2, Fig. 3).

Description of Present Method

In the working of a lift in the Mammoth bed 250 ft. long on a pitch of 65°, the bed being 40 ft. thick, the gangways and airways are first

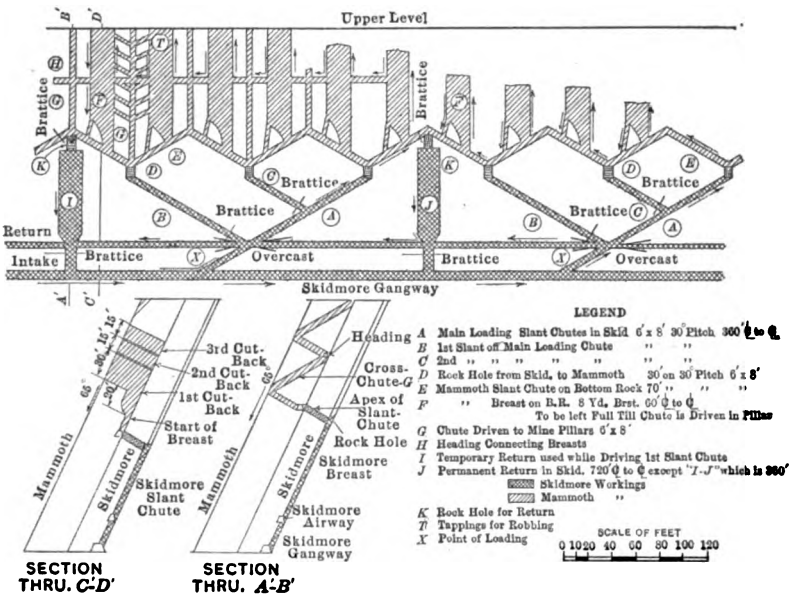

PLATE 5.—PRESENT METHOD OF MINING, UPPER SECTION.

driven in the usual way in the underlying bed. Rock holes or chutes to the Mammoth from this gangway level are not driven until the workings in the underlying bed, following the plan to be described just below, have reached a point about halfway up the lift, Plate 5, with the result that the breasts in the Mammoth are restricted to a length of about 100 ft. The length of breasts was limited to 100 ft. because, as previously observed, past experience showed that the breasts almost invariably ran away when this length had been driven, making it practically impossible to carry to completion any system of working involving any greater length.

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In the development of this plan, chutes *A*, *B*, *C*, Plate 5, are driven in the Skidmore bed, underlying the Mammoth, and rock holes *D* put through to the Mammoth 120 ft. apart, and from a point halfway up the lift. These rock holes are then connected by slant chutes *E* along the bottom rock of the Mammoth, from which the breasts are started on 60-ft. centers in the usual way. Each rock hole handles the coal from two breasts and the coal from three rock holes comes together at one loading chute at *X*.

It has been found desirable not to drive the rock chutes much in advance of the mining of the coal in the Mammoth, and to mine this coal as rapidly as possible. By this method after the work is once fully

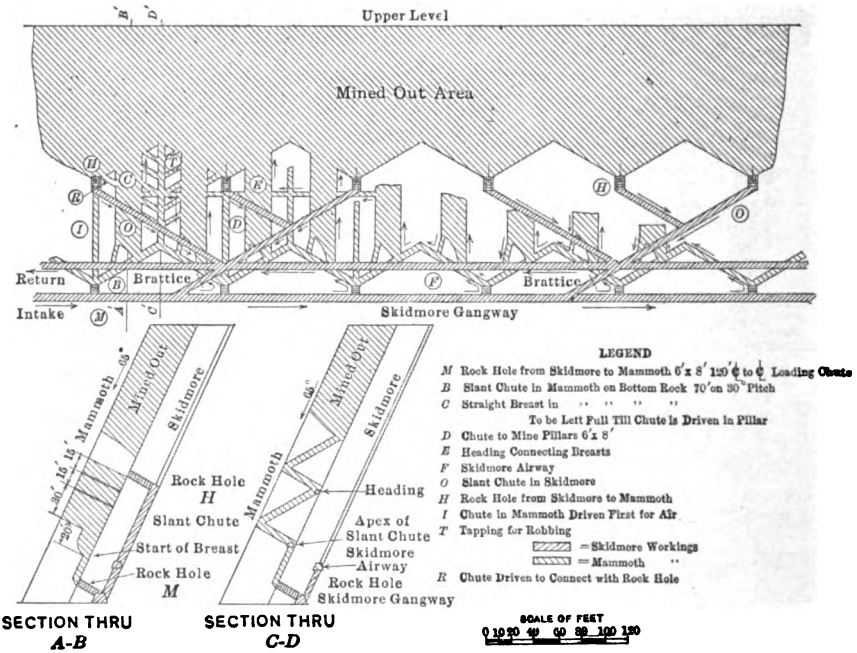

PLATE 6.—PRESENT METHOD OF MINING, LOWER SECTION.

opened out the cost of the rock work is only about 20 per cent. of the total cost of timbering under the former method of mining.

Each set of six breasts has a separate split of fresh air and a ventilating system of its own. The ventilation is arranged very simply, on the split system, by driving a Skidmore breast *I* and *J* from the Skidmore airway far enough up the pitch so that a rock hole *K* put through from the top of the breast will connect with the apex of the slant chutes *E*. The main loading chutes *A*, 360 ft. apart, then become the main intakes, and as shown by the arrows, the air returns through rock holes *K* and Skidmore breasts *J* to the Skidmore return airway.

After the breasts are driven through to the upper level, cross chutes

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G are driven across the bed from the apex of the slant chutes *E*, for the purpose of robbing the pillars. The breasts are allowed to stand full until the cross chutes *G* are driven through to the upper level, and are drawn as the cross chutes *G* are worked down, the pillars being completely robbed by means of the tapping chutes *T*.

In this way all the chutes are driven in solid coal, the men are in strong ground all the time, thus being well protected, and a good supply of fresh air is always available to dilute any outburst of gas.

When the upper half of the lift has been robbed back, the lower half is worked in a similar way from the rock holes *M*, Plate 6, driven direct from the Skidmore gangway. These rock holes are connected by slant chutes *B* driven along the bottom rock of the Mammoth, and the whole operation repeated as for the upper half of the lift. Working the upper half first, however, has given an opportunity to ventilate the lower half easily, as follows:

As soon as the first breast *C* is driven up, a short chute *R* is driven from near the top of the breast to connect with the rock holes *H*; the slant chutes *O*, in the Skidmore bed, which previously were used as coal chutes, now serve as return airways, with no expense for upkeep.

The main chutes, wherever possible, should be driven 6 by 8 ft. so as to provide a good-sized manway for traveling and for carrying up timber. In order to secure a reserve pillar for fire protection and for limiting possible squeezing a large pillar is left every 720 ft., the length of each double section.

This method was first tried on the west side of the fifth level of No. 4 Shaft, Lansford Colliery, where the upper half of the lift in the first part of the work is now being robbed down. The results as to cost and extraction have been excellent.

Ventilation System

One result of the method of mining before described is to get all the main airways in strong permanent ground, so that they are not subjected to squeezing, and require a minimum expense for upkeep. Two mines producing 700,000 tons per year were working the section where the ventilation was remodeled, this work taking 2 years to complete. Two electrically driven Sirocco fans were installed, of 100,000 and 150,000 cu. ft. capacity, respectively, the larger one operated blowing and the smaller exhausting.

The mines were originally ventilated by fans located in the center of the property in the usual way (Plate 7). The air traveled along the main haulageways as intake airways and returned along return airways parallel to the main haulageways. As the mining concentrated at the extremities of the property, the following defects became apparent, resulting in unsatisfactory quality of air in the working places.

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To get to the fans the air had to travel 2 miles; the main haulageways were so filled with traffic as the mines increased in production that their function as an intake airway was materially impaired; the return airways, where they were in the Mammoth bed, were exceedingly expensive to maintain, and where they were in the Skidmore, in many cases were too small to carry, for the required long distances, the volume of air needed.

By replacing the central system of ventilation with fans located near the extremities, the length of travel of the air was halved, and it was found practicable to abandon sections of the Mammoth airway which were expensive to maintain. In this way, an efficient method of ventilation was worked out, which made it possible to work coal which had been inaccessible for many years.

The diagram, Plate 7, shows the ventilating system employed in these mines, the central fan, by exhaust, ventilates the fifth or lower level,

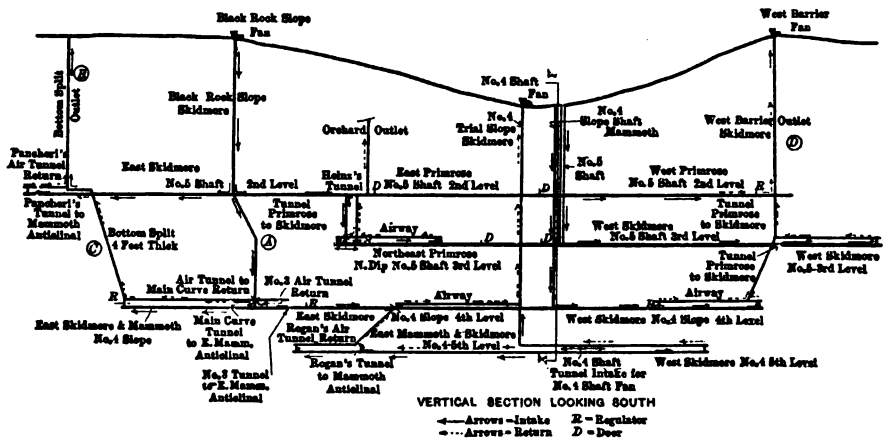

PLATE 7.—VENTILATING SYSTEM.

which is known as No. 4 Shaft level, and the "Black Rock Slope" force fan ventilates the second, third and fourth level on the east side, and the "West Barrier" exhaust fan ventilates the second, third and fourth levels on the west side.

By making the "Black Rock Slope" a blowing fan, it was possible to ventilate the section known as Pancheries' Tunnel, and the section known as the N. E. Primrose, without the necessity of having long return airways, the haulageways acting as intake airways for these sections.

When the three fans are in operation, the territory is so divided that there are a proportionate number of men to each fan, and the main air current is in each case divided into at least three main splits, each main split being again split up in the working places as required.

To date the adoption of the mining and ventilation methods described have given the following results:

In 1915, up to Oct. 30, 50,000 tons was mined by the new method,

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the labor cost being 19 c. per car lower than by the old method under similar conditions, and the saving in cost of timber at least 30 per cent.

While reliable figures as to extraction will not be available for about 2 years, it is reasonable to believe that as the coal is "mined" instead of being allowed to run, less rock will be extracted, and the total car yield will be materially increased.

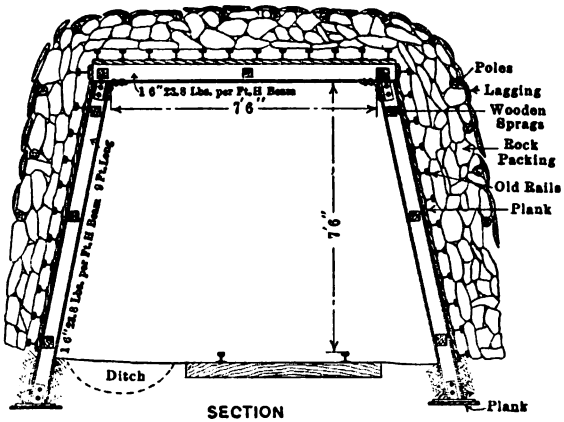

PLATE 8.—METHOD OF ERECTING STEEL TIMBER IN MAMMOTH BED.

Use of Steel Timber

The use of steel timber is only commercially justified in gangways in the Mammoth bed when such gangways are not subject to squeeze due to mining operations in their immediate vicinity; but all gangways, turnouts and other places in the Mammoth bed, which are required for use over a period of many years, should be lined with steel timber properly erected.

For general use, it has been found advisable to use H-section steel for both legs and collars; 5- and 6-in. H-beams have been found most suitable for sets in gangways, with sizes for turnouts and special work as required by the conditions.

The method of erection is shown in Plate 8.

Plates 9 and 10 illustrate typical examples of this method of timbering. In the gangways shown, the Mammoth bed is 60 ft. thick, very dry and very liable to run, the gangway being 1,000 ft. below the surface. The best of wooden timber rots out in 2 years.

Plate 9 shows the gangway timbered up to the rock lying between the Mammoth and Skidmore beds. The tunnel was afterward driven without any of the timber being disturbed by the blasting of the rock.

Plate 10 is an example of the replacement of wood by steel. Note the comparative size of wood and steel.

The author desires to express his appreciation of the assistance given by H. D. Kynor, John L. Richards and A. L. Walbridge in carrying out the work described and in preparing this paper.

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PLATE 9.—GANGWAY IN WHICH STEEL "TIMBER" IS USED.

PLATE 10.—WOOD BEING REPLACED BY STEEL FOR GANGWAY SUPPORTS.

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The Illuminating Power of Safety Lamps

BY W. M. WEIGEL,* E. M., STATE COLLEGE, PA.

(Arizona Meeting, September, 1916)

WHILE electric lamps both of the cap and hand type are being introduced into many mines requiring the use of safety lamps, the oil-burning safety lamp is still used in the great majority of cases, and even where the former are used the latter must continue to be employed when testing for gas. It is not the intention to discuss here the relative merits of the two types, but simply to give a comparison of the illuminating effect or candlepower of several kinds of ordinary safety lamps used for general work and for testing. Few data have been published in recent years on this subject and because of this it is hoped that the following information may be of some interest and value.

The lamps tested were in all cases of standard size and in good condition, all practically new, and were selected from a collection of about 100 with a view to obtaining results on as many different common types as possible. With the exception of the gasoline-burning lamps, five different oils were used in each lamp to determine whether particular oils were suited to particular lamps and whether there was any great difference in the illuminating power of the different oils, or whether some oils would maintain their illuminating power better than others throughout a working period of 8 hr. No attempt was made to determine the amount or cost of oil burned by each lamp, but the condition of the lamp, wick, glass, and gauzes was noted after each test.

Description of Apparatus Used

The candlepower was determined with a Weber photometer, Fig. 1, which consists of a tube *A* containing a movable milk-glass screen adjustable by the milled screw *B*. The distance of this screen from the standard light at *C* is read on the scale attached to *A*. The standard light consists of a benzine lamp so arranged that the height of the flame may be adjusted exactly by observing it against a scale in the lamp box through an opening in the front. The lamp to be tested is placed at *D* so as to illuminate a milk-glass screen in the holder at *E*. At the intersection of the axes of tube *A* and sight-tube *F* are placed two 45° prisms with their surfaces in contact so that the light from *D* may pass through with-

* Associate Professor of Mining, Pennsylvania State College.

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out deflection while that from C is reflected at the contact in a direction parallel to that from D. The faces of the prisms are blackened around an elliptical field and on this field is an annular silvered ring which reflects the light from C. When viewed through the telescopic eyepiece, G, focused on the prisms, on outer annular ring and inner elliptical surface are seen illuminated by light D and an inner ring illuminated by that from C. With D in a fixed position, the milk-glass screen in A is adjusted until an evenly illuminated field is observed through the eyepiece. Then the

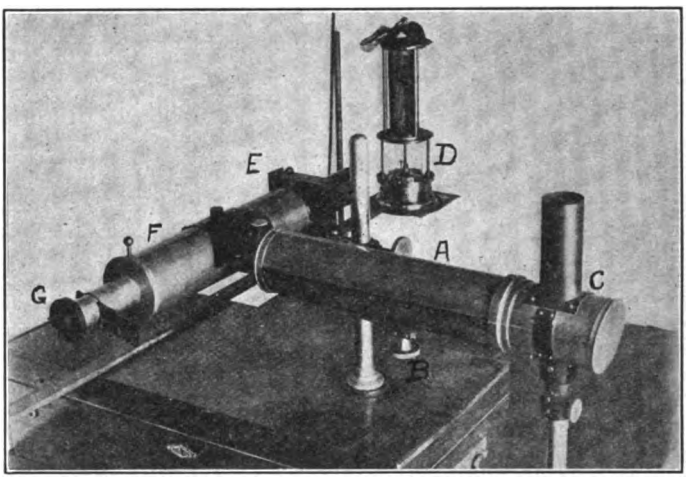

FIG. 1.—WEBER PHOTOMETER USED IN DETERMINING CANDLEPOWER OF LAMPS.

screens in A and E are illuminated to the same intensity and the intensities of the standard and unknown light are to each other as the squares of their distances from the respective screens, or expressed as a formula

$$i = \frac{R^2}{r^2} C$$

in which i = candlepower or intensity of light being tested,
R = distance of D (light being tested) from screen E,
r = distance of C (standard light) from screen in tube A,
C = constant involving the candlepower of the standard lamp, relative opacity of the two screens and characteristics of the apparatus.

Determination of the constant C was accomplished by placing a standard candle at D. The value of C is then given by writing the formula

$$C = \frac{r^2}{R^2}$$

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as the intensity i is then unity. About 50 readings were taken using standard spermaceti candles. Several candles were employed and their illuminating power checked by weighing before and after use and determining the consumption of spermaceti per hour. A standard candle is supposed to burn 120 grains per hour. The candles used consumed from 123 to 124 grains per hour equivalent to a candlepower of 1.03. The average value obtained of the constant C was 0.36. The candle was placed at the same distance as the lamps tested and also at greater and less distances.

The specific gravity of the oils was determined with a Westphal balance, and the Baumé degrees calculated.

Description of the Lamps

The lamps tested were selected to give a wide range in variety and were of standard make and size (Figs. 2, 3 and 4). Before beginning

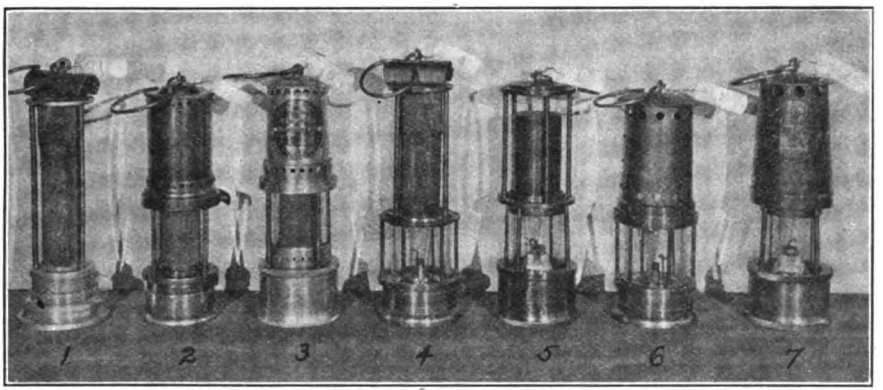

FIG. 2.—SAFETY LAMPS NOS. 1 TO 7 TESTED FOR ILLUMINATING POWER.

each test with the several oils they were all thoroughly cleaned, the oil vessels rinsed out with gasoline and dried and fresh wicks put in. The shape and size of wicks, number and dimensions of gauzes and oils used are given in Table 1.

Lamp No. 1 is an ordinary Davy with aluminum frame.

Lamp No. 2 is a Davy lamp with bonnet and glass shield surrounding the lower portion of the gauze but which could be lifted when testing. It is also known as the Hughes-Evan Thomas lamp. Normally it takes its air through openings in the base ring covered on the inside by the lower end of the gauze.

Lamp No. 3 is of the same type but with aluminum oil vessel, frame, and bonnet. Also it has a Hailwood porcelain flat wick burner.

Lamp No. 4 is an ordinary improved Clanny, unbonneted, with brass oil vessel and frame.

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Lamp No. 5 is constructed on the principle of a Clanny, unbonneted but the gauze consists of perforated sheet copper instead of woven wire. It is also equipped with a Hailwood porcelain flat burner and brass oil vessel and frame.

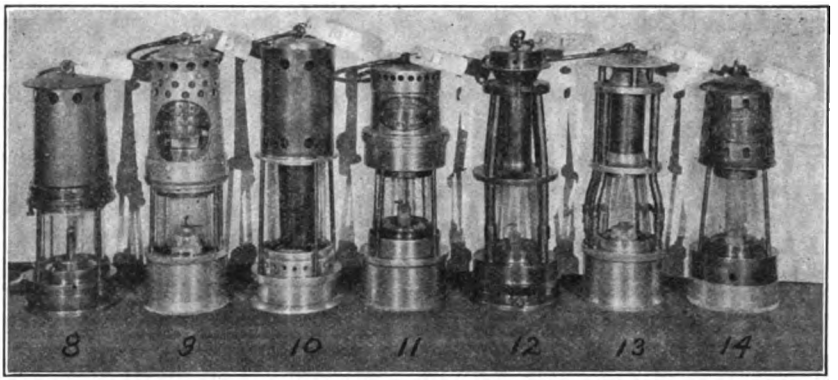

FIG. 3.—LAMPS NOS. 8 TO 14 TESTED FOR ILLUMINATING POWER.

Lamp No. 6 is a bonneted Clanny very similar to the lamp known as the Evan Thomas No. 7. The air is admitted through openings in the lower part of the bonnet, and a deflecting ring is placed between these openings and the gauze, requiring the air to pass upward and over this

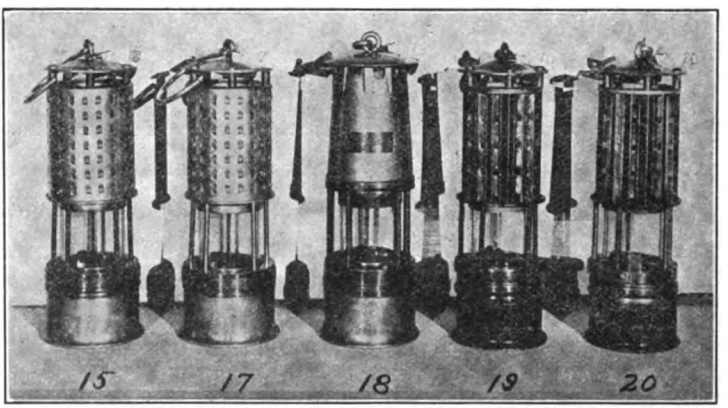

FIG. 4.—GASOLINE-BURNING LAMPS TESTED.

deflector. The oil vessel and frame is brass and bonnet is of sheet iron.

Lamp No. 7 is a standard Hailwood working lamp constructed on the Marsaut principle with two gauzes. Oil vessel and frame of brass, bonnet of sheet iron, and porcelain flat-wick burner.

Lamp No. 8 is a bonneted Clanny with smoke gauze.

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Lamp No. 9 is a Marsaut type with Stokes shut-off ring at base of bonnet by which air is admitted at that point or, when closed, forced to enter the lower set of holes near the top of bonnet and pass down behind a deflecting ring extending from just above the inlet openings to about the middle of the bonnet. The lamp is fitted with Hailwood flat-wick porcelain burner.

Lamp No. 10 is a Beard Deputy with Beard-Mackie sight indicator. The circulation of air is on the Eloin principle, the air entering through gauze-covered openings around the base below the flame.

Lamp No. 11 is a testing lamp of English manufacture. By means of a sliding ring around the base the air may enter gauze-covered openings below the flame, or, when these are closed, it enters through openings in the lower part of the bonnet passing through the lower part of the gauze as in the ordinary Clanny lamp.

Lamp No. 12 is an ordinary Ashworth-Hepplewhite-Gray.

Lamp No. 13 is an improved Ashworth-Hepplewhite-Gray with Hailwood porcelain flat-wick burner and six hollow inlet tubes from top to bottom, each with an opening at the bottom, instead of four inlet tubes as in the ordinary form No. 12.

Lamp No. 14 is an improved Gray lamp with brass flat-wick burner. A single hollow standard of large size connects the gauze-covered air inlet in the base with the interior of the bonnet. This standard has an opening near the bottom with sliding sleeve to cover it. The gauze is of much larger diameter than those of Nos. 12 and 13.

Lamps No. 15 and 17 are respectively round and flat-wick Koehler's, burning gasoline.

Lamp No. 18 is a flat-wick Cremer, burning gasoline, differing from the Koehler and Wolf in the construction of the bonnet. The air enters through slots in the circumference of the base of the bonnet and not through perforations along the sides. It has a corrugated expansion ring between the base of bonnet and flange of the gauzes similar to the Koehler.

Lamps No. 19 and 20 are respectively standard round- and flat-wick Wolf's.

The five gasoline lamps are all fitted with Pyro igniters.

The illuminating power of some of the types tested as quoted by other authorities is given in Table 2. A wide variation in some cases will be noted. It may be proper to mention at this point that some of the results obtained were checked by testing the same lamps with a Bunsen type photometer, the results agreeing closely. The greatest difference will be noted in Nos. 10 and 12. No. 10 was an aluminum Beard-Deputy with Beard-Mackie sight indicator. To check results on this, No. 10*a* was tested with one oil. No. 10*a* lamp was identical except that it was made of brass, the indicator was omitted, and it did

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TABLE 1.—*Candlepower of Safety Lamps*

Number	Type	Location of Air Inlet	No. of Gauzes	Size of Gauzes, Inches	Bonnet	Size of Wick Tube, Inches R = round F = flat	Kind of Oil	Candlepower, Freshly Filled and Clean	Candlepower at End of 4 Hr.	Candlepower at End of 8 Hr.	Average Candlepower	Condition of Lamp at End of 8 Hr.
1	Common-Davy...	Entire gauze unprotected.	1	1½×5½	No	¼ R	Eureka.....	0.12	0.16	0.12	0.13	Gauze fairly clean, wick in good shape.
							Mineral Seal	0.10	0.09	0.11	0.10	Top of gauze smoked.
							Sperm.....	0.15	0.11	0.14	0.13	Required frequent attention and snuffing.
							Lard and kerosene	0.14	0.13	0.10	0.12	Good.
2	Hughes-Evan Thomas.	Below glass shield....	1	1½×5⅜	Yes	¼ R	Eureka.....	0.12	0.11	0.11	0.11	Same as No. 1.
							Mineral Seal	0.12	0.09	0.11	0.11	Top of gauze and cap smoked.
							Sperm.....	0.10	0.11	0.13	0.11	Required frequent attention and snuffing to keep burning.
							Lard and kerosene	0.14	0.10	0.12	0.12	Good.
3	Hughes-Evan Thomas.	Below glass shield....	1	1⅜ top 1½ bottom ×4½	Yes	3/32×½ F	Eureka.....	0.16	0.18	0.15	0.16	Same as No. 1.
							Mineral Seal	0.16	0.11	0.12	0.13	Gauze clean.
							Sperm.....	0.15	0.15	0.18	0.16	Same as No. 2, gauze clean.
							Lard and kerosene	0.17	0.09	0.13	0.13	Good.
4	Clanny.....	Base of gauze.....	1	1⅝×4¼	No	9/32 R	Eureka.....	0.34	0.35	0.43	0.37	Same as No. 1.
							Mineral Seal	0.32	0.30	0.33	0.32	Gauze clean.
							Sperm.....	0.27	0.24	0.25	0.25	Same as No. 3.
							Lard and kerosene	0.47	0.41	0.33	0.40	Good.
5	Clanny, but with perforated sheet copper gauze.	Base of gauze.....	1	1⅞×3	No	3/32×½ F	Eureka.....	0.34	0.42	0.33	0.36	Same as No. 1.
							Mineral Seal	0.42	0.35	0.36	0.38	Gauze clean.
							Sperm.....	0.37	0.28	0.37	0.34	Same as No. 3.
							Lard and kerosene	0.48	0.28	0.25	0.34	Good.
6	Evan Thomas No. 7.	Base of bonnet.....	1	1 5/16 top 1 15/16 bottom ×3¼	Yes	9/32 R	Eureka.....	0.38	0.38	0.31	0.36	Same as No. 1.
							Mineral Seal	0.38	0.36	0.40	0.38	Top of gauze and cap slightly choked.
							Sperm.....	0.30	0.21	0.32	0.28	Same as No. 3.
							Lard and kerosene	0.46	0.41	0.34	0.40	Good.
7	Marsaut.....	Base of bonnet.....	2	1 13/16 & 1 15/16×3½ 1 5/16 & 1 1/16×3	Yes	3/32×½ F	Eureka.....	0.35	0.39	0.28	0.34	Same as No. 1.
							Mineral Seal	0.40	0.31	0.35	0.35	Gauzes clean.
							Sperm.....	0.45	0.37	0.38	0.40	Same as No. 3.
							Lard and kerosene	0.43	0.37	0.32	0.37	Good.
8	Clanny, bonneted.	Base of bonnet.....	1	1⅝ top 1 15/16 bottom ×3	Yes	5/16 R	Eureka.....	0.35	0.35	0.36	0.35	Same as No. 1.
							Mineral Seal	0.35	0.31	0.32	0.33	Top of gauze choked.
							Sperm.....	0.30	0.29	0.39	0.33	Same as No. 3.
							Lard and kerosene	0.40	0.34	0.30	0.35	Good.

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TABLE 1.—(*Continued*)

9	Marsaut with Stokes shut-off.	Base of bonnet, or lower holes at top of bonnet.	2	1¾×2×3½ 1⁹⁄₁₆×1¹¹⁄₁₆×3	Yes	³⁄₃₂×½ F	Eureka..... Mineral Seal .... Sperm..... Lard and kerosene	0.32 0.36 0.30 0.43	0.31 0.34 0.30 0.24	0.31 0.35 0.36 0.32	0.31 0.35 0.32 0.33	Same as No. 1. Inner gause slightly choked. Same as No. 3. Good.
10	Beard-Deputy....	Below flame.....	2	1½&1¹¹⁄₁₆×3½ 1¼&1¾×3¾	Yes	5⁄16 R	Eureka..... Mineral Seal .... Sperm..... Lard and kerosene	0.26 0.33 0.21 0.40	0.32 0.31 0.27 0.29	0.33 0.35 0.16 0.25	0.30 0.33 0.21 0.31	Same as No. 1. Gauses clean. Same as No. 3. Good.
10a					No		Lard and kerosene	0.41	0.31	0.34	0.35	Good.
11	Special.....	Below flame or through base of bonnet.	1	1⁹⁄₁₆ top 1¹¹⁄₁₆ bottom ×2½	Yes	³⁄₃₂×½ F	Eureka..... Mineral Seal .... Sperm..... Lard and kerosene	0.38 0.47 0.32 0.49	0.36 0.46 0.28 0.47	0.35 0.49 0.33 0.35	0.36 0.47 0.29 0.44	Same as No. 1. Good. Same as No. 3. Good.
12	Ashworth-Hepple-white-Gray.....	Below flame through top or bottom of air tubes.	1	1 top 1½ bottom ×2¾	Chimney	¼ R	Eureka..... Mineral Seal .... Sperm..... Lard and kerosene	0.40 0.34 0.26 0.44	0.32 0.10 0.30 0.26	0.34 0.12 0.26 0.26	0.35 0.19 0.27 0.32	Same as No. 1. After 4 and 8 hr. glass very smoky, lamp had to be opened and cleaned. Top and one side of gause choked. Same as No. 3. Fair.
13	A-H-G Improved, 6 air tubes.	Below flame through top or bottom of air tubes.	1	1 top 1⁹⁄₁₆ bottom ×2⅜	Chimney	1⁄16×½ F	Eureka..... Mineral Seal .... Sperm..... Lard and kerosene	0.57 0.49 0.51 0.66	0.47 0.50 0.56 0.44	0.50 0.55 0.54 0.49	0.51 0.51 0.54 0.53	Same as No. 1. Gause clean. Same as No. 3. Good.
14	A-H-G Improved, 1 large air tube.	Below flame through top or bottom of air tubes; top of tube opens into bonnet.	2	1⁹⁄₁₆×2¼ 1⁹⁄₁₆×2	Bonnet	5⁄16×³⁄₃₂ F	Eureka..... Mineral Seal .... Sperm..... Lard and kerosene	0.43 0.52 0.52 0.53	0.36 0.44 0.46 0.48	0.40 0.47 0.45 0.47	0.40 0.48 0.48 0.49	Glass smoked. Glass smoked, gause slightly choked. Same as No. 3. Good.
15	Koehler.....	Below flame.....	2	2 & 2⁹⁄₁₆×3¹¹⁄₁₆ 1⅜ & 1¹¹⁄₁₆×3¹¹⁄₁₆	Bonnet	³⁄₃₂ R	Gasoline 72° Bé...	0.64	0.65	0.60	0.63	Gauses clean.
17	Koehler.....	Below flame.....	2	2 & 2⁹⁄₁₆×3¹¹⁄₁₆ 1⅜ & 1¹¹⁄₁₆×3¹¹⁄₁₆	Bonnet	³⁄₃₂×⅜ F	Gasoline 72° Bé..	0.66	0.64	0.66	0.65	Gauses clean.
18	Cremer.....	Below flame.....	2	1¾ & 2×3½ 1¼ & 1⁹⁄₁₆×3½	Bonnet	1⁄16×⅝ F	Gasoline 72° Bé..	0.72	0.70	0.67	0.70	Gauses clean.
19	Wolf.....	Below flame.....	2	1⁹⁄₁₆ & 1⅞×3⅝ 1⁹⁄₁₆ & 1⅝×3½	Bonnet	³⁄₃₂ R	Gasoline 72° Bé..	0.59	0.60	0.57	0.59	Gauses clean.
20	Wolf.....	Below flame.....	2	1½ & 1¾×3⅝ 1¼ & 1⅝×3⁷⁄₁₆	Bonnet	³⁄₃₂×⅝ F	Gasoline 72° Bé..	0.64	0.59	0.61	0.61	Gauses clean.

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not have a bonnet. The results on No. 12, A-H-G lamp, were checked with a duplicate lamp, the results agreeing within 5 per cent. The good lighting power of No. 13 over No. 12 is undoubtedly due to the better air supply of No. 13. As noted in Table 1, this lamp has six air tubes each with an air hole at the base, whereas No. 12 has four tubes with air inlets near the base in two only. A considerable difference will also be noticed between the observed candlepower of the Wolf type lamps and the generally accepted candlepower with a value of 1.

Table 2.—Illuminating Power of Certain Lamps According to Different Authorities

(Expressed in Candlepower)

Type	Authority								
	Marsaut	Beard	Coal & Metal Miner's Pocket Book	Mines Accident Commission	Crossley	J. A. Longden	Prussian Fire Damp Com.	Lupton, Parr & Perkins	Average
Davy (common)	0.18	0.15	0.16	0.126–0.15	0.125	0.10	.....	.....	0.141
Clanny (various types)	.	0.25–0.50	0.20	....	...	..	....	....	0.317
Clanny, unbonneted	0.47	.....	....	0.50	0.23	0.21	0.62	....	0.416
Clanny, bonneted	..	.....	....	0.31–0.64	...	0.25	....	....	0.40
Evan Thomas No. 7	..	0.40	0.45	0.46	...	..	....	....	0.437
Marsaut (2 gauze)	..	0.60	0.55	....	...	..	....	....	0.575
Ashworth-Hepplewhite-Gray	..	0.65–0.79	....	....	0.65	..	....	1.00	0.77
Beard, Eloin-Marsaut (2 gauze)	..	0.75	....	. .	...	..	....	....	0.75
Wolf	..	0.80–1.00	0.90	....	...	..	0.66	0.40	0.75

Conditions of Test

After being cleaned, filled, and lighted, the flame of each lamp was adjusted to a normal height suitable for working conditions, as near as it was possible to judge. This was ordinarily done by turning up the flame until it would begin to smoke and then slowly lowering it until it stopped smoking. The lamps were then allowed to stand 20 to 30 min. or until all parts were thoroughly warmed up, before the first readings were taken. The flames were again adjusted just before the readings were made. They were also maintained at approximately a working height throughout the time of 8 hr. and carefully adjusted at each observation.

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Illuminants

The oils used were Eureka Safety Lamp oil, Mineral Seal oil, pure sperm oil, pure lard oil, a mixture of lard oil and kerosene, and gasoline. The first two are mixed oils sold on the market under the trade names given. Eureka Safety Lamp oil is apparently a mixture of lard or cotton seed, possibly both, and a petroleum oil. Mineral Seal is a mixture of fish or sperm oil and petroleum oil with possibly other ingredients. The mixture of lard oil and kerosene was prepared for the purpose of this test and was composed of 50 per cent. lard oil and 50 per cent. kerosene by volume. Pure lard oil was tried in all the lamps, but even after they were warmed up such constant attention was required to keep them burning properly that any results obtained would have been of no practical value.

The specific gravities of the oils and the corresponding gravity on the Baumé scale are given in Table 3.

TABLE 3.—*Specific Gravity of Safety-Lamp Oils at 18° C.*

Oil	Specific Gravity, Water = 1.00	Degrees, Baumé
Eureka Safety Lamp	0.863	32
Mineral Seal	0.818	41
Sperm	0.900	25
Lard	0.913	23
Lard 50 per cent.; kerosene 50 per cent.	0.860	32
Gasoline	0.695	,72

Conclusions

The average candlepower for the different oils indicates clearly the advantage of a mixed oil or the addition of petroleum oil. The average illuminating powers of lamps Nos. 1 to 14 for the different oils, excepting gasoline, are given in Table 4.

TABLE 4.—*Average Illuminating Power of Different Oils*

Oil	Average Candlepower	Specific Gravity of Oil
50 per cent. lard, 50 per cent. kerosene	0.333	0.860
Eureka Safety Lamp	0.318	0.863
Mineral Seal	0.316	0.818
Sperm	0.296	0.900

There is apparently no definite variation of candlepower with the specific gravity of the oil, for although sperm oil is the heaviest and has the lowest average illuminating power, Mineral Seal, the lightest, is intermediate in illuminating power between sperm and the other two heavier oils.

In addition to better lighting, the mixed oils burned more freely and required less attention to the wick and flame, but showed a slightly greater inclination to smoke than the pure sperm oil. The better illu-

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minating power of the mixed oils is especially noticeable in Lamps Nos. 4, 6, 10, and 11. The effect of a bonnet in decreasing illuminating power is indicated to some extent in Lamps Nos. 10 and 10*a*, identical except for a bonnet. With the mixed lard and kerosene a difference of 0.04 candlepower is noted.

The effect of admitting the air below the flame or even at the base of the bonnet, instead of higher up, is shown conclusively in lamps Nos. 9 and 11, constructed so that air could be admitted in different ways. In No. 9, the results given in Table 1 were obtained with air entering at base of bonnet; the same lamp with air entering near the top of the bonnet and passing downward between bonnet and deflecting ring gave an average candlepower of 0.24 with Eureka oil, 0.07 less than the value given in Table 1. With No. 11 the results are even more pronounced. With the inlet at the bottom closed, and air entering at base of bonnet and gauze, the average candlepower with Eureka oil was 0.23, while with air entering below the flame it was 0.36, a difference of 0.13 or 36 per cent.

In testing the lamps with flat wicks, the candlepower was in all cases taken in a line normal to the width of the flame. The decrease in lighting edgewise to the flame was much less than anticipated. For the oil lamps the candlepower edgewise varied from 5 to 20 per cent. less with an average of 10.5 per cent. For the gasoline lamps the illumination edgewise was 4 to 7 per cent. less.

With the exception of gasoline, the decrease of candlepower during the 8 hr. seems to vary inversely as the illuminating power of the oil. Sperm oil gave an average candlepower for 14 lamps when freshly lit of 0.301 and after 8 hr. of 0.304, a gain of 1 per cent.; Eureka oil gave at the beginning an average of 0.323 cp. and after 8 hr. 0.308 cp., a decrease of 4.3 per cent.; Mineral Seal at beginning an average of 0.340 cp. and after 8 hr. 0.314 cp., a decrease of 7 per cent.; a mixture of 50 per cent. lard and 50 per cent. kerosene at the beginning gave an average of 0.403 cp. and after 8 hr. 0.296 cp., a decrease of 26.5 per cent. To determine whether the decrease in candlepower was due to the lighter part of the oil burning off first, leaving a heavier residue, the specific gravity of the oil remaining in the lamps after burning the mixture of kerosene and lard oil was determined. This was found to be 0.865 or 0.005 greater than the original oil. As this is only 0.58 per cent. gain, it is within a possible experimental error, and it would be hardly safe to assume from it that there was a separation of the oils. It would be more reasonable to conclude that the decrease in illumination was due to the more rapid deposit of soot on the gauze and a film of unburned carbon on the glass when burning oils containing petroleum products, than when burning pure sperm oil.

With the gasoline lamps, the illuminating power decreased in 8 hr. from an average of 0.65 to 0.62 cp. or 4.6 per cent.

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The Application and Earning Power of Chemistry in the Coal Mining Industry

BY EDWIN M. CHANCE,* WILKES-BARRE, PA.

(Arizona Meeting, September, 1916)

DURING the last decade many conditions have been encountered that have materially increased the cost of the production of coal. As in most cases it has not been practicable to increase the selling price of such coal sufficiently to insure the necessary margin of profit, the mine management has been forced to avail itself of what might be termed unusual means that costs might be held at as low a figure as possible. The so-called efficiency engineer has been called upon, costly power plants have been built and machinery has been installed to replace manual labor wherever possible.

Another aid used recently to secure economies through the more intelligent production of coal, is the chemist. As the coal industry, barring the production of byproduct coke, does not at once give evidence of its need of the services of the chemist, the writer will endeavor to point out a few of the services that this branch of the staff has been able to render.

The preparation of coal in the past has been carried on entirely upon an empirical basis, that is, the criterion adopted has been an ocular inspection. Unfortunately, the appearance of a coal and especially of an anthracite, has but little relation to its fuel value, and as long as coal is purchased for the heat that can be obtained from it by its combustion, the quantity of such heat that is purchased must be one of the principal desiderata in selecting or preparing a coal. Were coals purchased for use as bric-a-brac or for exhibition in museums it might be both wise and just to purchase them upon their appearance. After 4 or 5 years of investigation the fact has been established that coals of very inferior appearance, and I now have special reference to anthracite, might and often did possess heating and burning properties superior to those possessed by bright coals which in the past had commanded considerable and uniform premiums in the market. This being the case the question arose as to whether the trade could be convinced of this fact. While at first some difficulty was experienced in establishing the quality of such dull coals, still it was found that the real obstacle to the sale of these materials in the past had been the fact that the coal producers' own sales departments felt convinced of the inferiority of this type of fuel and when complaints were registered they were given ready credence. Upon convincing the sales department of the quality of this type of fuel, but

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little difficulty was experienced in securing a ready market for it and it was even found possible to secure premiums for certain of these dull coals. It is readily understandable that the only available means whereby a true appreciation of the quality of a fuel can be arrived at is by its chemical analysis or by a service test. The cost and unreliability of service tests have led to their general abandonment, hence the chemical examination of a fuel has been found to be perhaps the most ready and practicable method for determining its real position in the scale of usable fuels. Now when we consider that in the past hundreds of thousands of tons of dull and unpromising looking coal have been gobbed at a positive cost to the coal producer, with the ever present danger of gob fires, that many other hundreds of thousands of tons of this material have been sent to the rock bank as refuse, and that because of the similarity of the specific gravities of the dull and bright coals, the difficulties and inefficiencies of coal preparation have been enormously increased by the attempted removal of this material, it will be seen how enormously profitable the utilization of this excellent fuel has been. In order to give an adequate idea of the relative worth of the dull and bright material from the same veins, I will quote the following average analyses:

Size	Test Number	Color of Ash	Moisture, Per Cent.	Volatile Matter, Per Cent.	Fixed Carbon, Per Cent.	Ash, Per Cent.	Sulphur, Per Cent.	Heating Value per Pound Coal as Received, B.t.u.	Heating Value per Pound Dry Coal in B.t.u.
Dull coal.....	C-502	Red	2.98	6.39	82.24	8.39	0.67	13,110	13,510
Bright coal...	C-503	Red	3.70	5.67	81.16	9.47	0.71	12,890	13,380

These analyses are in both cases the average of a number of analyses of characteristic material of both types, and are in no sense selected. In the past this dull material, test No. C-502, has been condemned as bone. Thus by a systematic chemical investigation of the various steps in the preparation of coal undiscovered inefficiencies are revealed and their correction is made possible.

In the efficient operation of boiler plants it is necessary to secure as soft and non-corrosive a boiler water as possible. While the most desirable method is to secure a supply of water that is naturally soft, still in the mining industry it is frequently necessary to use water that is both incrusting and corrosive. To meet these conditions a host of so-called boiler compounds has been foisted upon the unsuspecting mine manager. While certain of these compounds are worthless or of little value, others have considerable merit in special cases. Unfortunately, however, their use is indiscriminately recommended by the vendor, and a boiler compound that might prove efficient with waters of a certain type will prove of little value under different conditions. Moreover, the cost of these compounds is out of all proportion to their merit and a grave and positive

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risk is run of doing the boilers infinite damage by the indiscriminate use of such nostrums. I recall at this moment the case of one new boiler house equipment of several thousand horsepower capacity that was so damaged by 3 months' use of a highly recommended water softener, even though the water in this case was not very hard, that it required over a year and a half of patient and costly effort to put the plant back in a condition having the semblance of efficiency. Now a competent chemist can readily recommend a treatment for such boiler feed waters that will meet the requirements of the particular case in question both cheaply and efficiently.

By reason of inadequate storage facilities at many mines, a prolonged drought will occasionally require the use of mine water as boiler feed. The writer recalls one occasion when a large number of collieries of one company were operated with such feed water, containing in many cases as much as 10,000 parts per million of free sulphuric acid and 35,000 to 40,000 parts per million total acidity. These plants were operated successfully over a period of months with such feed water, though of course many difficulties arose and much care was required, while the surrounding coal-mining companies, not possessing expert chemical assistance, were in many cases required to cease operation.

The subject of lubrication is one that has in the past been favored by the so-called efficiency expert. It is, therefore, with a sense of some diffidence that the writer approaches this subject. As conditions governing colliery lubrication are so very different from those governing the lubrication of the mill or industrial plant, it has been found that the lubrication secured by the use of lubricants recommended by the vendor has in the past been unsatisfactory and somewhat costly. Some years ago the writer conducted numerous field and laboratory tests in order to establish just which lubricants would best meet coal mine conditions. After these points had been well established an effort was made to go into the open market and, by specification, purchase the material desired. While this plan was successful, still it was found that almost continual testing of the material supplied was required, as the bidders purchased their materials in the open market and hence there was little or no uniformity of supply. Those bidders not purchasing oils in this way were, in many cases, refiners handling such a diversity of crudes that practically the same complaints arose. It was found that the independent refiners of Pennsylvania petroleum produced the lubricants required, and since the materials were produced by them and their range of production was rather limited, little difficulty was encountered in securing uniform products from them. Moreover, by concentrating the purchases for a number of coal producers upon one refinery, it has been possible to secure most advantageous prices while at the same time the quality of materials supplied is directed by the purchaser and not by the vendor. In other words, the oil to meet any special condition is purchased from the refiner

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without consulting him as to its use. Hence there is little or no probability of his attempting to substitute a material that he may consider almost as suitable, as he has no knowledge of the use to which the material in question is to be put. It is an axiom that given a maximum price that a purchaser will pay, the average vendor will endeavor to supply the material that costs him the least but that will meet the condition in question. In purchasing lubricants as indicated above this condition does not arise, as the oil for the purpose in question is selected without considering its price and further, since the vendor is not consulted in the matter, the best oil that can possibly be obtained for the particular purpose is secured. It may seem that the cost of such a procedure would be prohibitive, and this would be the case were these materials purchased at the scale of prices that usually obtains. By means of the coöperation mentioned above, however, prices have been secured that are so advantageous that the highest grade of product that it is possible to secure, irrespective of price, is generally purchased at a lower price than the product of mediocre quality that was formerly used. In this way the actual first cost of lubrication has been reduced from 30 to 50 per cent., while the actual efficiency of the lubrication secured, though less readily determinable, has been greatly increased.

The chemist has been found to be almost indispensable where a mine fire is being extinguished by cutting off its air supply, for the chemical analysis of the air from the fire zone makes it possible to form an intelligent opinion of the progress of the extinction and the air-tightness of the dams or fire walls built to control the fire.

Many more instances than those briefly touched upon might be quoted and those that have been quoted might be developed in greater detail. Such a dissertation, however, would be burdensome and would fall without the purpose of these notes.

Discussion

Edward H. Coxe, Knoxville, Tenn. (communication to the Secretary*).—Mr. Chance has omitted to mention one very important use for the chemist in connection with the preparation of coal, and that is in connection with coal washing.

Efficient coal washing, and especially of the finer sizes, can be accomplished only under the supervision of a chemist, as the proper regulation of the washer can only be had by frequent sampling and analysis of the raw and washed product and the refuse, say at least semi-weekly; and only in this way can a check be kept on the quality of the product and the amount of good coal wasted in the refuse.

In large operations, frequently enough coal can be saved from going to the refuse pile to more than pay the salary of the chemist.

* Received Apr. 10, 1916.

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The Function of Alumina in Slags

Discussion of the paper of CARL HENRICH (p. 621).

J. E. JOHNSON, JR., New York, N. Y. (communication to the Secretary*).—The discussion of Mr. Henrich's paper took place at a meeting from which I had the misfortune to be absent, and has just come to my attention. I do not wish to let the subject pass without contributing to it a little information which has come into my possession.

In 1912, I published a brief note in the *Transactions* on The Effect of Alumina in Blast-Furnace Slags, giving in tabular form three typical slags used in iron blast-furnace practice, one of them the high alumina slag of which Professor Richards speaks. That table is reproduced herewith.

TABLE 1.—*Composition of Slags*

	1 Al_2O_3, Per Cent.	2 SiO_2, Per Cent.	3 CaO by Difference, Per Cent.	4 Neutral Substances, Per Cent.	5 Ratio $\frac{CaO}{Al_2O_3 + SiO_2}$	6 Ratio $\frac{CaO + Al_2O_3}{SiO_2}$	7 Ratio $\frac{CaO}{SiO_2}$
Virginia practice.....	6.5	36.0	54.0	3.5	1.27	1.68	1.50
Lake-ore practice.....	13.5	33.0	50.0	3.5	1.08	1.92	1.51
Special experiment...	36.0	24.7	36.8	3.5	0.59	2.90	1.44

It supplies what seems to me convincing evidence that, for the purposes of the iron blast furnace, alumina can be regarded only as a neutral substance, dissolving in the slag and materially altering its viscosity, but not affecting its chemical activity.

In subsequent experience with charcoal furnaces, I found the same rule to hold; that the chemical effect of alumina could be disregarded, providing the ratio of the lime and magnesia to the silica were kept constant. This is exactly the experience which Messrs. Dwight and Mathewson had in copper- and lead-smelting furnaces.

I have never seen any evidence or argument adequate to support the contention of Professor Richards—that the alumina acted as a base in the charcoal iron furnace and as an acid in the coke furnace, and that the function of alumina in iron-furnace slags is inherently different from that in copper- and lead-furnace slags—and I am compelled to differ absolutely with his conclusions in that respect.

* Received June 27, 1917.

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I am not familiar with lead and copper furnaces, but certainly the experiences of Messrs. Dwight and Mathewson, and to a great extent the data of Mr. Henrich himself, might be taken as the description of my own experience with iron furnaces.

The rôle of alumina appears to be a purely physical one, but is highly important in that respect since it alters essentially the viscosity of the slag. Moreover, its effect in this respect is not constant in iron-furnace slags. The viscosity of the slag rises as the alumina rises, from 5 or 6 per cent. to about 20 per cent., and the viscosity falls from 23 or 24 per cent. to 36 per cent. (which is as far as my information goes). The high-alumina slags are distinctly more fusible than those in the range from 20 to 25 per cent., and about as fluid as those around 12 per cent. Al_2O_3, which are in common use.

The clever device of Mr. Feild, described in his paper at the same meeting, furnishes us the means of really measuring accurately viscosity at varying temperatures, and by the use of his method, I hope to see in a few years the solution of the slag problems which have bedevilled so many metallurgists.

E. P. MATHEWSON, Toronto, Canada (communication to the Secretary*).—Mr. Henrich asks for analyses of certain slags (p. 631), and I am endeavoring to obtain these analyses. In the meantime, I have just received from S. J. Gormly, of the Fundicion de Guayacan at Guayacan, Chile, the following: "The average slag run here has the following composition: Silica, 37.5 per cent.; iron oxide (FeO), 31.0 per cent.; alumina, 15 per cent.; lime (CaO), 12 per cent.; copper, 0.25 per cent. This is the highest alumina slag I had had any experience with and was somewhat worried at the very stringy consistency until I found that it was so clean and that it was very free running regardless of how it looked." My recollection of the old Pueblo slag is that in composition it was similar to the above, but a little lower in silica content.

E. P. MATHEWSON, Toronto, Canada (communication to the Secretary†).—With further reference to the paper on the Function of Alumina in Slags, I submit herewith copy of a letter from George A. Marsh, Superintendent of the Pueblo plant of the American Smelting & Refining Co., giving a number of analyses of high-alumina slags, the slags to which I referred in my former communication. Mr. Marsh says:

"We are enclosing a few analyses of slags, high in alumina, taken from the chemists' record book.

"We ran across the blast-furnace record covering part of the time these slags were made, so we have included tonnage on some of the dates. The tonnage includes ore and limestone, when any was used; but does not include coke or slag. The ton-

* Received July 20, 1917.
† Received Aug. 17, 1917.

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nage while running this charge compares very favorably with that made both before and afterward."

High Alumina Slags

	Matte				Slag						
	Pb	Cu	MgO	Zn	SiO_2	Fe	Mn	CaO	$BaSO_4$	Al_2O_3	
1/7–1895	8.0	6.3	.....	...	32.6	23.1	3.0	11.1	6.0	15.8	
1/8	8.4	5.8	.....	...	32.7	22.2	2.9	11.5	4.8	14.6	
1/9	10.4	6.1	.....	...	32.2	27.7	2.7	10.0	4.0	10.4	
1/10	7.6	5.4	.....	...	32.5	26.0	2.0	6.7	6.8	12.9	
1/14	8.6	4.6	.....	...	33.3	24.6	1.8	6.6	6.7	13.1	
1/15	8.5	5.2	.....	...	33.1	23.5	2.4	9.9	5.9	13.6	
1/16	6.6	6.8	.....	...	31.8	25.6	1.8	11.7	5.2	13.2	
2/1–1895	8.2	4.7	.....	3.0	30.4	22.0	1.8	16.6	5.6	11.8	
3/13	.....	.....	.....	...	32.6	18.9	4.0	13.0	7.6	10.8	
3/14	8.8	11.4	.....	...	32.8	19.0	4.2	13.5	6.9	13.6	
3/15	8.0	10.5	3.1	...	32.2	18.7	4.8	14.0	7.1	11.6	
3/17	6.4	9.0	.....	...	29.8	23.7	4.2	13.5	6.2	10.6	
3/21	4.0	5.4	.....	...	29.4	24.6	2.4	15.0	3.9	12.0	
4/3–1895	14.4	12.3	.....	...	31.2	22.6	2.2	14.7	6.2	12.2	
4/10	6.0	8.0	.....	...	30.7	22.9	3.1	16.5	5.6	10.2	
7/1–1895 92 tons Fce day	.....	.....	.....	...	31.2	18.5	6.3	13.5	11.8	10.2	8
7/2 116 tons Fce day	.....	.....	.....	3.0	35.8	16.3	1.1	5.0	0.0	10.8	5.4
7/3 115 tons Fce day	.....	.....	.....	...	28.8	20.0	7.2	13.7	12.6	10.4	
7/5 105 tons Fce day	.....	.....	.....	...	31.4	22.3	4.5	15.0	6.8	9.0	
7/16 95 tons Fce day	.....	.....	.....	...	32.6	21.3	1.2	11.0	7.8	10.1	
7/24 111 tons Fce day	.....	.....	.....	...	31.4	22.5	3.3	13.4	8.6	11.7	
8/17 100 tons Fce day	4.6	11.6	10.0	...	29.3	17.3	8.4	13.7	12.1	10.6	
8/20 118 tons: no limestone.	6.9	16.0	10.0	...	32.6	18.3	10.5	10.4	.....	9.3	
8/21 106 tons: no limestone.	6.2	7.5	13.8	...	31.3	20.4	9.3	9.8	5.8	8.4	
8/22 106 tons: no limestone.	9.0	8.5	.....	...	32.2	17.3	13.2	9.6	10.7	9.0	
8/23 108 tons: no limestone.	7.0	10.0	3.3	...	31.5	18.0	11.4	10.2	8.7	11.0	
8/26 108 tons: no limestone.	10.8	10.5	.....	...	31.6	17.8	11.4	9.4	9.2	9.7	
Average	.....	.....	.....	...	31.7	21.1	5.15	11.7	7.15	11.3	

CARL HENRICH (communication to the Secretary*).—I desire to express to Mr. Mathewson my appreciation of the interest taken in the subject of my paper on The Function of Alumina in Slags, on which, of course, high-alumina slags are bound to give most information.

In regard to the above cited statement of S. J. Gormly: The average composition of the slag made at the Fundicion de Guayacan is certainly interesting, as is also its "very stringy consistency." The information would be still more valuable if it were supplemented by the dimensions of the blast furnace, area at tuyeres, height of charge above the tuyeres, blast pressure, percentage of fuel, whether pyritic, semi-pyritic, or straight smelting, whether the FeO given is present as FeO only, or whether some MnO is included in the 31 per cent., also whether the 12 per cent. lime given is solely CaO, or whether some MgO and BaO has been included.

It is not so much the average composition of the slag that is the main interest, but the variations from this average composition, as they are

* Received Sept. 4, 1917.

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bound to occur in practice, and the influence which these variations have on the viscosity (stringy consistency) of the slag, formation of noses and scaffolds at and above the tuyeres.

Consideration of the oxygen ratios:

$SiO_2 = 37.5$	$O = 20.0$
$FeO = 31.0$	$O = 6.9$
$CaO = 12.0$	$O = 3.4$
$Al_2O_3 = 15.0$	$O = 7.0$

Al_2O_3 acid : O ratio = 27.0:10.3 = 2.6

Al_2O_3 basic : O ratio = 20.0:17.3 = 1.26.

It looks to me, as if in the making of this slag they were "skating on thin ice" at Guayacan. The influence of considerable variations in the composition of this average slag, whenever they occur, would be decidedly interesting.

The Pueblo Smelter records, in the form transmitted to me, are, in some cases, rather startling on closer inspection. I find in these records five cases in which the composition of the matte is charged with from 3 to 14 per cent. MgO, and two cases in which the matte composition is charged with 3 per cent. Zn, while the slag analyses show neither MgO nor ZnO in any one of the 27 cases. Of course, I assume this as a clerical error and transfer the MgO from the matte column to the slag column. The FeO and MnO of the slag are given as Fe and Mn. I substitute the equivalent FeO and MnO percentages. BaO in the slag is given as $BaSO_4$, as which no doubt BaO was determined in the analysis, but as which it can hardly be assumed to have been contained in the slag. I substitute the equivalent BaO percentage.

We will consider some of the highest alumina slags, for which no MgO determination is given—*i.e.*, slags made in 1/7/95, 1/8/95, 1/14/95, 1/15/95, 3/14/95—and some of the slags with a higher and lower percentage of MgO, and alumina percentages between 8 and 11 per cent.—*i.e.*, slags made 8/17/95, 8/21/95, and 8/23/95. We have:

Date		SiO_2	Fe(Mn)O	CaO	BaO	MgO	Al_2O_3	Total Per Cent.
1/7/95	a.—	32.6	33.6	11.1	4.0	.....	15.8	97.1
1/8/95	b.—	32.7	32.3	11.5	3.2	.....	14.6	94.3
1/14/95	c.—	33.3	34.0	6.6	4.4	.....	13.1	91.4
1/15/95	d.—	33.1	33.3	9.9	3.9	.....	13.6	93.8
3/14/95	e.—	32.8	29.8	13.5	4.6	.....	13.6	94.3
8/17/95	f.—	29.3	33.0	13.6	8.0	10.0	10.6	104.6
8/21/95	g.—	31.3	38.2	9.8	3.8	13.8	8.4	105.3
8/23/95	h.—	31.5	37.8	10.2	5.8	3.3	11.0	99.6

In looking over these analyses, we notice the low percentage totals

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of "b," "c," "d" and "e," and the high totals of "f" and "g." However, with these doubts in mind, we have to take the figures given for what they may be worth. Smelter chemists are not expected, and cannot be expected, with the "hurry" results demanded, to work on scientifically accurate lines.

The oxygen ratios of these slags, on the figures given, would be:

	a	b	c	d	e	f	g	h
Al_2O_3 acid	2.21	2.25	2.4	2.26	2.2	1.29	1.20	1.67
Al_2O_3 basic	0.94	1.0	1.15	1.05	1.0	0.74	0.8	0.91

The ratio of Fe(Mn)O to Ca(Ba,Mg)O, the latter as unit, would be: a.—2.1; b.—2.1; c.—3.3; d.—2.3; e.—1.5; f.—0.84; g.—1; h.—1.67.

The ratios of SiO_2 and Al_2O_3 would be:

a.—7:2; b.—15:4; c.—13:3; d.—33:8; e.—4:1; f.—14:3; g.—25:4; h.—24:5.

An approximate formula for slag "g" would be, for example:

$$(25FeO + 25CaO) \times (24SiO_2 + 4Al_2O_3),$$

which, although a little basic, is a well proportioned slag composition.

On the basis of the views set forth in my paper, slags "f," "g," and "h" should be the best proportioned, freest running and cleanest slags of the lot. Whether the Pueblo furnace records will, or would confirm this view, I don't know. The data furnished by Geo. A. Marsh in his letter to E. P. Mathewson seem to point in that direction. Slags "f," "g," and "h" were evidently among the freest running slags of the high-alumina slags made in 1895 at Pueblo, and ought to have been according to recommendations for the composition of high-alumina slags contained in my paper.

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